13

Sir George Biddell Airy

... from a Photograph

... Macmillan & Co

NATURE

A WEEKLY

ILLUSTRATED JOURNAL OF SCIENCE

VOLUME XVIII.

MAY 1878 to OCTOBER 1878

"*To the solid ground*
Of Nature trusts the mind which builds for aye."—Wordsworth

London and New York:
MACMILLAN AND CO.
1878

LONDON:
R. CLAY, SONS, AND TAYLOR, PRINTERS,
BREAD STREET HILL, QUEEN VICTORIA STREET

INDEX

M

M

NATURE

A WEEKLY ILLUSTRATED JOURNAL OF SCIENCE

"To the solid ground
Of Nature trusts the mind which builds for aye."—WORDSWORTH

THURSDAY, MAY 2, 1878

RETROSPECT AND PROSPECT

IN beginning with the present Number what is practically a New Series of NATURE, the journal has reached a definite stage of its career, one at which it will perhaps not be considered out of place that we should make a reference both to its past and future.

That NATURE has succeeded in attaining the object for which it was started will, we think, be admitted by men of science both at home and abroad; and it is because NATURE has become more and more widely recognised as the organ of science all the world over, that at last we are compelled to enlarge it in order to find space for the stream of communications that week after week come pouring in upon us from all parts. If we have, either directly or indirectly, contributed to this spread of a taste for scientific knowledge, this is due to the untiring assistance and sympathy which the many students and friends of science in this and other countries have afforded us, and which aid we are anxious to take this opportunity of acknowledging. In the future, as in the past, we are sure this stream will continue to flow, so that the editor's function will, as heretofore, be a modest one.

Beyond this we need not dwell on the past and future of the journal. The work accomplished in the past nine years, and the direction in which it has progressed, is a much more important matter. Both writers and readers have, as it were, arrived at a stage of their journey. From it the countries which they have explored are still plainly visible, while those in front of them, never before trodden, even if they show fair promise of being similar to those already traversed, are shrouded in mystery, and may in truth turn out to be very different from what they seem.

We may take advantage, then, of this halt to glance at the nine years' crop of knowledge which NATURE has recorded; to consider the scientific progress accomplished in that period, and to seek for the indications afforded as to the lines along which activity may be expected in the future.

We certainly do live in deeply interesting times. Since the first number of this journal appeared there has undoubtedly sprung up a much greater general interest in science than was formerly to be found, and questions concerning scientific discovery, research and teaching, have now a much more direct interest to the public than they formerly possessed. No better test of this can be required than the continually growing space given to such matters by the daily press, and the more intelligent discussion of such questions as the endowment of research, and the importance of science at our universities and in our schools which is everywhere noticeable.

The matters to which we have just referred are, indeed, those in which a distinct progress has been made—a progress which we firmly believe is only a small foretaste of that which is to follow.

We have, ever since the journal was started, maintained, through evil report and good report, the crying necessity to the country of a greater endowment and a wider diffusion of pure science, because the one provides us with raw material, the other distributes it, so that throughout the length and breadth of the land, new manufactures in the shape of new applications of science may arise. There are many signs which indicate that the necessity of this, which is obvious at the present time only to the comparatively few, will be universally insisted upon. If it is not, future historians may have to show how different might have been our condition if our mental resources, which are doubtless as rich as our material ones, had been utilised in the same way; if education had done for mind, what, for instance, coal has been compelled to do for iron.

The Government has not been slow to recognise this growing interest in scientific matters. In the period to which we refer a commission, of which the Duke of Devonshire was chairman, and of which the two secretaries of the Royal Society were among the members, has gone over, and reported at length on, the whole field of scientific instruction and the advancement of science in the three kingdoms.

The only direct response made up to the present time by the Government to the recommendations of the Duke of Devonshire's Commission, has been the

endowment of scientific workers. The sum of 4,000*l.* placed at the disposal of a committee of the Royal Society for this purpose, was doubtless a large sum to begin with, and the committee has made a good beginning with it. The question was a delicate and a difficult one, and allowances must be made, but we doubt whether the intention of the Government will be fulfilled by increasing the stipends of professors who are already hard worked at institutions, the managers of which will now have a most excellent excuse for underpaying them; or by cutting down the personal grants to investigators to provide small sums for apparatus, which the fund was not primarily intended to meet.

Another recommendation of that Commission, namely, the formation of a museum of physical apparatus, complementary to the new museum of Natural History now being erected in South Kensington, has been more than half carried out. A loan Collection of Scientific Apparatus was formed as an experiment, and all, or almost all, have recognised the importance of a permanent museum of this nature.

The Duke of Devonshire's commission has given rise to three others, which have reported on the Universities of England and Scotland; and there is little doubt that out of these labours much good will come, though very likely it will be long in the coming. We say this because such ancient corporations as universities are the products of so many conditions that a mere determination to effect organic reforms, without minute examination, may do much more harm than good. The great point now is that the weak points of our university system are no longer within the ken merely of the few. There are hundreds of thousands of people now in the country who can contrast that getting of knowledge for knowledge sake, which is the glory of the German system, with that utterly demoralising cramming of things that pay in an examination, which is the disgrace of our own. Enough also is known of the activity of the laboratories and scientific workshops in foreign universities to create astonishment at the masterly inactivity in the matter of original work displayed in some of our own. To recognise such defects as these is half to rectify them.

There is now a prospect of science taking its place in our schools alongside of those other branches which until quite recently held exclusive sway in them; and it is probable that in the course of a few years, British schools will have reached the stage attained by German ones a quarter of a century ago.

The foundation of Colleges at Newcastle, Leeds, and Bristol, is among the signs of this increased activity in educational matters, while side by side with these new foundations, Owens College has now arrived at such a pitch of completeness and usefulness that its erection into a university cannot be long delayed. France, as well as ourselves, is now perceiving the advantages of the German system, and before long we may expect to find separate faculties abolished in that country, and the erection of many new universities. That at Lyons is almost already freed from the leading strings of Paris, and others will soon follow.

The Government has been largely influenced in another way—not only, indeed, our Government, but the whole civilised world. Never before in an equal period have so many expeditions been organised to distant parts of the world, to bring back rich fruits of pure knowledge of one kind or another. The voyage of the *Challenger* will for ever mark this century in the history of science, and our country must be congratulated at having so largely helped in the accumulation of the vast stores of new knowledge which that and other similar expeditions have gained for the use of all.

Animal forms new and strange, the secrets of the deepest depth of ocean, the coursing of the blood along the arteries of the world, the building up of a large portion of the planet itself; such are the topics which we shall hear much of during the next years, as volume after volume of these precious records makes its appearance.

It is not the fault of the men of science if the Polar expedition, which our Government, in friendly rivalry with America, Austria, Germany, Sweden, and other countries sent out, will not live so long in story as will the voyage of the *Challenger*. It is, after all, but a grim consolation that the elaborate instructions drawn up, and the valuable series of facts collected for the use of this expedition, will, beyond question, be found useful in a not distant future. The lesson we have learnt, however, goes to strengthen the view so definitely expressed by the German committee, that a mere dash to the Pole is precisely the thing that is *not* wanted. Careful scientific work, chiefly of the physical sort, carried on for a long period of time, and necessarily, therefore, by relays of observers, would in all probability furnish us with a large array of facts, which could at once be applied to the solution of many outstanding questions in various branches of terrestrial physics. The way of the winds alone, in these weirdest regions of the world, in itself presents a problem which may be of the highest importance.

We hope that the enormous sums which have been spent on the observations of the transit of Venus of 1874, will be amply justified by the result which will be obtained when all the observations of Europe and America have been discussed. It remains to be seen whether, when this has been accomplished, the transit of 1882 will have so great a charm for the astronomers as its precursor of 1874. In any case if so much money is to be spent on the determination of a numerical value which can be arrived at by physical means, it is but fair that the interesting physical phenomena which occur during a transit should not be neglected to so great an extent as they were on the former occasion. Those who control expeditions of this kind incur a grave responsibility if any avenue to knowledge is barred by them.

Our first number contained an account of an eclipse of the sun observed in 1869 in America. We commence our eighteenth volume while again in that country astronomers are vying with each other in perfecting their methods to observe the eclipse of next July. Since 1869 our Government has aided three eclipse expeditions: one to the Mediterranean in 1870, one to India in 1871, and one to Siam in 1875. A precedent has therefore been established by which active workers may profit in the coming time, for every solar eclipse and every piece of work at the uneclipsed sun raises questions—and will doubtless for ever go on raising them—which eclipses alone can settle.

Another mark of the recognition of science accorded by our Government has been the increase of the sum placed at the disposal of the meteorological Council, while at the same time a reorganisation of the administration of the Council has taken place. Perhaps the most startling event which has taken place during the last eight years touching the administration of scientific work in England, has been the nomination of the members of this Council by the Royal Society. Among these we do not find one of our distinguished meteorologists. We fear, therefore, that most of the meteorological work of the future will lie outside that body, a result which all must intensely regret, for there is enough loyalty to the Royal Society among British men of science to make them wish that everything it touches should succeed.

Never before have the larger questions of meteorology attracted so many minds: the connection of solar changes with terrestrial changes and everything which depends upon them, is now beginning to loom out of the mists of obscurity in a most gigantic shape. The subject is one of such intense interest to humanity, that here there must be no hasty work. The magnetician, the meteorologist, and the physicist, must march together with cautious tread, and when they do we shall doubtless in the next few years find the basis surer and surer, and the methods employed freer from that mutual criticism which makes outsiders think that meteorological butter depends more upon the churn than upon the milk. In any case we may congratulate ourselves that the next eight years will in all human probability give us a more unbroken chain of solar facts than that secured during the last similar period of time.

It is not only in the larger problems of physical meteorology that progress is being made. Nine years ago found us warning Deal from Valentia. We now, thanks to the public spirit of Mr. Bennett, of the *New York Herald*, warn Valentia from New York. The laws of the passage of storms over the Atlantic will soon, doubtless, be more within our grasp, and the next decade may enable us to watch the travels of a cyclone over a distance equal to half the circumference of the earth.

The Government explorations carried on by the *Challenger* and other expeditions, to which we have already referred, have not been the only ones which made the last decade a very remarkable one.

From the time of Marco Polo nothing more wonderful in the way of foreign travel than the stupendous feats accomplished by Cameron and Stanley in Central Africa have been placed on record. In the near future, and perhaps even in a more distant one, we are not likely to have to chronicle anything coming up to the level of the work accomplished by these men. Nor must we omit to mention Warburton, Giles, and Forrest, whose ride across the Australian desert was scarcely less remarkable. Exploration will doubtless still go on, but by the nature of the problem, it must be exploration of the less sensational sort, but by no means the less useful on that account.

Although these African and Australian adventures have been the greatest achievements of their kind which we have had to chronicle, there is scarcely any part of the world on which the explorer's activity has not recently left his mark. In fact, between Cameron, who makes a dash across a continent, and the much-to-be-pitied members of our own geological survey, who, according to a recent parliamentary paper, spend a year in mapping a region of twelve square miles, there is an unbroken series of workers, thanks to whose labours, *per mare per terras*, a complete inventory of our planetary riches is being got together.

Coming from exploration to the sciences of observation and experiment, when we have referred to the enormous increase in telescopic power on the one hand, and to the gradual consolidation and new grouping of facts on the other, we have, perhaps, referred to the most salient points. The increase of observing power since 1869, as is best evidenced by the discovery of two satellites of Mars, is simply stupendous; in that year we chronicled the erection of the Ferndene telescope; since then this telescope has not only been eclipsed actually, but in imagination dwarfed into such dimensions that it may serve as a finder to the telescope of the future. Henceforward the attempts of those who experiment on 10-feet mirrors will be the central point of interest.

The development which our knowledge of the motion of the intimate particles of matter has received during the last ten years from the work done on the kinetic theory of gases and in the exploration of spectroscopic phenomena, is greater than we have as yet any idea of. It will not be a surprising thing if, before very long, these two streams of work find their meeting-place as do the Rhône and the Saône at Lyons, when the clear formulæ of the kinetic theory, commingling with the already multitudinous but far from organised spectroscopic observations, shall form a noble river, the molecular science which in the coming time will embrace all others.

Another grouping, of which we have recorded the gradual consolidation, and which is destined to change the points of view from which many Aspects of Nature have been regarded, is that of physiography—a name which conveniently defines the region where the physicist, the chemist, the geologist, and the astronomer, find each a common interest. Such a grouping as this would, of course, be impossible in a planet where the chemistry of extraneous matter, or the origin of its own, presented problems beyond the range of investigation. But precisely because such points as these are continually receiving important developments here, this new grouping is destined to form a centre of an ever-widening interest —an interest from which all must gain, for so surely as all Nature is one, so must the work of each explorer, to be of its greatest value, form only a part of one combined attack.

The radiometer and the telephone are products of recent investigations in physics which of themselves are fit to mark an epoch, but first-rate work has been done besides, by which the continuity of the gaseous and liquid states of matter has been demonstrated, and the last gas reduced to a liquid form. The revival of the contact theory affords a new standpoint for electricians, while the ever-increasing analogies between light and magnetism which are being brought forward indicate that before long some vast generalisation may be expected in this direction. On all sides the interest attaching to physical problems is increasing, and it is well that this

is so, because the chemists among us are for the most part silent, and chemical theory is almost dead in England; indeed it would appear as if the centre of gravity of this science had gone bodily eastward, and Berlin and St. Petersburg now replace London and Paris so far at all events as organic chemistry is concerned.

But if the chemist has ceased to employ physical tools this is made up for by those large fields of physical work which are being more and more utilised by the physiologist. The introduction of physical methods into biological research is one which has already borne, and which will in the future bear, rich fruit, and all work of one kind in this direction will be as largely modified in the future by the introduction of physical methods as that of another will be rendered practically a new science by the generalisations of the immortal Darwin.

All experimental science will gain by this, for each branch of scientific work reacts upon all others, and while in the future a physiologist who simply knows how to use a microscope and a dissecting knife will be an impossibility, physics, on the other hand, will be sure to receive new methods of observation and new instruments from those who have been compelled to invent them for their new needs.

We have recorded the completion of, perhaps, the greatest work ever undertaken and carried to a conclusion by any one man. We allude to the planetary tables, the final touches of which were added by Leverrier only a few hours before a death which has left a void in science which it may take centuries to fill.

The physical side of geology has attracted much attention during the last nine years, and it has been our privilege to chronicle many investigations dealing with the interior structure and heat and the probable age of our planet. The facts collected by our surveyors with an activity which, especially in America, has been something beyond all parallel, thus find themselves supplemented by theoretical views, the fitting together of which, in the future, will be a work which will be second to none in interest.

Of practical applications of science made since 1869 the number is legion, and some are of high order. The advance in navigation, perhaps, is the most striking. We have not only in the way of new instruments the bathometer, a machine for taking flying soundings, and a perfect compass, but also the whole art of navigation promises to be revolutionised by the introduction of new methods. One thing which all friends of science should take to heart, has been abundantly established, the science most applied is the science of which the theory is bound to receive the greatest development. The telephone, duplex telegraphy, steam fog-signals, and the application of electricity to lighting, must also be mentioned.

From the prosecution of science itself we must turn to some of its surrounding conditions. We have had to watch, and have recorded with pleasure, the establishment of several new societies, and the strengthening of old ones since our first number was issued. Mathematicians have now a strong society; physical science is now represented in this way by the side of chemistry; while the latest born of these societies, though by no means the least active, is that devoted to mineralogy. We do not suppose the coming time will see a very large increase in the number of these bodies, but we think that it certainly will see a considerable influence of them all upon the Royal Society. It would be a loss universally deplored if the Royal Society were to abate one jot or tittle of its influence, but with active societies all round it representing each branch of inquiry and at once discussing each advance of knowledge in full meetings, it is difficult to understand that the Royal Society may not suffer if some better method than the one at present adopted of providing for the reading of the multitude of papers presented to it is not adopted.

From our own English societies we once more come to individuals, and here our task is a sad one. It is almost impossible to name a period of nine years during which death has played such havoc among men of science of all nationalities. Herschel, Graham, Wheatstone, Sedgwick, Lyell, and Murchison are no more; Leverrier, the great Leverrier, has gone with Regnault, Milne-Edwards, Claude Bernard, Becquerel, and many other Frenchmen of note. America has lost Agassiz; Germany, Liebig, Argelander, Erdmann, Mayer, and Heis; Russia, von Baer and Mädler; Italy, Secchi; while in all countries the thinning of the ranks of men of lesser note has been disastrous. We may surely hope that in our new series the sad task of bidding farewell to men who have done their work in science may fall less frequently upon us.

EDITOR

THE AMERICAN STORM WARNINGS

THE interest excited in Europe, and particularly in England and France, by the weather predictions cabled by the *New York Herald* to its London Office during the past year (commencing February 14, 1877) proves that these warning messages are regarded as important to the interests of commerce, navigation, and agriculture. The generally expressed opinion as to their accuracy is a favourable one, and is justified, I believe, by the fulfilment of a very large percentage. Such a result of the first year's work affords me unqualified satisfaction. It represents all the success I aimed to attain, and much more than I hoped to win.

I will state at the outset that the carrying out of the whole project of warning the European coasts of the approach of storms has depended on, and has been sustained by, the munificence and generous enterprise of Mr. James Gordon Bennett, the proprietor of the *New York Herald*, whose encouragement and support of every undertaking calculated to promote the advancement of science and discovery are well known and appreciated. The work accomplished so far is the result of some years' study of the phenomena of atmospheric movements. The deductions, therefrom, I have endeavoured to reduce to a practical application in these cabled weather-warnings of the *New York Herald*. In this, I believe, a useful step has been made in meteorological inquiry, which may lead to greater and more definite results.

Before February 19th, 1877, the day on which the first weather warning of the *New York Herald* (sent on the night of the 14th) was fulfilled, the question as to the possibility of establishing a reliable connection between

the meteorological phenomena of the American and European continents was unsettled. In stating this I do not ignore the efforts previously made with that object by many scientific men in Europe, like the late M. Leverrier, Director of the Paris Observatory. In many scientific circles the possibility had long ago grown to be regarded as a probability, and public as well as private efforts were being constantly made toward a thorough investigation of the laws of atmospheric movement and of storms. Indeed the failures, so called, that attended these researches were, in reality, successes of the highest importance to meteorological science, because they taught the investigators to eliminate all that was worthless in theory, and pay closer attention to the simpler and grander facts of nature which direct and patient observation made apparent. The chief difficulty in the way of success lay in the limited area of the physical field of investigation. Local phenomena have been treated as general, and the observations made in a comparatively small district have been used to found the theories applied to a hemisphere.

Except in few cases, recent works on meteorology are barren of original information. They are chiefly mad up of quotations from earlier works, and the experiences of isolated observers who, straining after the establishment of narrowly based theories, permit their enthusiasm to lead them to false conclusions. This accusation may, and probably will, be levelled against myself, but I assure the critics that I will submit to any adverse judgment on my work that is based on scientific truth and feel grateful for the enlightenment. Whatever may be the value, or otherwise, of the statements I make, they are based upon personal observations, and depend in no way on the generally accepted meteorological theories with regard to the origin and movement of storms. I aim at winning for my work all that may be due to its merit, while I am willing to bear all the censure for its defects.

The importance to the interests already referred to of a system of weather predictions, which can be published for the general information of the people, several days in advance of the events they announce, is one that cannot be disregarded. We find in America that many branches of trade are seriously affected by weather changes, and that timely warnings are calculated to insure against losses that would, in their absence, be sustained. The great grain-growing districts of the Western States have their respective centres to which the produce is brought for sale, storage, and shipment to the eastern sea-board. Sudden and severe storms not only injuriously affect the condition of the roads and other lines of transportation, and thus delay shipments, but also the produce itself; and the anxiety of the farmer for the safety of his crops is equalled by that of the merchant whose capital is invested in that special branch of trade. Hence, both producer and dealer, as well as the transportation agent, anxiously watch the western horizon, and eagerly receive every item of information bearing on the all-important condition of the weather. The same state of feeling must exist wherever trade flourishes and agriculture represents wealth. Whether the corn be stored in a Chicago elevator ready for shipment to Europe, is borne by the steamship across the Atlantic, or is stored at the centres of consumption in England and France, the conditions vary only in degree. The cotton-fields of the Southern States, the cotton ships on the ocean, and the staple stored in the warehouses of Liverpool or Manchester are under the same all-pervading influence of the weather.

To the seaman the timely storm warning is of paramount importance. Whether he threads his dangerous course among narrow channels along the coasts, or sails boldly into the broad ocean, the foreknowledge of an approaching storm causes him to adopt those precautions which insure his safety. The dreadful story of shipwreck which has been continued through the annual chapters of the past twenty-five years, will reach its hoped-for "*Finis*," when meteorological science effectually aids the nautical skill of the mariner in warding off the great dangers of the sea. Then the headlands of every coast will have their signal stations, and the sailor when taking his parting look at the land he is leaving, or getting his first of that he approaches, will see the warning signal that shall tell him of coming storms, and bid him prepare to meet them.

In many other respects the value of timely storm signals will be immense. Take, for instance, the case of an army on campaign. The general commanding mus regulate his movements as much by his facilities for transportation and supply as by strategic necessities. He must cross rivers and wade through marshes; climb and hold rugged mountain passes; and secure his communications by substantial bridges and practicable roads. His supplies must be largely drawn over difficult routes, and, perhaps, from districts liable to inundations and heavy snow or rain storms. If he relies on a co-operating fleet, the ships must be guarded against storms in exposed anchorages. In a word, the variations in the conditions of the weather must be recognised in all the operations of an army, otherwise great disasters may overtake it, notwithstanding the valour and endurance of the troops and the skill of the commander. I have watched with the greatest interest the progress of the recent campaign in Bulgaria, and have frequently announced in New York many days in advance the changes of weather that impeded the Russian progress, endangered the Danube bridges, and filled the Balkan passes with snow. Such calamities as befell Napoleon in 1812, and a portion of the allied forces in the Crimea in 1854-55, would have been avoided if a meteorological service existed at those times to give warning of the weather changes that produced them.

If a special military service of meteorologists, such as the United States enjoys in its Signal Service Corps, was organised in European armies, many of the difficulties incidental to warfare on that continent could be provided against. But as the foundation of such a system must rest on the accuracy of weather predictions by cable from America, the duties of an Army Signal Corps in Europe with relation to the weather would be simplified to a close observation of the western and southern coasts or frontiers, and the forwarding of information to the proper points. At the present time the European western coasts cannot receive by local observations what can be called timely storm-warnings in the strict sense of the term. The British Channel, the German Ocean, the Baltic, and

West Mediterranean, which represent the chief commercial areas of home navigation, are near the points where the first weather indications present themselves. It is not surprising, therefore, that notwithstanding the vigilance of coast-observers, and the prompt distribution of warnings from London and Paris, that many vessels are overtaken and fairly surprised by storms within sight of the British and French coasts. The *New York Herald* warnings have been forwarded to lessen this danger to navigation in European waters, as well as to give notice of bad weather in the Atlantic to vessels bound for our coasts.

I shall first deal with the field of observation from the West Pacific Ocean to the Ural Mountains.

I will limit my remarks on the general and local phenomena of storms, to which the *New York Herald* system of cable weather predictions relates, to the field of observation that extends from the western part of the Pacific Ocean in a great but irregular zone, eastward to the line of the Ural Mountains.

The irregularity in the width of this field which lies generally between the 10th and 70th parallels of northern latitude is caused by our want of information regarding the meteorology of the far northern sections of this continent and of the region in North Africa between the equatorial zone and the northern limit of the great desert of Sahara.

While the prevailing conditions in these regions may be correctly inferred from their relations to contiguous territories, it will be unsafe for the present to base any assumptions thereon, especially when such are not absolutely necessary for my purpose in this article. I will therefore refer only to the Pacific Ocean, between the 10th parallel and the Aleutian Islands, the North American continent between the same parallel, and the regions of Manitoba and north of the great lakes and Canada; the Atlantic between a line drawn from the intersection of the 40th meridian and the 10th parallel, to the African coast at Cape Blanco; and the line drawn from Cape Farewell, in Greenland, and the North Cape, in Norway; and Europe between the 30th and 70th parallels.

This immense area contains two great oceans familiar to navigators, and the two continents that represent in the majority of their peoples, the commercial enterprise, the power, and the intelligence of the world. It also represents a considerable portion of the earth's surface subjected to a diurnal and equal share of solar influence according to latitude. Whatever may be the real effect of the sun's heat and magnetism in producing atmospheric perturbations, the field selected is that which they must almost uniformly influence, and on which the extent of that influence is most likely to be accurately determined by scientific observation and study.

It will be observed that the oceanic and continental areas are each divided into two sub-areas by well-marked lines; the oceans by equatorial currents having a general direction from south-west to north-east, and the continents by distinct regions of mountain and plain. The distinction in the latter case is most marked on the North American continent, but is also very clearly defined in Europe. We have therefore eight sub-areas of the field of observation, each exercising its peculiar influence on the movement of the atmosphere over the whole field. The *Kuro Siwo* or Japan current of the Pacific Ocean, which corresponds so closely with the Gulf Stream in the Atlantic, moves north-eastward with a smaller resistance from the north polar waters than the Gulf Stream. The narrow Behring Strait, through which the Arctic current must pass southward is even narrower than Smith Sound, consequently the northern waters of the North Pacific maintain a higher general temperature than those of the North Atlantic, but owing to the spreading out of the *Kuro Siwo* over a greater area than the Gulf Stream covers in corresponding latitudes, the waters of the latter are relatively warmer and probably deeper between latitudes 30° and 60°. Hence a more uniform temperature overspreads that part of our field of observation represented by the North Pacific Ocean. It is reasonable to suppose that the compensatory flow of polar water toward the equator comes chiefly from the Antarctic regions in the Pacific Ocean and in nearly equal proportions from both poles in the Atlantic. The effect therefore must be, as I suggest, that the surface of the North Pacific has a very uniform temperature, making due allowance for latitude. The atmospheric conditions are consequently affected so far as to promote the development of large areas of low pressure without many important centres of very violent disturbance. I cannot say if the infrequency of storm centres, as we are accustomed to regard them, on the Pacific, suggested the name, but it cannot be considered an inappropriate one. Violent storms cross the northern parts of this ocean, but they come from the Asiatic continent, and are probably identical with those which had already passed over Northern Europe in their eastward courses. We have no satisfactory evidence that such storms again pass over Europe, but they undoubtedly traverse the circumpolar seas, carrying to those regions the great winds and snows that are experienced by whalers and explorers in the far north.

Over such an immense area of warm water surface as the Pacific presents the atmosphere absorbs an extraordinary evaporation, and in its general eastward movement brings the humid air to the western coast of the American continent, where, by condensation against the mountain chains that extend from Lower California to the Arctic Ocean, it becomes deposited in heavy rains. The liberation of latent heat consequent to this process causes a barometric fall near the coast line, and the development of storm centres which move inland over the Continent, and have been traced from Oregon to Armenia. Cyclones that are developed in the equatorial zone of the Pacific cross the ocean and are experienced on the American coast from latitude 20° to 55°, according to their point of origin, and high or low trajectories. The movements of these storms will be referred to under another head.

On the North American Continent the mountain sub-area extends eastward from the Pacific Coast to the line of the Rocky Mountains. It is represented by a great elevated plateau from four to eight thousand feet above the sea-level, and from three to six thousand feet above the general level of the sub-area of the plains which extends eastward from it to the Atlantic. The peculiar alignment of the axes of the mountain chains running

over this great plateau presents them as direct obstructions to the eastward movement of storms, and their influences on the latter are very marked. Indeed the most interesting study in American meteorology is that of the modifications produced by the great mountain plateau of the west, or the disturbances passing over it. The sub-area of the plains is that in which some of the most remarkable phenomena of storms are observed. The valleys of the Mississippi, Missouri, and Ohio, and the basins of the lakes and Gulf of Mexico are the theatres of tremendous storm movements, and are consequently the favourite areas for observation chosen by American meteorologists. Within them are experienced nearly every type of storm that traverses the Atlantic toward Europe. Unlike the sub-area of the mountains to the westward, that of the plains is favoured with an abundant rainfall, which renders the great expanse fertile in nearly all its sections. The growths of the tropics flourish in the south, and productiveness marks its various climatic zones until the vast pine forests of the north define the agricultural limits. The contrast between the two sub-areas is extraordinary, yet their widely different conditions are easily accounted for when their respective meteorological aspects are studied. The Gulf of Mexico, with its accumulation of tropical waters, plays a very important part in creating the prevailing weather conditions of the sub-area of the plains. From it flows a continuous current of warm humid air, which supplies moisture and energy to the storms that descend from the regions of the north-west into the great river valleys. It is the cradle of the equatorial current that sweeps across the ocean far into the Arctic seas, carrying warmth and verdure to latitudes in Europe far north of the general habitable limit on the American continent. But it is unnecessary to do more than refer to so familiar a region in describing briefly the natural subdivisions of the field of observation.

For the Atlantic, like the Pacific, we have the dividing line of the equatorial current of the Gulf Stream. North and west of that line the surface temperature is low, south and east of it very uniform, and along it high. Air in motion over these surfaces is consequently affected by rapid variations of temperature, which affect in turn the energy of the disturbances traversing the atmospheric volume.

A very marked effect of this kind is produced when storms leave the Nova Scotia coast, and at once commence to pass over the equatorial and Polar counter currents. The pressure falls rapidly, and great gales are induced, but the storm seems to be held for several hours over the region between Nova Scotia and Newfoundland, as if controlled by forces which it strove to overcome. When fairly past Cape Race the movement of the storm is no longer interrupted by the influences of the currents, and makes a very uniform progress towards Europe. When cyclonic storms reach the Florida or Carolina coasts from the Gulf of Mexico their energy seems to be increased when passing over the Gulf Stream, but their courses are not altered very much by the influence of that current. This is probably due to its narrowness when passing along the coast to latitude 35°. Eastward of the Gulf Stream, and over the oceanic region of uniform surface temperatures the energy of the storms decreases somewhat, and the areas of their depressions increase. But on approaching the west coasts of Europe the storms again resume their forces and deposit heavy rains. Europe, like America, is divisible into two sub-areas, one of mountains and the other of plains. The eastern limit of the former is that of a line following the Scandinavian Mountains toward the Alpine development into Saxony, thence following the Carpathian mountain outline, and passing southward over Bulgaria and the Balkans to the Syrian mountains. The irregularity of such a dividing line is very apparent, but we may assume that given to be correct enough for our purposes. In crossing the Scandinavian Mountains, Atlantic storms invariably deposit a great rainfall over Norway and pass into the Gulf of Bothnia and Eastern Russia with a reduced precipitation. When on the great Muscovite plains the storms again increase in area, just as they do in the valley of the Mississippi after crossing the Rocky Mountains in Montana; the break in the dividing-line between the sub-areas of mountain and plain in Europe represented by the Baltic and the low lands of Northern Germany, forms a storm gateway to the interior plains, which is frequently passed by Atlantic disturbances. The mountain systems of Switzerland, Italy, and the Balkan peninsula, perform important parts in modifying the conditions during storm movements in Northern Europe, and have each their peculiar local influences on the weather. If these mountains did not form barriers between the regions of great evaporation with their humid winds from the south, and those of Northern and Central Europe, a parallel between the meteorological phenomena of the Mississippi Valley and those of Eastern and Southern Russia in Europe could be drawn very easily. Having now roughly sketched the field of observation at present available, and suggested here and there a few points worthy of special consideration, I will endeavour in the next article to explain how storms move over the several sub-areas, and the changes they undergo in each.

JEROME J. COLLINS

(*To be continued.*)

NEWCOMB'S ASTRONOMY

Popular Astronomy. By Simon Newcomb, LL.D., Professor U.S. Naval Observatory. (London: Macmillan and Co., 1878.)

A WORK on popular astronomy by an author so distinguished in the higher branches of the science as Prof. Newcomb, will be welcomed with more than ordinary interest. The main object of the present volume is to present the general reader with a condensed view of the history, methods, and results of astronomical research, especially in fields of most popular and philosophical nature at this epoch, in such language as to be intelligible without mathematical study; it has not been designed to instruct either the professional investigator or the special student of astronomy.

In his first chapter the author briefly treats of the phenomena of diurnal motion, the motion of the sun amongst the stars, the precession of the equinoxes, of the moon's motion, and of eclipses of the sun and moon, concluding with some account of the calendar. In his

second and third chapters the true or Copernican system of the universe is described, the obliquity of the ecliptic, the seasons, &c., according to this system, the Keplerian laws of planetary motion, and progress from Kepler to Newton. The latter of these chapters is devoted to Newton's discovery of universal gravitation and consequences flowing from it; the gravitation of small masses, the figure and density of the earth, the tides, the inequalities in the motions of the planets produced by their mutual attraction, and the relation of the planets to the stars. The three chapters form the first part of the work, or "the history of the world historically developed."

The second part is devoted to practical astronomy, the telescope and the successive improvements and modifications introduced in its construction and application down to the present day. The cumbrous yet elaborate form in which an instrument, powerful for its time, was used in the middle of the seventeenth century, is well illustrated by an engraving of the great telescope used by Blanchini in the observations whereby he attempted to determine the time of rotation of the planet Venus upon its axis, one of the instruments constructed by the celebrated Campani, mounted in the grounds of the Barbarini Palace at Rome, and extracted from the historical work "Hesperi et Phosphori Nova Phenomena." As specimens of modern optical and mechanical achievement, we have illustrations and descriptions of the great reflectors of the Earl of Rosse and Mr. Lassell, of the Melbourne instrument and the new reflector in the grounds of the Observatory of Paris. The great refractor of the Naval Observatory of Washington is represented in the frontispiece—a reduction from the picture forming one of the series in the last volume of Washington Astronomical Observations; the instrument with which observations have been made that have afforded us the first really satisfactory knowledge of the elements of the orbits of the four satellites of Uranus and the satellite of Neptune, and what is of still greater interest, the instrument with which Prof. Asaph Hall has brought to light the two minute satellites of Mars, a discovery justly characterised by Leverrier as one of "the most important observations of modern astronomy." The application of the telescope to celestial measurement, the meridian circle and its use, the determinations of time and of terrestrial longitudes, are also considered, and the author proceeds to treat of parallax in general, and in particular of the investigation of the solar parallax, through the intervention of the parallax of one of the planets Venus and Mars when nearest to the earth. Prof. Newcomb supplies a sketch of progress and results in this direction from the first application of the method to the planet Mars, on occasion of the French expedition in 1671, when Richer was sent out to the colony at Cayenne, in South America, to secure observations of Mars, while corresponding observations were made at the Observatory of Paris, from the discussion of which observations Cassini made what is usually given as the first reliable approximation to the amount of the sun's parallax, the resulting value being $9''\cdot5$. The author, however, remarks upon the determination made by Huyghens at the end of his "Systema Saturnium," as the best of the seventeenth century, the reason, as he states, of its being the best being that it was not founded on any attempt to measure the parallax itself, which was then really incapable of measurement, but on the probable magnitude of the earth as a planet. The idea of Huyghens was that the earth, being a planet, its magnitude would probably be somewhere near that of the average of the two planets on each side of it, viz., Venus and Mars. So, taking the mean of the diameters of Venus and Mars, and supposing this to represent the diameter of the earth, he found the angle which the semi-diameter of the supposed earth would subtend from the sun, which would be the solar parallax. By a fortunate accident, Prof. Newcomb remarks, Huyghens's estimate was nearer the truth than any determinations made previous to the transit of Venus in 1769, "his result for the distance of the sun being 25,086 semi-diameters of the earth, or 99,000,000 of miles." But it is to be noted that if Huyghens had used the correct measures of Venus and Mars, he would have been further from the truth; his telescopes showing the planets with diameters in excess of the true ones, "he just hit the diameter of the earth, and reached the true solution of the problem." This attempt of Huyghens to ascertain the amount of solar parallax, is not often mentioned in our astronomical treatises.

The section bearing upon investigations of the solar parallax from transits of Venus, though brief, contains some interesting facts; the proceedings of the American expeditions for the observation of the transit of 1874, are particularly noticed. It appears that the stations finally occupied by the observers sent out from the United States were, Wladiwostok, in Siberia, Pekin and Nagasaki, Japan, in the northern hemisphere, and Kerguelen Island, Hobart Town, and Campbelltown, Tasmania, Queenstown, N.Z., and Chatham Island in the opposite one. The American astronomers relied chiefly upon the photographic method of observing the transit, as possessing obvious advantages over the old method of noting the contacts, and the author describes and illustrates by a diagram, how by the use of a telescope of great length—nearly forty feet—difficulties in the measurement of the photographs were sought to be obviated by the French and American parties, as well as by Lord Lindsay. With regard to the success attending the American expeditions, Prof. Newcomb, (who it may be remarked was one of the principal agents in the arrangement of their equipment and plans of observation) states that the full number of photographs expected was not obtained at any station—but that the result taking the stations collectively, was about half this number; the British, French, and Russian expeditions were about equally successful, while the most fortunate, as regards weather, were the German parties who were successful at all six of their stations. He adverts to the amount of labour attending the investigation and measurement of the photographs, and with respect to the time when a final result may be expected to be worked out from the observations of the transit of 1874, having in view the comparison of the whole to ascertain how consistent they are with each other, adds: "this cannot be done for several years," yet that upon the question whether it is worth while to send out parties to observe the transit of 1882, which must soon be a subject of discussion among astronomers, the answer must depend very largely on the success of the

efforts made in 1874. We should be inclined to hope, however, that the results of Mr. Gill's expedition to Ascension, with Lord Lindsay's heliometer, for observations of the recent close opposition of the planet Mars, may very materially facilitate a decision upon this point. If, as many practical astronomers have anticipated, an equally reliable determination of the sun's distance can be obtained from measuring heliometrically the diurnal parallax of Mars at those oppositions when he approaches nearest to the earth, as from the observation by the combined exertion of civilised nations, of a transit of Venus, then it may be reasonably expected that a method admitting of comparatively such frequent repetition, and involving also so small an outlay, not only as to cost, but labour of preparation, must be preferred by astronomers generally, thus facilitating a proper conclusion with respect to expensive preparation for observing the transit in 1882.

After briefly describing other methods of approximating to the amount of solar parallax which have been applied, including M. Cornu's determination from measurement of the velocity of light and Leverrier's results from the planetary theories, and the method first suggested by Prof. Galle, by measuring the parallax of the small planets, a method which, in a modified form, was applied with so much success on the occasion of the near opposition of Juno in 1874, Prof. Newcomb sums up results by stating that "from the general accordance of the various methods described, it would appear that the solar parallax must lie between pretty narrow limits, probably between 8″·82 and 8″·86, and that the distance of the sun in miles probably lies between the limits 92,200,000 and 92,700,000." This, however, it must be observed, appears to have been written before any of the results of the transit of Venus were published, as the author expresses the hope that after their discussion the uncertainty may be brought within yet narrower limits.

The chapter concludes with an outline of the various investigations of stellar parallax, the most trustworthy values being collected in a tabular form at the end of the volume, and the same has been done as regards successive determinations of the mean parallax of the sun. By thus avoiding the introduction of any considerable amount of numerical detail into the text, the volume is rendered much more readable; indeed, in this respect we may remark, once for all, that the author's arrangement leaves nothing to be desired.

The second part of the work further includes chapters on the motion of light as measured by celestial observations, and experimentally on the methods devised or practised by MM. Foucault, Fizeau, and Cornu; the application of the revolving wheel is explained. A brief outline of the principles of spectral analysis as applied to the heavenly bodies is presented in conclusion. With reference to doubts which have found expression at times as to the degree of certainty attaching to some of the inferences drawn from spectroscopic observation, and remarking that the dark and bright lines in the spectrum are "the letters of the open book which we are to interpret so as to learn what they tell us of the body from which the light came, or the vapours through which it passed," the author introduces the question, How do we know but that the lines we observe may be produced by other substances besides those which we find to produce them in our laboratories? May not the same lines be produced by different substances? The answer to this question can only be founded upon an appeal to probabilities. "The evidence in this case is much the same as that by which, recognising the picture of a friend, we conclude that it is not the picture of any one else. For anything we can prove to the contrary, another person might have exactly the same features, and might, therefore, make the very same picture. But, as a matter of fact, we know that practically no two men whom we have ever seen do look exactly alike, and it is extremely improbable that they ever would look so. The case is the same in spectrum analysis. Among the great number of substances which have been examined with the spectroscope, no two give the same lines. It is therefore extremely improbable that a given system of bright lines could be produced by more than one substance." Nevertheless, it is remarked that the evidence of the spectroscope is not necessarily conclusive in all cases. In the case of a single line only of a substance being found in the spectrum of a star or nebula, it would hardly be safe to infer from this alone that the line was really produced by the known substance, In such doubtful instances collateral evidence must be allowed its weight, and conclusions must be drawn with care and discrimination, in accordance with the probabilities of each special case.

The third part of the work is devoted to a description of the sun and planets, the comets and meteors. The author has had the advantage of outlines of the views of several distinguished students of the physical constitution of the sun, which he presents in their own words. These include notes by Father Secchi, M. Faye, and Prof. Young. On the subject of intra-mercurial planets he remarks upon the fact of suspicious objects thus far having been seen only by amateur observers, and escaping the skilled astronomers who have occupied themselves in watching the sun's disc, and appears to consider that this circumstance places their real existence beyond moral probability. He favours the idea that if the motion of the perihelion of Mercury be due to the action of a group of planets, they are each so small as to be invisible in transits across the sun, and during total eclipses, and yet being so large in the aggregate, their number must be counted by thousands, and if seen at all they would appear only as a cloud-like mass. The zodiacal light offers this aspect, and the question arises whether the matter which reflects this light can be that which affects the motion of Mercury, and Prof. Newcomb is rather in favour of this explanation, though, he adds, "a great deal of research—more, in fact, than is likely to be applied to the subject during the present generation —will be required before the question can be settled."

The fourth part treats of the stellar universe, the second chapter on the structure of the heavens meriting especial attention. Space will not allow of our entering in detail upon the contents of this last portion of the volume; it concludes with an expression of the author's ideas relative to the much-discussed question of the Plurality of Worlds. This last part will probably possess greater interest for many than the rest of the work, though necessarily entering upon subjects not yet removed from the region of speculation. Prof. Newcomb,

however, does not mix up what is merely speculative with well-established conclusions in such way as to mislead his readers who may be entering upon the study of astronomy—a failing of too many works issued at the present day.

As affording a thoroughly reliable foundation for more advanced reading, Prof. Newcomb's "Popular Astronomy" is deserving of strong recommendation.

J. R. HIND

SLATE AND SLATE QUARRYING

A Treatise on Slate and Slate Quarrying, Scientific, Practical, and Commercial. By D. C. Davies, F.G.S. (London: Crosby Lockwood and Co., 1878.)

AMONGST the manufacturing industries which, during the last hundred years, have expanded into large proportions is the production of roofing slate. Nor is it difficult to account for this expansion. Building operations have in this period progressed both over town and country, both in the Old and New World with extraordinary rapidity, while canals and railroads have facilitated the transport of the roofing slates from their mountain sources to all parts of the land, and ships traverse the seas with cargoes of the same material to various countries. Before the introduction of canals and railways into the British Isles, the slates of Wales, Cumberland, Scotland, and Ireland were restricted to the immediate neighbourhood of the quarries from which they were extracted, and buildings in various parts of the country far removed from these quarries, were supplied with roofing materials from other sources. In many districts tiles of burnt clay formed the only available material, while in others, flag-stones and tile-stones from the Carboniferous, Triassic, or Oolitic formations were extensively used. In the eastern and central districts of England the tile-stones of Stonesfield, near Oxford, those of the Cotteswold Hills, and of Collyweston formed an available source of supply; and it must be admitted that their greyish colour and general appearance harmonise well with the prevalent Gothic or Tudor styles of architecture of those districts. To such an extent is this admitted that these tilestones (erroneously called "slates") are still largely used in the counties of Northampton, Oxford, and Gloucester, even when the Welsh slate might be obtained at an equal or less cost, and, owing to their heaviness, the high-pitched roofs, which are so ornamental, and add so much to the appearance of buildings, became a necessity. Nevertheless, the Oolitic tilestones are inferior in strength, lightness, and durability to the latter material, and are only used where æsthetic considerations prevail over those of economy.

In no country of equal extent has the art of slate-quarrying reached such proportions as in the British Isles, and especially amongst the mountains of North Wales, which is its principal seat; and considering the magnitude of the works carried on, the large number of persons employed, and the enormous sums made and lost in this branch of trade, it is somewhat strange that no work specially devoted to the subject of slate quarrying has appeared up to this time. We therefore welcome Mr. Davies' little treatise, in which will be found a large amount of interesting and valuable information regarding the slate industries of North Wales and other districts, gathered from much experience and observation, and placed before the public in a very readable form.

Mr. Davies, being a geologist, treats his subject geologically, recounting the various formations of North Wales in which the various "veins" or beds of the best slates are to be found; and giving numerous details, often illustrated by sketches of the stratigraphical phenomena which are encountered in the quarries. Few people have any idea of the physical impediments which occur in such places. What with dykes, veins, slips, the disappearance of cleavage planes, the local change in texture and composition of the slate itself, and other disturbances and "troubles," it is only a comparatively small proportion of the entire slate-bed which can, even in the best quarries, be converted into marketable slates, of the larger sizes and qualities, known as "Princesses," "Duchesses," and "Countesses." Hence it is, that while a band of slate-rock, in a favourable position for carriage, and comparatively free from such impediments to its useful application, is a source of profit, another band, which is not so free from these accidents of stratification, remains a profitless, or ruinous possession.

Commencing with the oldest formation of North Wales, the Lower Cambrian of Prof. Sedgwick, Mr. Davies describes the eminently successful quarries opened in this formation in the Pass of Llanberris, which have proved a source of untold wealth to their fortunate owners. The slates from this formation are generally purple or greenish —locally becoming greyish—and are amongst the smoothest and strongest in Wales. The succeeding formations of the Upper Cambrian and Lower Silurian are also productive of slates—generally of bluish and dark tints—and are worked over the central portions of the mountain region. The value of such beds of slate depends chiefly upon the uniformity and fineness of the grain of the slate, and the facility with which the rock splits along the planes of cleavage—which, as all geologists are aware, are independent of those of bedding. In reference to the origin of the cleavage structure, we are glad to find that the author adopts the "mechanical theory," which ascribes the structure to the enormous lateral pressure to which the rocks have been subjected when undergoing contortion; but in enumerating the observers who have contributed towards our knowledge on this subject, he has omitted the name of the late Mr. Daniel Sharpe, whose remarkable papers published in the *Journal* of the Geological Society (vols. iii. and v.), clearly established the relationship between cleavage and pressure, and the structural alterations which have been brought about within the mass of the slate-rock itself, as subsequently confirmed and illustrated by Mr. Sorby, after the microscopical examination of thin sections.

The author's notices of slate-production in districts other than those of North Wales are scanty, and consist of extracts from other works. His book is, therefore, mainly valuable for the information it affords regarding the position, structure, and mode of working the bands of slate in the lower palæozoic formations of North Wales; and as such it will be found a useful guide-book.

OUR BOOK SHELF

Familiar Wild Flowers. Figured and Described by F. E. Hulme, F.L.S., F.S.A. With Coloured Plates. Parts I.-XIII. (London: Cassell, Petter, and Galpin.)

THERE has certainly been a wonderful improvement of late years in the art of chromo-lithography as applied to botanical illustrations; and the specimens in the work before us are among the best that we have seen. The colouring, the outline drawing, and the general representation of habit, are all remarkably true to nature. The floral initial letters and tail-pieces, which are stated to be drawn "by various artists," are not so uniformly successful. Each part, published at the remarkably low price of sixpence, contains two coloured plates, more than one species being occasionally placed on a plate. The accompanying letter-press descriptions, though rather shorter than would in many cases be desirable, are written in plain and easy and not too technical language. There is no indication of the proportion of the British flora intended to be included under the designation of "familiar wild flowers;" but whenever the volume is completed, it will be a useful addition to our popular botanical literature, and well calculated to promote an accurate knowledge of the common plants of our fields and hedges.

Heroes of South African Discovery. By N. D'Anvers. (London: Marcus Ward and Co., 1878.)

The Countries of the World. By Robert Brown, M.A., Ph.D., &c. Vol. ii. (London: Cassell. No date.)

The Life of Sir Martin Frobisher. By the Rev. Frank Jones, B.A. (London: Longmans, 1878.)

THERE seems to be no end to the number of geographical works published nowadays. Mr. D'Anver's work is a companion volume to "Heroes of North African Discovery," by the same author, already noticed by us. Like its predecessor its numerous pictures and the many adventures of the "heroes" of its pages will render it attractive reading for boys, who, if they read it faithfully, will carry away with them much valuable information. The work does not pretend to anything like minute research, but so far as it goes, it is, we believe, trustworthy.

The present volume of Dr. Brown's work, which may be taken as a typical specimen of Messrs. Cassell's showy popular publications, deals mainly with the United States and Mexico. Dr. Brown has taken considerable trouble to obtain varied information concerning the different States, and his account of them is fairly full and accurate. In a work like this he cannot be blamed for repeating the oft-told story of his adventures in the west and north-west, though the style, rather than the stories, pall somewhat on one. The pictures, we believe, may be taken as on the whole what they purport to be; though it is curious to notice the uniformity of Nature under different conditions, and at widely separated places. One of the illustrations connected with Mexico is entitled a "Lagoon in the Sierra Calientes." Dr. Brown will be interested to know that an exactly similar scene is pictured as occurring on the banks of the Ucayli in South America, in "Paul Marcoy's" Travels; but as it is doubtful if "Paul Marcoy" was ever many miles from Paris, the "Scene on the Ucayli" may be as mythical as his "Travels."

Judging from the formidable list of authorities given by Mr. Jones, his life of the rough, but brave and even chivalrous old Frobisher must be the result of much research. Mr. Jones seems, however, to be entirely deficient in literary skill; his materials have been put together in the crudest manner possible. Though Frobisher added little to geographical knowledge, he deserves a place among the heroes of the North-West Passage for his three attempts to discover it. Unfortunately the object of his last two expeditions was to bring home shiploads of the "black earth" which people had been deluded into believing was rich in gold, and all Frobisher's efforts at discovery were balked. His life deserved to be written, but we cannot say that Mr. Jones has shown himself competent for the task.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

The Telephone

THE following experiments lately made as to the use of the telephone in connection with a magneto-electric machine, have given results which are somewhat interesting.

In the first instance a small medical magneto-electric machine was employed with the result (described by Mr. A. Percy Smith, NATURE, vol. xvii. p. 380), of a loud click at each rotation of the bobbins in front of the magnet. Driving the former by means of a small turbine, the clicks combined to form a loud musical note which rose and fell as the speed of rotation was increased or diminished. This note was well heard through a resistance of 32,000 units.

A magneto-electric exploder having two horseshoe magnets, four bobbins, and two rotating armatures was next employed. This gave a loud sound through 57,000 units of resistance. With a view to test the power of the machine to work through bad insulation, it was tried through about thirty yards of bare copper wire lying on wet grass. The sound was still powerful.

A break was then made in the line by cutting it across and dipping the two ends in a fountain basin filled with water. The two ends in the water were about twelve feet apart, and the sound was still perfectly audible. It was found in this experiment that it was *not* necessary to connect the magnetic exploder to earth, and that a sound feebler, but quite distinct, was obtained when *only a single wire was led from it.* The line was thus from exploder through twelve feet of water to telephone, the other binding screw being connected to a wire simply touching the wet gravel, there being therefore no return line.

Again the exploder and telephone were connected to a stretched wire belonging to a fence, at a distance apart of about fifty yards. The wire was supported by fifteen intermediate iron uprights with their ends buried in the ground. Earths were made for the telephone and exploder by means of a clasp knife and a little garden fork. A perfectly distinct sound was heard.

Lastly, one terminal of the exploder and telephone were connected by a wire, the others being joined by a length of twenty-four feet of thin string dipped into river water and subsequently drawn through a dry cloth. An audible sound was noticed.

The above experiments seem to point to two conclusions:—

1. That magneto-electric currents can be employed through exceedingly defective insulation, almost no insulation, in fact.

2. The omission of the earth connection of the exploder seems to indicate that the production of the sound is due either to a very slight leak from the exploder to earth—the machine was inclosed in a wooden box standing on a wooden table—or, not impossibly, to the rapid variation of potential in the line.

In the way above indicated it would appear to be possible to transmit the Morse code by means of magneto-electric currents under conditions which would render a battery absolutely inapplicable.

GEORGE S. CLARKE
HERBERT MCLEOD

Cooper's Hill, April 17

Poisonous Australian Lake

PERHAPS some of your readers may be interested in the following:—

This year the lakes forming the estuary of the Murray have been very low and the water unusually warm. The river is very low and the inflow to the lakes very slight and having a temperature of 74° F. Lake Alexandrina—on calm days surface

76°, depth 73°—during breezy temperature is 72°. A conferva that is indigenous and confined to the lakes has been produced in excessive quantities, so much so as to render the water unwholesome.

It is, I believe, *Nodularia spumigera*, allied to protococcus. Being very light, it floats on the water except during breezes, when it becomes diffused. Thus floating, it is wafted to the lee shores, and forming a thick scum like green oil paint, some two to six inches thick, and as thick and pasty as porridge, it is swallowed by cattle when drinking, especially such as suck their drink at the surface like horses. This acts poisonously, and rapidly causes death; symptoms—stupor and unconsciousness, falling and remaining quiet, as if asleep, unless touched, when convulsions come on, with head and neck drawn back by rigid spasm, which subsides before death. Time—sheep, from one to six or eight hours; horses, eight to twenty-four hours; dogs, four to five hours; pigs, three or four hours.

A *post mortem* was made on a sheep that had thirty ounces of fresh scum administered by the mouth: death was long coming on—about fifteen hours; examination made six hours after. Stomachs: none of the green scum left, all absorbed; dry grass food in stomachs. Abdominal cavity contained two pints of yellow serum; heart flaccid, but not pale; great effusion of serum around it. Lungs, liver, kidneys, and substance of brain healthy and normal, but the dura mater congested. Blood throughout veins and arteries and in both ventricles black and uncoagulable, neither did it become scarlet on exposure to the air. Many sheep that died, on being opened, presented the same appearances, all being without any sign of its presence in the stomachs.

This shows that the plant is rapidly absorbed into the circulation, where it must act as a ferment and cause disorganisation. The cattle will not touch the puddles where the scum has collected and gone putrid. Thus all they take is quite fresh, and the poisoning is not caused by drinking a putrescent fluid full of bacteria as at first supposed. When this scum collects on the banks and is rapidly left dry, it forms crusts of a green colour. This has gone out of the Murray mouth into the ocean and been wafted ashore, forming thick beds of green stuff from a few inches to twelve inches thick. When, however, this scum is left in wet pools and puddles it rapidly decomposes, giving off a most horrid stench like putrid urine, or archil in process of manufacture; but previous to its getting into that state it emits the smell of buytric acid, smelling like very rancid butter.

There exudes from this decomposing matter a blue pigment which has remarkable properties. Sample tube 1 contains the fluid as strained off from the scum and will be found full of bacteria. No. 2 is the same with glycerine, and filtered to separate the bacteria.

This fluid is remarkably red, fluorescent by reflected light, being blue by transmitted light. Spectrum a broad and deep band total at top in the red, but shading off to green, quite cutting off orange and yellow.

Chemical properties:—Heat destroys colour; sulphuric acid no action; nitric acid reddens; hydrochloric acid, the alkalis, and ammonia, destroy colour; chlorine and ozone bleach; light but little action, yet sunlight gradually bleaches; dries to a mass, retaining colour; soluble in water, glycerine, and weak alcohol. I think this is allied to the colouring matter of some lichens, is a product of decomposition, and not pre-existing in fresh plants. Its fluorescent powers are remarkable, and the most powerful I have ever met with. GEORGE FRANCIS

Adelaide, S. Australia, February 11

Transmission of Vocal and other Sounds by Wires

THE following are notes of some additional experiments since those recorded in my paper laid before the Physical Society of London, an abstract of which appeared in NATURE of 25th inst.

1. An ordinary iron fence railing was selected containing six lines of wires varying from $\frac{3}{16}$ to $\frac{1}{4}$ inch in thickness. These wires were passed through iron supports at every *two yards*. A disc, mouth and ear-piece, was attached to one of the wires when speaking, singing, whistling, and breathing were transmitted through distances varying from twenty to sixty yards, whilst the sound of a small tuning-fork was heard at 100 yards.

2. In an iron fence, with heavy iron top-rail, half inch square in section, and having iron supports at *every yard*, it was found that the above-mentioned sounds could be transmitted through about thirty yards; the tuning-fork sound, however, was heard at sixty-six yards.

In the latter experiment the best results were got with a hollow wooden mouth-piece, pressed against the iron, the ear being connected with the iron by means of a solid body, such as a cork.

3. Some yards of No. 16 copper wire were attached to the ordinary bell-wire connection from one room to another; another portion of the same copper wire was attached to the brass bell crank in another room—a lobby intervening;—speaking, singing, and other sounds were readily transmitted; the tone was low, but clear.

For this experiment the terminal discs were of pasteboard, set in metal rims.

In the experiments with the iron fence, the sounds were free to pass not only up and down the particular wire selected, thus necessarily *doubling* the range of distance given above, but suffered breaking up at each support, and consequent distribution through the other wires.

Glasgow, April 27 W. J. MILLAR

Westinghouse Brake

THE experiment shown by the Westinghouse Brake Company was described by Sir W. Armstrong as long ago as 1843, in a paper "On the Efficacy of Steam as a Means of Producing Electricity and on a Curious Action of a Jet of Steam upon a Ball" (*Phil. Mag.* xxii.). The explanation of the phenomenon as due to the centrifugal force of the diverted jet is given in general terms in Young's "Lectures on Natural Philosophy" (Lecture xxiv. p. 297). R.

April 28

The Oxford Commissioners' Statement

MAY I be permitted to draw attention to the very marked discrepancy between the arrangements proposed by the University of Oxford Commissioners for the Animal and for the Vegetables side of Biology? Assuming, as we fairly may, that by "Physiology" the Commissioners mean Animal Physiology, and supposing—what is by no means improbable—that the future Reader in Invertebrate Anatomy would refuse the Professorship of Zoology; when that office is next vacant, we see that there would be *four* University Professors (or Readers) of Animal to *one* of Vegetable Biology; while we may also note that at Christ Church there is a Reader in Anatomy, and that at no College is there any Reader in Botany.

When the efforts, which may fairly be described as violent, to effect the removal of the Botanical Gardens to a peculiarly objectionable site failed, it was hoped that those who, wittingly or unwittingly, endeavoured to paralyse the study of Botany in this place, would have yielded fairly.

The suggestions now made lead us to fear that the Commissioners have been persuaded to do what the University would not do.

At any rate, if the matter is too delicate for the Professor of Botany to deal with, it is to be hoped that other Botanists will make proper representations to the University Commissioners.

April 27 B.

Contact Electricity

IF a Volta's condenser be formed of an iron and a copper plate having their surfaces of contact well ground together, it is found that, on placing them together and then separating them, the iron acquires a positive charge and the copper a negative. This occurs so long as the atmosphere surrounding the plates is the ordinary one containing watery vapour and other oxygen compounds. But if the atmosphere contain sufficient hydrogen sulphide, the iron will be found negatively and the copper positively electrified. Sir Wm. Thomson has shown that "a metal bar insulated so as to be movable about an axis perpendicular to the plane of a metal ring made up half of copper and half of zinc, the two halves being soldered together, turns from the zinc towards the copper when vitreously electrified, and from the copper towards the zinc when resinously electrified."

Substituting for the zinc half of ring an iron half, the same effect takes place, but in a less degree; but if the ring be

surrounded by an atmosphere containing sufficient hydrogen sulphide, the opposite effect occurs. Now the needle, when vitreously electrified, turns from copper to iron; when resinously, from iron to copper. The conclusion to be drawn from these results seems to be that the electrical behaviour of metals in contact is almost, if not entirely, due to the difference of their affinities for one of the elements of such compound gases as may be in the atmosphere surrounding them. This would be entirely analogous to their behaviour in electrolytes containing these same elements, *e.g.* iron is positive to copper in an oxidising electrolyte such as water, because of the affinity of oxygen for iron being greater than for copper, while iron is negative to copper in potassium sulphide solution, because of the affinity of sulphur being greater for copper than for iron. J. Brown

Edenderry House, Belfast

Solar Halo

The following was noticed at Bordeaux on April 4, at 11 A.M.:—1. A well-defined and very complete circumzenithal circle (80° in diameter), of a brilliant white light, passing through the sun. 2. An iridescent circle, larger than the first, and cutting it at two points 60° distant from the sun. The second circle showed more especially the red rays on its concavity (*i.e.* towards the sun), except at the parhelia, where it was bright iridescent. Near the western parhelion the brilliancy of the mock sun was quite insufferable to naked eyes.

The morning was very warm, but the night had been very cold. E. Rodier

29, Rue Saubat, Bordeaux, April 20

FLOATING MAGNETS

The extract from the *American Journal of Science* describing experiments with floating magnets by Mr. Alfred M. Mayer to illustrate the equilibrium of mutually-repellent molecules each independently attracted towards a fixed centre, which appeared in Nature, vol. xvii. p. 487, must have interested many readers.

It has interested me particularly because the mode of experimenting there described, with a slight modification, gives a perfect mechanical illustration (easily realised with satisfactory enough approximateness) of the kinetic equilibrium of groups of columnar vortices revolving in circles round their common centre of gravity, which formed the subject of a communication I had made to the Royal Society of Edinburgh on the previous Monday. In Mr. Mayer's problem the horizontal resultant repulsion between any two of the needles varies according to a complicated function of their mutual distance readily calculable if the distribution of magnetism in each needle were accurately known. Suppose the distributions to be precisely similar in all the bars and in each to be according to the following law:—Let the intensity of magnetisation be rigorously uniform throughout a very large portion, C D, of the whole length of the bar (Fig. 1), and let it vary uniformly from C and D to the two ends A and B. The bar will act as if for its magnetism were substituted ideal magnetic matter,[1] or polarity, as it may be called, uniformly distributed through the end portions C A and D B; the whole quantity in D B to be equal in amount and opposite in kind to that of C A. For example, suppose true northern polarity in A B and true southern in B D. The lengths of C A and D B need not be equal. Let now A′ C′ D′ B′ be another bar with an exactly similar distribution of magnetism to that of A C D B, and let the two be held parallel to one another. The mutual repulsion will vary inversely as the distance, if the distance be infinitely small in comparison with D B or C A, and if each of these be infinitely small in comparison with C D. If the true south pole S of a powerful bar-magnet be held in a line midway between B A and B′ A′, at a distance from the ends B and B′ infinitely great in comparison with B B′, and comparable with the length of each needle, the horizontal component of its effect on each magnet will be a force varying directly as its distance from the central axis. Under these conditions Mr. Mayer's experiments will show configurations of equilibrium of two, or three, or four, or any multitude of ideal points in a plane, repelling one another with forces inversely as the mutual distances, and each independently attracted towards a fixed centre with a force varying directly as the distance. This, as I showed in my communication to the Royal Society of Edinburgh, is the configuration of the group of points in which a multitude of straight columnar vortices with infinitely small cores is cut by a plane perpendicular to the columns; the centre of inertia of a group of ideal particles of equal mass placed at these points being the fixed centre in the static analogue.

The consideration of stability referred to by Mr. Mayer has occupied me much in the numerical problem, and it is remarkable that the criterion of stability or instability is identical in the static and kinetic problems. In the static problem it is of course that the potential energy of the mutual forces between the particles, together with that of the attraction towards a fixed centre is less for the configuration of stable equilibrium than for any configuration differing infinitely little from it. The potential energy of the attractive force is a function of distance from the central axis, diminishing as the distance increases, and the statement of the criterion may be conveniently modified to the following:—

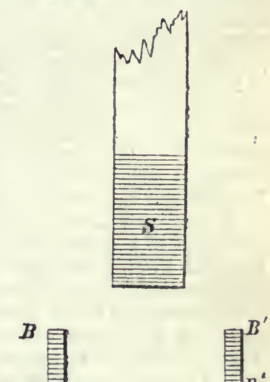


Fig. 1.

For a given value of this function the mutual potential energy of the atoms must be a minimum for stable equilibrium. When, as supposed above, the attractive force varies directly as the distance its potential energy is:—

$$C - \tfrac{1}{2} c \, \Sigma r^2$$

where C, c, denote constants, and Σr^2 the sum of the squares of the distances of all the particles from the attractive centre. And when the law of force between the particles is the inverse distance, their mutual potential energy is equal to—

$$K - k \log. (D\, D'\, D'' \ldots .)$$

where K, k, denote constants, and D, D′, D″, &c., denote the mutual distances between the particles. Thus the condition of stable equilibrium becomes that the product of the mutual distances between the particles must be a true maximum for a given value of the sum of the squares of their distances from the attractive centre. A first conclusion from this condition must be that the centre of gravity of the particles must be the attractive centre. Now the condition of kinetic equilibrium of a group of vortex columns, that is to say the condition that they may revolve in circles round their common centre of inertia is, as proved in my communication to the Royal Society of Edin-

[1] Reprint of papers on Electrostatics and Magnetism, § 469 (W. Thomson).

burgh, that the product of their mutual distances must be a maximum or minimum or a maximum-minimum for a given value of the sum of the squares of their distances from the common centre of gravity;[1] and the condition

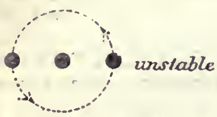


FIG. 2.

FIG. 3.

that this kinetic equilibrium may be stable is that the product be a true minimum for a given value of the sum of the squares of their distances from the centre of inertia. Taking for example a triad of vortices (or of the little magnetic needles of Mr. Mayer's problem), it is thus obvious the equilibrium is unstable in the case represented by Fig. 2, and stable in the case represented by Fig. 3. The arrow-heads in Figs. 2 and 3 represent the motions of the vortex columns round their centre of gravity. It must be understood that the core of each column revolves also round its centre of gravity in the same direction as the group round the common centre of gravity of all with enormously greater angular velocities.

I have farther considered the problem of oscillations in the neighbourhood of configuration of stable equilibrium. The general problem which it represents for mathematical analysis has a very easy and simple solution for the case of a triad of equal vortex columns in the neighbourhood of the angles of an equilateral triangle.

A mechanism for producing it kinematically is represented in Fig. 4, showing three circular discs of cardboard pivotted on pins through their centres at the angles of an equilateral triangle rotating in a vertical plane. The plane carrying these three centres may be conveniently made of a circular disc of stiff cardboard, or of light wood pivotted on a fixed pin through its centre. Each of the small discs or epicycles is prevented from rotation

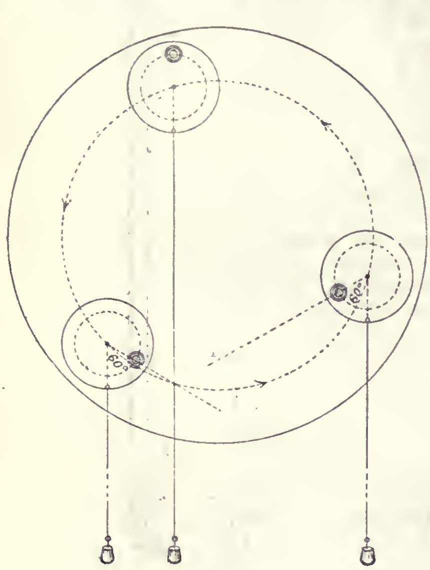


FIG. 4.

by a fine thread bearing a weight, and attached to a point of its circumference; and on each of them is marked, by a small dark shaded circle, the section of one of the vortex cores in proper position.

[1] Helmholtz proved that whatever be the complication of motions due to mutual influences among the vortices, their centre of gravity must remain at rest.

The rule for placing the vortices on their epicycles is as follows:—Each vortex keeps a constant distance from its mean position (this being the centre of the epicycle, carrying it in the mechanism); each of the radius vectors drawn from the centres of the epicycles to the centres of the vortices keeps an absolutely fixed direction, while the equilateral triangle of the centres of the epicycles rotates uniformly; and these three fixed directions are inclined to one another at equal angles of 120° measured backwards relatively to the order in which we take the three vortices. It is easily verified that when the distances of the vortices from their mean positions are infinitely small (that is to say, when the triangle of the triad is infinitely nearly equilateral), the product of its three sides remains constant in the movement actually given by the mechanism, and so does the sum of the squares of the distances of its three corners from its centre of gravity. From the stability of the equilateral triangle it follows[1] that there must be stability with three equal vortices at the corners of an equilateral triangle, and one (whether equal to them or not) at its centre,[2] . For four equal vortices I have found that the square order, , also is stable. From the stability of the square follows (for vortices or for particles repelling according to inverse distance) the stability of four equals at the corners of the square, and one (whether equal to them or not) at its centre,[3] . I have not yet ascertained *mathematically* whether for a pentad of equal vortices there is stability also in the pentagonal arrangement, . But Mr. Mayer's experiment, showing it to be stable for the magnets, is an experimental proof that it must be stable for the vortices, for it is easily proved that if any of the figures is stable with mutual repulsion varying more rapidly (as is the case with the magnets in Mr. Mayer's experiment), than according to the inverse distance, *à fortiori*, it must be stable when the force varies inversely as the distance. From the stability of the pentagon I infer (for vortices and for particles repelling according to inverse distance) the stability of the configuration .

Mr. Mayer's figure [4] . . shows that the hexagonal order was unstable for his six magnets. I had almost convinced myself before seeing the account of his experiments in NATURE, that the hexagonal order is stable for six equal vortices; and Mr. Mayer's last figure shows that with his magnets the hexagonal order is rendered stable by the addition of one in the centre . . .

The instability of the hexagon of six magnets shows the simple polygon to be unstable for seven or any other number exceeding six. Thus Mr. Mayer's beautiful experiment brings us very near an experimental solution of a problem which has for years been before me unsolved—of vital importance in the theory of vortex atoms —to find the greatest number of bars which a vortex mouse-mill can have.

WILLIAM THOMSON

[1] in the case of vortices or of the static problem when the law of the mutual repulsions is the inverse distance, but not with the law of repulsion with ordinary proportions of linear dimensions and magnetic distributions, in Mr. Mayer's magnetic arrangement.

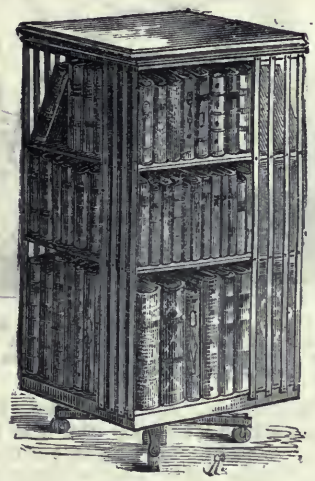
[2] In repetitions of Mr. Mayer's experiments, I have always found this configuration unstable, and for four only the square stable.

[3] This configuration of the floating magnets I have found stable, but with less wide limits of stability than the pentagon.

[4] I have not found this, nor any other configuration than the pentagon with centre, stable for six floating magnets.

A ROTATING BOOK-CASE

WE have received from Messrs. Trübner and Co. a book-case on a novel principle, the invention of an American, the introduction of which, into local museums would, we believe, be attended with considerable advantages. The accompanying woodcut will render a detailed description of it unnecessary, while the practical advantages of storing books in such a small space will be obvious to everybody. We draw attention to it not so much from its use in a private library, to the owners of which it will at once commend itself, as from the convenient manner in which books and specimens supplementing each other, may be arranged in close proximity. We believe that if in a museum geological books, for instance, were thus arranged



by the side of the geological specimens, references to the former would be more often made than they are at present when they have to be consulted often in another room at a considerable distance away. Of course the more valuable books and the special memoirs should find their place in libraries as they exist at present, but a typical collection of books side by side with a typical collection of specimens such as this neat arrangement suggests would, we believe, form a novel feature which would be greatly appreciated by many students. The four sides of the rotating book-case also afford a capital means of dividing the subjects as well as saving space, almost to as great an extent as the rotating case for art illustrations introduced by Sir Henry Cole into the South Kensington Museum.

FAUSTINUS JOVITA MARIANUS MALAGUTI

WE are called upon to chronicle a new loss to French science in the death, on April 24, of the well-known chemist Prof. Malaguti. He was born in Bologna February 15, 1802, his father being a pharmaceutical chemist. At the age of sixteen he completed the course in pharmacy at the Bologna University, and undertook the direction of his father's establishment. Although holding himself aloof from political questions, he became unintentionally involved in the complications of 1831, and was forced to leave his native land. He arrived at Paris unfamiliar with the French language, and succeeded in exciting the sympathies of Gay Lussac, who admitted him into his laboratory as assistant to Pelouze. After finishing a course in the École Polytechnique, he was appointed, in 1843, chemist to the porcelain manufactory at Sèvres. Soon after he received the degree of doctor of science, and in 1850, as the result of a competitive examination, was appointed to the chair of chemistry in the scientific faculty of Rennes, a position which he has since then occupied. In 1855 he was elected dean of the faculty.

As an investigator Malaguti won a prominent place in the annals of French chemistry, the period from 1833 to 1867 being especially fruitful. Devoting his attention to the theoretical dispute of the day, he laboured unweariedly in widening the region of experimental chemistry, showing himself equally at home, not only in the organic and inorganic departments, but also in technical and vegetable chemistry. In inorganic chemistry he devised methods for preparing a variety of metallic oxides, such as cuprous and chromic oxides, in the pure state, and investigated closely the properties of a number of salts and minerals. The extensive occurrence of silver in the blood, in sea-water, coal, salt, &c., as well as the most profitable means of winning the metal, formed subjects for an interesting series of papers in 1849.

In the province of organic chemistry, Malaguti rendered his chief services, his exhaustive studies into the principles of etherification and the action of chlorine on ethers and organic bodies in general being models of careful, thorough work. The enormous mass of facts which he gathered together contributed in no small degree to the establishment of the principle of substitution. Among other important researches in this department should be mentioned the discovery of methylal, the study on the action of acids on sugars, the papers on amides and nitrites, &c.

The influence of soils on the composition of plants was investigated by Malaguti in the most thorough manner, and led to the general conclusion that the constitution of the soil affected the ashy constituents of the plants, but not their physical properties. Equally important was the series of experiments on the division of inorganic matter among the various plant families, on the temperature of the soil and the air, and on the action of various compounds on living plants.

Besides these more purely scientific labours he accomplished no small amount in analytical chemistry, and made some valuable discoveries in practical metallurgy.

In addition to his numerous contributions to the periodical scientific literature, Malaguti published in 1848 "Leçons de Chimie agricole," and in 1853 "Leçons élémentaire de Chimie," a text-book highly regarded in France, and recently honoured with a new edition.

In 1855 he was elected a corresponding member of the French Academy of Science, and in 1860 he was appointed an officer of the Legion of Honour. The Academy of Turin, the London Chemical Society, and several other learned societies, numbered him among their honorary members.

T. H. N.

DR. THOMAS THOMSON, F.R.S.

A SERIOUS and protracted illness had removed Dr. Thomson,—who departed this life on Thursday, April 18,—so long from any very steady participation in the progress of botanical research that, except to those old friends who cherish the memory of his more active days, his name has of late been little before the public. Few, however, have done more permanent work in descriptive botany, in which department of science his information was not only extensive, but unusually accurate, while the range of his acquirements in other branches of natural science was distinguished by a correctness of judgment which, added to no ordinary amount of general knowledge in matters of taste and literature, made him a most delightful and instructive companion. Like many others who have acquired a permanent name in science, he had the advantage of being trained under a scientific father, while his intimate acquaintance with Sir W. J. Hooker, was no small advantage, and not less the having as the constant companion of his youth, Sir Joseph Hooker.

On his entering as assistant-surgeon in India, a great field was open to him, of which he happily availed himself. After a participation in some of the miseries of the Cabul campaign, though not actually serving in the expedition, and a narrow escape, in company with Lady Sale, of endless captivity, he was able to devote his time very much to science. He was employed in 1847 and 1848 in the Tibet Mission, a winter residence at Iskardo, a perilous journey along the portion of the Indus which runs beyond Iskardo, though, from the state of the country, he could not pursue its course to Kashmir, and the results of the previous journey gave him the opportunity of publishing a most instructive volume which, for soundness and multiplicity of information can scarcely be surpassed.

Dr. Thomson joined his friend Dr. Hooker in Darjeling in the end of 1849, after the completion of his arduous journeys in the North-West Himalaya and Tibet, and they spent the rest of the year 1850 in travelling and collecting, returning to England together in 1851. Having obtained permission from the Indian Government to distribute his botanical collections, which were equal in extent and value to those of Dr. Hooker, after taking part in the preparation of the Indian Flora, he returned to India as Director of the Botanical Garden at Calcutta. On his return to England, increasing infirmity soon made him unequal to any constant participation in the work, but up to a very few weeks before his death he was employed as examiner, his qualifications for which made him a most desirable and efficient colleague. Though in a very failing state of health, he collected last summer assiduously in the neighbourhood of Pitlochrie, and was so fortunate after three times ascending the Sow of Atholl as to rediscover the long-lost *Menziesia cœrulea*. It remains only to add that his kind and affectionate disposition endeared him to all who knew him, and to none more than to the writer of this short and imperfect notice.

M. J. BERKELEY

THE GREENLAND ESKIMO

A COMMISSION was appointed by the Anthropological Society of Paris to examine the Eskimo whom M. Geoffroy St. Hilaire, the intelligent director of the Jardin d'Acclimatation has brought from Greenland. This Commission was composed of MM. Broca, Dally, Girard de Rialle, Topinard, Masard, and Bordier (*rapporteur*). The following are the details which I have given to the Society as the result of the observations made by the Commission.

The Greenlanders, whom all Paris has been to see at the Jardin d'Acclimatation, are six in number, viz., Okabak, thirty-six years; Majak, Okabak's wife, twenty-three years; their two daughters, Anna, twenty-five, and Catarina, thirteen, months; Kojank, twenty-three years; and Jokkik, forty-one years, who is recognised at once as a half-breed between Dane and Greenlander.

These Greenlanders came from Jacobshavn, on Baffin's Bay, on the west coast of Greenland, about 69° N. lat., not far from Disco Bay and Island. In that latitude the temperature in winter falls as low as − 49° C. It differs notably from that which has to be endured by other Eskimo whose habitat extends to the south of Greenland, from Labrador to Behring Strait.

Jacobshavn, although belonging to the north district of Greenland, is not, however, the most northern town; for Bessels has given, as the human habitat nearest to the pole, the town of Ita, in 78° 16′ N.; Ita appears, however, to be only a summer station. At Disco Bay the sun does not rise from November 30 to January 15. It may not be useless to give a rapid glance at the surroundings in the midst of which these Greenlanders live.

The flora is rudimentary. The Greenlanders have but little wood at their command; the little they use is imported from Denmark.

The fauna, less poor, is composed, first of all, of the seal, which constitutes the prime material of all their

FIG. 1.—1 and 2. Toy dog and seal, cut in wood. 3. Knife to scrape fat off seal-skins. 4. Seal-skin hunting girdle with ivory medallion. 5. Seal-skin pouch. 6. Obsidian lamp. 7. Bone fish-hooks with iron points. 8. Tuft for catching vermin.

civilisation—food, light, heat, clothing, boat-building, various implements and utensils—the seal furnishes all. The white bear is sought for its fur, but the flesh seems to be reserved for the dogs. The reindeer is also found in Greenland. According to Dr. Hayes the reindeer is still very abundant in the interior of the land, but the Greenlanders do not make use of it either as food or as a means of locomotion. Birds are very abundant; their plumage is used as fur, and their sinews as thread.

FIG 2.—1. Fur glove with bear claws. 2. Bone knife for cleaning boats. 3. Drinking-spoon. 4. Bone table-spoon. 5. Bone boxes with bundles of thread made of birds' entrails. 6. Bone hook with iron points.

But the domestic animal is the dog, which they yoke to sledges by means of a small harness of sealskin. The nine dogs which the Eskimo have brought to Paris, and

harness to their sledge, are very large. Their white hair, spotted with black and red, is long and abundant; their ears are erect, head large, their iris of the colour of *café au lait*.

Let us, however, examine the more immediate environment of the Eskimo—their house. It is composed of a hillock of turfed earth, of square form, recalling somewhat our military fortifications. It is entered by a low door giving access to a narrow and very low passage, in which the Greenlander himself, notwithstanding his small size, is forced to bend down. The single apartment to which this passage gives access, and the floor of which is lower than the surrounding ground, is ventilated by an orifice in the upper part. It is lighted by two openings on each side of the door, and hermetically closed by strips sewn together of a sort of goldbeater's skin made of the intestines of the seal. This kind of immovable glazing sifts into the apartment a sufficient light, but appears from without altogether opaque. The furniture consists of a sort of camp-bed which occupies the entire half of the apartment, provided with sealskins, and on which the whole family pass the night, after having taken off their day costume, and put on another more ample dress. On the ground a stone basin, said to be of serpentine, the form of which resembles that of a fish, is filled with seal oil, in which are steeped several wicks. The flame which rises from this vessel

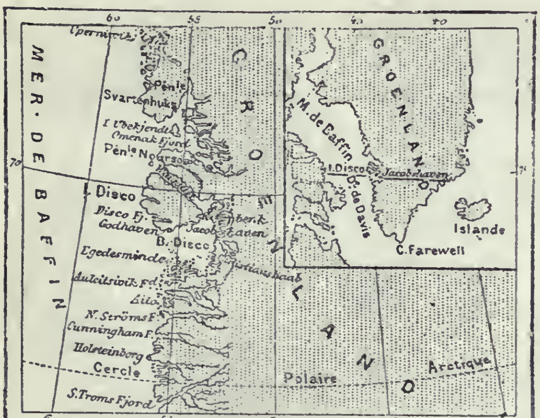


FIG. 3—Map of Greenland.

gives a sufficient light, and maintains the confined space at a high temperature. The cotton wicks come from Denmark, as also the chemical matches which the Greenlanders constantly use to light their briar-root pipes, which, with their tobacco, their alcohol, and their coffee, are sent them each year by the Danes.

Their costume is made almost entirely of sealskin. It consists, in the case of the men, of a shirt (Danish), above which is placed a woollen vest. The pataloons are of hairy sealskin; the boots, under the pantaloons, of sealskin leather. Gloves of fur, armed, when necessary, with bear's claws, blue spectacles—against the wind and the reflection from the snow—complete the accoutrement. The costume of the women is not wanting in elegance. The hair is raised *à la Chinoise* on the top of the head, and bound into a sort of vertical chignon, tied by a coloured knot. A well-fitting blouse of European material trimmed with fur, is provided with a hood, in which the mother carries, when necessary, her latest born, as the opossum does her young. The woman wears very tight breeches of sealskin and high boots reaching above the knees; red, embroidered with yellow, after marriage; white, embroidered with green, among unmarried girls.

Their arms consist of bows with which they shoot arrows pointed with bone or iron, and similarly made harpoons, which they throw from the hand. When the harpoon is to be thrown into the water it is attached to a cord provided at the other end with an inflated seal-bladder which acts as a buoy and prevents the loss of the wounded animal, which would run away into deep water with the harpoon. Their other apparatus are iron fish-hooks, wooden baits representing fish, coloured, and very well imitated. To these we may add cases of skin which they put on the paws of the dogs when the cold is very intense; leathern muzzles to put over the snout of the dogs, smoothing-irons of *stone*, knives identical with those which iron-tanners use to dress skins, and intended for the same purpose. This will give an idea of all that the Greenlanders have to help them to struggle against the inclemency of their native climate.

But an element not less important than the house in the idea of an Eskimo is the boat. The boats, all of sealskin well stretched over a framework of wood or bone, are furnished with tackle of leather. They are of two kinds; every man possesses a small boat, the *Kayak*, a boat decked all over, except in the middle, where it is pierced with a circular opening, into which the fisher insinuates and fits himself, the legs extended under the deck, and where he remains hermetically enveloped around the loins by what looks like the upper part of a leathern bag fixed to the edge of the hole and attached round the waist. Thus united to his boat, the Eskimo manœuvering his double-bladed paddle produces an impression analogous to that which gave origin to the legend of the Centaurs. A large sealskin bottle placed behind the fisher—a sort of swimming-bladder—increases the specific lightness, and renders the whole unsinkable. The other boat, very much larger—the *Umyak*—is used only by the women, who manage it with the children and furniture.

Before concluding what relates to the surroundings, one word about the alimentation. The word *Eskimo* is not the name which they give to themselves. They call themselves *Innuit* (the men); so true is it that under all climates human vanity prevails (*Los Ombres: Innuit*). The name *Eskimo* (eater of raw fish) is a malevolent nickname given them by their American neighbours. It is not, however, so well merited now as it was last century, at the time when Crantz observed them. They continue, nevertheless, to eat raw the lard sent them from Denmark and also the lines of the seal. The rest is eaten cooked. This custom of eating raw lard gives rise to the frequency of tapeworm in Greenland.

What has been said of their voracity still appears not to have been exaggerated. Like all peoples whose *pabulum vitæ* is uncertain, they go two or three days, especially during winter, without food, but on the first favourable occasion they exhibit a gluttony which is not always a compensation.

Phthisis is extremely frequent; it produces about three-fourths of the total mortality, and is almost always characterised by blood-spitting.

If we seek for what relates to intellectual phenomena, we find little artistic sentiment, but great accuracy, and an easy submission to what has come to them from Europe. Converted to Protestantism by the Moravian Brothers, they read in the Jardin d'Acclimatation a Greenlandish translation of the Bible, which appears sufficient to satisfy their literary aspirations. They sing slowly psalms which their ministers have taught them. Their writing, in Roman characters, is neat, correct, and precise; it has something of the slowness of their movements.

An extreme precocity of development seems to characterise them. Thus, the young Catharine Okabak, who was born on October 20, 1876, possessed, on October 20, 1877, four canine teeth, eight incisors, four premolars, in all sixteen teeth. She ran alone and commenced to speak at the age of ten months. Her sister Anna, who

is twenty-five months old, has twenty teeth. It is true that this precocity corresponds, as is often the case, to a feeble longevity. The *metis* of forty-one years appears already old, and it is generally acknowledged that in Greenland a man of fifty-five, or a woman of sixty years, is an exception. Young girls are quite formed at from fourteen to fifteen years; suckling continues for four or five years.

Their height is small; their black hair is straight; the face broad and flat; the head is dolichocephalic; the cheeks are large, plump, and round; the lips are thick, the lower pendent; the eyes are small, dark, oblique, like those of the Chinese, and connected by a fold of skin at the level of the internal angle. The teeth are large, yellow, but sound; the canines a little projecting. The beard is feebly developed, as is indeed the hair in other parts of the body; the skin is brown and even black among the aged. The following figures give a more precise idea of these peculiarities:—

Their mean height is 1·46 metres, a figure higher than that which is given by Hearn to the Eskimo (1·299 m. for the men and 1·271 m. for the women), but inferior to that of MM. Bellebor and Guerault (1·50 m.); a figure which places them in the category of small-sized men, *i.e.*, under 1·60 m., and between the Veddas (1·535 according to Bailey) and the Negritos (1·478 m.), after whom we find only the Bushmen.

The cephalic index places these Eskimo among the dolichocephalic, one only, the woman, among the sub-dolichocephalic. Their mean index is 73·51 (maximum 76·88, and minimum 70·95). The mean index of Bessels was more dolichocephalic still; it was 71·37. It should not be forgotten, however, that the figure varies with the locality of the Eskimo. The Eastern Greenlanders of

FIG. 4.—The Eskimo in the Jardin d'Acclimatation.

Davis gave a cephalic index of 71, the Western, 72; those of Virchow, 71·8; those of Pansh, 72.

It is necessary to compare the anterior or intellectual part of the cranium with the posterior part. The results deserve to be given one by one. In Okabak, the total horizontal curve of the cranium being supposed equal to 100, the anterior curve will be represented by 45·2; in Kojanki by 48·5; in the woman by 48·1; lastly in the half breed by 44·7; I do not speak of the children. It is worthy of remark that this numerical classification corresponds exactly with that which each of us had made in estimating the intelligence of each of these subjects. Kojanki and Mrs. Okabak were judged superior to the two others. The mean of the anterior cranium is 46·6, which places the Eskimo in what Gratiolet designates the occipital races—those among whom the posterior cranium outweighs the anterior cranium. The mean facial angle is 66·7°. The height of the nose being supposed equal to 100, the mean breadth will be 70·5, while among the Cochin-Chinese it is 89, and among the negroes 110, 112, and even 115. One index gives very well the measure of the small prominence or flatness of the nose; this is the antero-posterior nasal index. Among our Eskimo, the breadth of the nose at its base being supposed equal to 100, the mean prominence of that base in front will be 55·5, while the same mean index among Europeans is 66·6.

Such are the principal facts I have been able to collect in reference to the Eskimo of Jacobshavn, now in Paris; they may be defective, because based on a very small number of subjects; nevertheless, it is to be wished that the director of the Jardin d'Acclimatation will continue the experiment so well begun, and that besides the Nubians and Eskimo he will introduce other representatives of races so interesting and so little known, whom civilisation will, so far as science is concerned, cause to disappear sooner or later.

A. BORDIER

POZZOLANA MORTAR AND PINE TIMBER

THE following letter has been sent us for publication by Prof. Tyndall:—

Villa Guastalla, Via Palestro, Rome, April 14

SIR,—A very curious and unexpected circumstance has occurred in Rome, which, as it depends on chemical action, may have some interest for you.

Prior to 1870, when Rome became an integral part of the kingdom of Italy, the beams used in the construction of houses were of chestnut wood. After that date a vast amount of building was undertaken and now a whole quarter of the city stands on ground formerly occupied by vineyards and gardens.

In lieu of chestnut, pine was largely used, having been brought *viâ* Venice from the Dolomite Alps. The latter was preferable, as being procurable of larger scantling, of greater length and at a less cost.

After a few years, the roofs and floors in which the pine had been used were found to be failing. A beam used in a flat roof or in flooring, where it was imbedded in a wall was found to be rotten; while the body of the beam was perfectly sound. A very considerable sum of money was thus lost, as many of the roofs and floors of the new houses on the Esquiline had to be renewed.

But what was the cause of this sudden perishing of the ends of the pine beams, such as had been known to last centuries in Venice? The answer to this question remained a puzzle for a long time; until on taking down the scaffolding of the Ministry of Finance lately completed, a complete answer was found.

One of its scaffold poles had been imbedded for, say, four feet in the ground; about its foot was a heap of the *débris* of Pozzolana mortar, say, six feet high. That part which had been underground was perfectly sound; that which had been surrounded by mortar was utterly rotten; and finally, the remainder of the pole above the ground was perfectly sound. Hence, it was clear that the mortar was to blame. But in what respect did this mortar differ from that used at Venice in which pine wood beams lay embedded for centuries with impunity? The sole difference was in the use of pozzolana—a volcanic earth—instead of sand, and as this substance had been used for mortar in Rome and Naples for ages in contact with chestnut beams with impunity, the only logical conclusion is that pozzolana and pine wood have some chemical affinity which causes some of their ingredients to combine, to the destruction of the latter.

Inclosed are a few grains of pozzolana, such as is used for mortar in Rome.

Yours faithfully,
HENRY H. MAXWELL, Lieut.-General R.A.

Dr. Tyndall

STANFORD'S STEREOGRAPHICAL MAP OF THE BRITISH ISLES

MR. STANFORD has recently issued a map which marks a distinct advance in British cartography, and one which gives us ground for hoping that some day we may be able to equal in this country the work of the geographical establishments of Germany. The map in question represents, in the first place, the United Kingdom, with its hills and mountains standing solidly out from the ground, as if a perfect relief model of the country lit up from the North had been photographed. The plains and valleys are also clearly shown; on ordinary maps these cannot be distinguished, and yet they are as important features as the hills themselves.

Great care has been taken to embody all the usual information without in any way detracting from the beauty of the map. Thus the railways are shown, and cities and towns, so as not to interfere with the physical features, as well as the hills and plains, vales and rivers, are named in a clear yet delicate type.

As an example of the information conveyed, we can mark in the map how the ground rises gradually in going west from London all the way to the summit ridge of the Chiltern Hills, and then falls suddenly to the Vale of Aylesbury and the Vale of the White Horse; the ground again rising gradually to the summit of the Cotswold Hills, and then falling suddenly to the valley of the Severn; how the headwaters of the Thames all lie on the top of the second ridge, while the first ridge is the boundary between the Upper and Lower Thames Valley, presenting only one vulnerable point, between Walsingham and Reading, through which the river can make its way.

Mr. Stanford claims that the map is at the same time artistic and scientifically accurate; and from the examination we have made we believe both claims can be well made out.

OUR ASTRONOMICAL COLUMN

TRANSITS OF MERCURY.—After the transit of Mercury across the sun's disc on Monday next, May 6, which will be visible in this country through about half its duration, there remains only one transit of this planet at the descending node in the present century; it will take place on May 10, 1891, with the following elements according to Leverrier's tables of sun and planet:—

G.M.T. of conjunction in R.A. 1891, May 9, at 15h. 55m. 40s.

	°	′	″
R.A.	46	44	14·1
Sun's hourly motion in R.A.		2	26·2
Planet's ,, ,, ...	−	1	18·7
Sun's declination	+17	32	1·9
Planet's ,, ...	+17	18	0·4
Sun's hourly motion in decl.	+	0	39·6
Planet's ,, ,, ...	−	1	6·7
Sun's horizontal parallax			8·76
Planet's ,, ,, ...			15·92
Sun's semi-diameter		15	50·33
Planet's ,, ...			6·01

Whence the first external geocentric contact occurs at 11h. 53m. 19s. at 65° from the sun's north point towards the west, for the inverted image, and the last external contact at 16h. 52m. 18s. at 12° from the north point towards the east. At Greenwich the external contact at ingress takes place at 4h. 50m. 26s. A.M. on May 10, and the sun's centre is in the horizon at 4h. 18·5m., so that Mercury will be only half an hour upon his disc, after observation is possible here. And while the egress of the planet from the solar disc is alone visible in these islands in the transit of 1891, in that of November 10, 1894, at the opposite node—the last phenomenon of the kind in the nineteenth century—the ingress only can be witnessed here, under favourable atmospheric circumstances, not to be insured at this season; the first external contact at Greenwich taking place at 3h. 55m. P.M. and the sun setting at 4h. 18m.

At the sitting of the Paris Academy of Sciences on April 22, a letter from M. André was read, stating that the expedition sent by the Academy and the French Minister of Public Instruction, to Ogden in the Utah territory for the Observation of the Transit of Mercury in the present month, had arrived safely at its destination. After experiencing very liberal treatment from the French Trans-Atlantic Company, the instruments were admitted without payment of duty at New York, and the observers received free passes on the lines of railway converging in Utah, both for the outward and homeward journeys. The Government of Washington placed at their disposal the nearly-finished observatory at Ogden, at the same time undertaking to provide gratuitously all necessary appliances for the observations. A telegraphic wire from Washington to Utah was available

for determination of time, and the authorities of the U. S. Naval Observatory confided to the expedition the photographic instruments which had been employed by the American parties on the occasion of the late Transit of Venus, for comparison with those brought from France. M. Sainte-Claire Deville in communicating these particulars to the Paris Academy, adds—"Il suffit de publier tous ces détails pour que la gratitude de tous les savants soit acquise à de pareils actes de confraternité scientifique."

KEPLER'S MANUSCRIPTS AND RELICS.—In the last Annual Report of the Director of the Imperial Observatory at Pulkowa, M. Otto Struve, to the Visiting Committee, attention is called to an interesting acquisition recently made by this great astronomical establishment. It is known that the library possesses in addition to all the notable published works of Kepler, the nearly complete collection of his manuscripts. This circumstance caused Prof. Galle, of the Observatory at Breslau, to inform M. Struve that certain articles of which the last direct descendants of Kepler, resident in Silesia, were in possession, and which had been religiously preserved in the family as memorials of their immortal ancestor, might be obtained by purchase, and the result has been that they are now deposited at Pulkowa, to be preserved with other astronomical treasures, which the Struves, father and son, have secured for the institution. Amongst these articles are particularly mentioned two miniature portraits on copper of Kepler and his first wife, at the time of their marriage, and a memorandum-book used by his first wife and continued by his eldest daughter.

THE PULKOWA LIBRARY CATALOGUE.—In the same Report from the Director of the Russian Observatory, it is mentioned that a continuation of the Catalogue of the valuable library has been some time in preparation, the numerous additions, upwards of 10,000, which have been made to it since the publication of the first Catalogue in 1860, rendering a more complete work very desirable. M. Otto Struve justly remarks that the Catalogue of 1860 has had its uses beyond the pale of the establishment, and we feel sure that workers in almost every branch of astronomy will bear witness to the assistance they have received from that excellent and well-arranged analysis of the contents of this important library, whereby they will have been guided with comparative facility to a knowledge of the literature special to particular astronomical subjects upon which they have been engaged.

GEOGRAPHICAL NOTES

ROUND THE WORLD.—The French Société des Voyages Autour du Monde, have obtained the steamer *Picardie*, of the company Valery frère et fils, of Marseilles, in which to make their intended voyage round the world. The vessel is 1,560 tons and 1,000 horse-power, and is fitted up in the best manner. She is announced to leave Marseilles on June 30 under the command of Lieut. M. G. Biard. The staff is complete, and it is stated that the passenger list will shortly be closed. This project seems likely to have a better result than the much-talked-of American Woodruff Continental Voyage Round the World, which from the first seems to have been utterly hollow, and collapsed on being probed.

AFRICA.—In his recent journey in East Central Africa, the late Capt. Elton, H.M.'s Consul at Mozambique, paid considerable attention to the northern end of Lake Nyassa, which was previously very imperfectly known. He arrived, we believe, at a very positive conclusion that no river flowed out of the lake, but he discovered an important and navigable affluent, the Rombashi River. This he considered to be well suited for the lake end of the caravan road from the coast. This road, which is being constructed by private enterprise and under the supervision of English engineers, starts from Dar-es-Salam, some twenty miles to the south of Zanzibar, and thirty or forty miles of it have already been completed. When finished it will, no doubt, have an important bearing on the future of this part of Africa, and it will open up to commerce and civilisation a region a considerable portion of which has remained hitherto entirely unexplored.

The Abbé Debaize, who recently received a subvention of 100,000 francs from the French Government for purposes of African exploration, left Marseilles on April 20 for Zanzibar, where he will arrive at the end of May. He will remain there for some time in order to make the most complete preparations for his journey across the Continent, which is expected to occupy three years. The same steamer carried nine French missionaries despatched to establish posts at the Victoria Nyanza and Lake Tanganyika.

NOTES

PROF. HUGHES, the well-known inventor of the type-printing apparatus so largely employed on the Continent, has made the wonderful discovery that some bodies are sensitive to sound as selenium is sensitive to light. If such a body be placed in the circuit of a small battery it will be so affected by the sonorous vibrations when spoken to as to replace entirely the transmitter of a Bell telephone. Conversation, music, and all the sounds transmitted by an ordinary telephone are easily reproduced. A mere scratch with the finger-nail, or a touch with the soft part of a feather is distinctly transmitted. The sonorous vibrations produce strains in the conductor, which cause variations in the resistance of the circuit, and thereby produce similar variations in a current flowing through that conductor.

THE French deserve all the praise that has been recently lavished upon them for the energy and determination and sound judgment with which they have quietly carried on the preparations tht culminated in the imposing ceremony of yesterday. Their new Exhibition is the one bright spot in the European horizon at present. Even till very recently many doubted whether these preparations would ever come to anything, partly on account of the disturbed state of Europe, and partly because the earnestness and perseverance of the French as a people were doubted. We have had frequent occasion recently to bring before our readers evidences of the renewed energy of the French in respect of scientific research; and the unprecedentedly magnificent display which now divides the attention of the world with the Eastern crisis, is only one of many other proofs that the French are rapidly achieving for themselves a position more solid than ever they held before. The world, then, is once more taking stock of her industrial riches, and ever since the Exhibitions of Vienna and Philadelphia, the discoveries and applications of science have been so many and so rapid that the Paris Exhibition must present many new features. For, indeed, however much the great mass of visitors may ignore it, the multitudinous display that was opened yesterday, is simply a specimen of the gifts of science to humanity, as the French themselves would say. Judging from the catalogues British trade is well represented, and our principal scientific-instrument makers are well to the front; but British culture and British science are nowhere, and, as we have said already, the British Commissioners have lost a splendid opportunity, and will have simply nothing to show beside the magnificent educational and scientific collections of France herself. We have already spoken at length of the many preparations made for representation of French science—scientific conferences, the scientific lectures, scientific excursions, besides the great display of

scientific exhibits; the British Department will be nothing more than a trade show. Let us hope that the British Commissioners and British visitors generally will return from Paris ashamed of their shoppy display, and filled with a sense of the vast national importance of science, which in the case of France, it will be seen, truly "exalteth a nation."

THE large fresh-water and salt-water aquariums in the Trocadero Gardens at the Paris Exhibition were stocked last week. A regular service of barges is engaged in bringing daily quantities of sea-water from the coast to supply the second aquarium.

THE amount proposed to be spent upon the building of the new Natural History Museum at South Kensington for the present financial year (1878–79) is, according to the Civil Service Estimates, 80,000*l.*, being 10,000*l.* more than last year. Of this sum, 60,000*l.* is for the building, which is now verging towards completion, and 20,000*l.* for internal fittings. We are pleased to see that the authorities are already turning their attention to the last subject, but should they not also begin to think about a *library?* As regards scientific work, the natural history collections in their new house will be absolutely *useless* without a library. Our readers may possibly think that a scientific library may be got any year by the use of a certain quantity of money, but they will find themselves very much disappointed when they attempt to try this experiment. The fact is, such a library as is required for the use of a great national museum can only be picked up by slow degrees, and so soon as it was determined to move the collections away from the great public library in Great Russell Street, steps should have been taken to form a new one for the collections in their new site. This, however, does not appear to have been thought of yet.

OUR readers will be glad to hear that Prof. Clifford, who is at present at Gibraltar, is somewhat better.

THERE was a *conversazione* at the Royal Society last evening.

THE Vice-Chancellor of Cambridge University has appointed Prof. Clerk Maxwell Rede Lecturer for the ensuing year.

AN agreeable variation on the daily news from Constantinople is the report of the completion of the Museum of Antiquities in the Turkish capital. In 1875 Arifi Pacha, the Minister of Instruction, ordered the renovation for this purpose of an old kiosk on the Seraglio Point, built in 1471 by the conqueror of Constantinople, and the work has been pushed steadily forward, even despite the war, until now a spacious edifice, richly decorated with marble, is ready to receive the archæological collection of the city. Visitors at Constantinople who have found their way to the dark, dusty hall in the arsenal, where quantities of valuable antiquities were crowded together in chaotic confusion, will appreciate the value of this ample provision for their exhibition, especially for the extensive collections resulting from Schliemann's excavations at Troy. A school of archæology is to be established in connection with the museum.

WE have variety enough of Associations, learned and otherwise, in this country, but none corresponding to that which met on April 24 and subsequent days at the Sorbonne, composed of the delegates of the various learned societies throughout France, and founded by Leverrier many years ago. We have the elements for such an association in abundance; and, indeed, concretions of greater or less extent have begun to form in different parts of the country. There is, for example, the Cumberland Association, which met last week, and which, if not founded by our national astronomer, like the French Association, had the honour of listening to what he describes as probably his last public lecture. Then there is that extensive association of societies and field-clubs in Yorkshire, which publishes a journal of its own; and most recent of all, there is the Midland Union, with head-quarters at Birmingham, extensive ramifications, and "running" an excellent magazine, the *Midland Naturalist.* But there is room for something more national and more universal than any of these, and not interfering with their action at all; and as a preliminary step we would suggest that a general meeting of delegates from the various local societies throughout the kingdom should be held at some central city. Such a meeting might be useful in many ways, leading as it might do to united action with regard to common interests, as useful, indeed, in respect to our local societies, as the recent Conference of Librarians has been to the libraries of the world. If properly organised we believe the meeting would become an annual institution.

THE President of this year's meeting, the sixteenth, of the French Learned Societies, was M. Milne-Edwards, who devoted his opening address mainly to the memory of the Association's founder, Leverrier. The number of delegates was smaller than in former years, many of them having postponed their visit to Paris till the Exhibition was opened, and the discussions seem to have lacked the keenness and impressiveness which always characterised them when Leverrier presided. The first two days were devoted to sectional meetings, and on the concluding day the distribution of prizes took place, as usual under the chairmanship of the Minister of Public Instruction. An immense crowd had been attracted in the hope of hearing from M. Bardoux himself what was the intention of the Government with regard to education; but he postponed any definite statement to the month of October, when the association will hold a supplementary meeting after having taken part in the several scientific congresses and lectures held at the Trocadero. He reviewed all the improvements realised last year in the educational system of France. "Soon," he said, "everywhere when the want will become manifest, libraries, laboratories, and collections will be established exhibiting the passionate zeal of Government for everything which touches the superior interest of instruction. A time will soon arrive when every hamlet will have its own school and when the tools of intellectual work will be at the disposal of every seeker." There can be no doubt of the sympathy of the present French Government for every form of scientific effort. Some important scientific papers were read during the meeting, but we cannot at present do more than mention the fact. In the scientific section gold medals were assigned to M. Cailletet for the liquefaction of gases, Dr. Armand for explorations in Cambodia and Laos, General de Nansouty, founder of the Observatory on Pic du Midi; Prof. Terquem for physical researches, and Prof. Houel for mathematical works.

ALTHOUGH no allusion was made by M. Bardoux in his address at the Sorbonne to the contemplated improvements meditated for French meteorology, we can state that he will ask from the French Parliament a credit of 10,000*l.*, and 2,000*l.* for five successive years, in order to organise in France ten large meteorological observatories, possessing each a complete set of registering instruments. The contemplated institutions, some of which have been already created, will be located at Lille, Paris (Montsouris), where M. Marie Davy will be continued superintendent, at the country seat of Mr. Hervé-Mangon in the department of La Manche, where a private observatory has already been organised, at Bordeaux, Toulouse, Marseilles, Lyons, Besançon, and the three elevated observatories, Pic-du-Midi, Puy-de-Dôme, and Mont Ventoux.

THE Annual Meeting of the Cumberland Association for the Advancement of Literature and Science may now be regarded

as an established institution. The gathering at Cockermouth on Monday and Tuesday last week was large and successful. The event of the meeting was no doubt Sir George Airy's Address, which we hope to give next week, but there were other addresses and papers read which would do credit to more pretentious associations. The president, Mr. Isaac Fletcher, M.P., F.R.S., in his address, gave an interesting sketch of George Graham, the eminent horologist of the eighteenth century, who was a Cumberland man. Mr. Clifton Ward, to whom the success of this Association is largely due, read a valuable paper on Quartz in the Lake District. The telephone of course was exhibited, and several interesting excursions made; and last, not least, the Report tells us that the Association and its affiliated Societies are prosperous. Why should not each county or group of counties, have a similar association? The Midland Union of Natural History Societies, numbering over 2,000 members, are to have their meeting at Birmingham on May 27 and 28; and judging from the brief programme it promises to be an interesting one. With independent sources of many-sided and vigorous activity in the country like Birmingham, there is no danger of over-centralisation.

AN alarming paragraph recently appeared in the Swiss correspondence of a German paper, which, affecting as it does the existence of the St. Gothard tunnel, we are surprised that it has not been even referred to in English journals. The paragraph stated that the great engineering undertaking of boring through the St. Gothard was threatened by the possibility of a severe check in a direction hitherto unexpected. "The geologists engaged in the work," it was stated, "have lately noticed a peculiar depression of the strata through which the tunnel is progressing, leading to the suspicion that a subterranean sea occupies the interior of the mountain chain at this point. The last report laid before the Swiss Federal Council, states that these indications are becoming more and more decided, and it is expected that the next 700 feet of boring will yield decisive proofs for or against this theory. If the fears prove true, the whole of the work on this magnificent undertaking, will come to an abrupt and unfortunate conclusion." These sentences partake of the usual character of what may be called "newspaper science." They contain just enough of scientific phraseology to impress the ordinary reading public with the importance of their announcement; while at the same time their statements are so vague as to afford the reader who knows something of the subject no means of deciding whether the thing is a hoax or may have some kind of foundation in fact. Happily the apparently insuperable difficulty has been boldly faced with the usual results. A recent report of the inspector of the tunnel states that the irregular character of the formations pierced by the tunnel, which led to the above fears, has entirely ceased, and that the work is now progressing through uniform regular strata. On the south side the boring progresses at the rate of ten feet daily through gneiss. The rate is somewhat less on the north side, where the tunnel is not yet out of the serpentine. The thickness of this stratum of serpentine now being pierced is already the double of that estimated by geologists from the surface indications.

THE forty-ninth anniversary meeting of the Zoological Society was held on Monday. The report of the Council stated that the number of fellows, fellows-elect, and annual subscribers, at the close of the year 1877 had amounted to 3,358, showing a net addition to the list of 47 members during the year 1877. The income of the Society in 1877 had amounted to 30,988*l.*, being, with the exception of 1876, a larger total than the receipts of any previous year since the foundation of the Society. The total ordinary expenditure of the Society in 1877 had been 27,290*l.*, the remaining sum of 1,711*l.* having been devoted to certain special objects, such as new buildings. The Society has purchased the freehold of the present house (11, Hanover Square), and of the house immediately adjoining it at the back (314½, Oxford Street). The total assets of the Society on December 31, 1877, had been calculated to be 17,989*l.*, while the liabilities were reckoned at 4,019*l.* The total number of visitors to the Society's gardens during the year 1877 had been, according to the report, 781,377, a number greater than had been recorded in any previous year except in 1876. With regard to the state of the menagerie, it was stated that the total number of animals belonging to the first three classes of vertebrates living in the Society's menagerie at the close of 1877 had been 2,200. The total number of registered additions to the menagerie in 1877 had been 1,260. The Marquis of Tweeddale, F.R.S., was re-elected president; Mr. Robert Drummond, treasurer; and Mr. Philip L. Sclater, Ph.D., F.R.S., secretary to the Society for the ensuing year. The new members of the Council elected were—Sir Joseph Fayrer, K.C.S.I., F.R.S., Lieut.-Col. Godwin-Austen, Dr. Günther, F.R.S., Dr. Edward Hamilton, and Prof. Huxley, F.R.S.

DR. F. V. HAYDEN sends us a first proof of a plate to appear in one of the volumes of the Bulletin of the U.S. Geological Survey, in which is represented the greater part of a fossil skeleton of a very remarkable new bird about to be described by Mr. Allen under the name *Palæospiza bella*.

THOUGH we have not heard of or from Mr. Benson for some time, he has not been idle. Two papers by him are now before us. In one of these ("Facts and Figures for Mathematicians; or, the Geometrical Problems which Benson's Geometry Alone can Solve") the problem is, "given the area of a circle, say of one acre, to find that of another circle, which being described from a point as centre, on the circumference of the given circle, shall have that portion of its area outside the given circle equal to the area of the given circle." A similar problem to this vexed us in our undergraduate days. We were required to find by purely geometrical means (if possible) the length of a chain which, fastened to a stake in the boundary of a circular field, would allow an ass to graze over just half the field. Mr. Benson says the solution depends upon the *actual*, not the supposititious properties of the circle, and therefore the result as given in the *Scientific American* (where the ratio of 1 to 1·158728 is stated to be the one required) "which is based upon the false supposition that the circle has similar properties to those of the polygon" is erroneous. It may be remembered that Mr. Benson will have it that the value $3\cdot1415926 \times R^2$ for the area of a circle is wrong. As we stated in our notice of the "Geometry," our author maintains that the reasoning mathematicians employ to get this result is fallacious, and in his opinion he makes this easily evident. He still holds that $3R^2$ is the area.

"A man convinced against his will
Is of the same opinion still."

Mr. Benson argues *more suo* in the twenty-two pages which he devotes to the problem. The second publication ("New Mathematical Discoveries") is a four-page one, and is concerned with the discovery of Archimedes that the proportion between the parabolæ and the rectangle on abscissa and ordinate is in the proportion of 2 : 3. From the proof employed to show this, he comes round to the circle again, and gets area $= 3R^2$. To judge by the printed letters, Mr. Benson has adherents to his views; among them one a graduate of the Polytechnic School in Paris, writes that "they (these discoveries) will revolutionise the mathematical world," and he is translating them for publication in France. Mr. Benson (whose motto should be "indefessus agendo") is engaged upon "Philosophic Thoughts in all Ages" and "Geometer's Manual," containing history of geometry and correspondence

with prominent English and American mathematicians on new geometrical subjects. Our author has a mission; if any hold with him, they should write to L. S. Benson, 149, Grand Street, New York City, and become the happy possessors of a copy of "Facts" for thirty cents. *De gustibus non disputandum.*

THE *North China Herald* reports a curious desire for improvement on the part of two Corean medical men, who belong to a nation which has hitherto shown itself the most determined in its self-isolation. These men have applied to Dr. Dudgeon, the Superintendent of the London Mission Hospital, for permission to attend there during the stay of the Corean embassy at Peking. They are described as very intelligent men, and they speak very disparagingly of their own medicine. For years they have been studying Hobson's medical works in Chinese, and they have also obtained Dr. Dudgeon's Anatomical Atlas. They are greatly interested in vaccination, and wish to introduce it into Corea. The stringency of Corean laws prevents natives from living out of their own country, but the next time the embassy visits Peking these two men intend to devote more time to the study of foreign medicine and surgery.

ALTHOUGH the existence of kerosene oil in several of the provinces of Japan is said to have been known for 1,200 years, the Japanese did not know how to refine it till about six years ago. Now, however, refining establishments are springing up rapidly, and its manufacture is becoming an important industry.

AT Dresden a new journal appeared on May 1 entitled *Zeitschrift für Museologie und verwandte Wissenschaften*; the editor is the Director of the celebrated "Grüne Gewölbe," Hofrath Dr. Grässe, the publisher, Herr T. M. Hofmann. Thus the circle of "collection-journals," *i.e.* journals for archives, libraries, and museums, is complete.

A GERMAN inventor has found a new use for asbestos, in the shape of leaves for a bank-note-album. These albums are said to protect bank-notes or other valuable documents to such an extent, that if they are laid between the leaves and the album is closed firmly, they even remain legible after being burnt to cinders.

MR. F. C. PENROSE writes to the *Times* from Copse Hill, Wimbledon, that on April 24, at 8.12 P.M., he saw an unusually fine meteor descending at a very steep angle, and when first noticed, at about 2° to the north of the bright star Procyon, and sloping a little to the north. It was yellowish, and although not in itself intensely bright, from its apparent size (5′ long and 3′ broad by estimation), surpassed the light of Venus at her maximum. It was as usual pear-shaped. After a course of about 10° from the point first mentioned, it left behind it three or four very bright blue star-like points, and vanished in a clear sky at about an altitude of 22° and 57° west of south. No sound of explosion was heard.

A PERUVIAN chemist, Dr. Arosemano, will exhibit an invention at the Paris Exhibition, which may become a very important one for commerce. He has succeeded in obtaining a magnificent dye from the violet or maroon Welshcorn of Peru, and this dye is said to impart the colour, odour, and taste of claret to all light white wines to such a degree, that it is impossible to distinguish the coloured wine from real claret, without being in the least injurious to the health of the consumer. Besides this a number of other uses are mentioned to which this Welsh corn-dye can be put.

THE German Telegraph Office is rapidly introducing the telephone; 68 stations are already provided with this instrument, 41 others will have it in a few weeks, and 111 more before the end of the year; thus there will be then a total of 220 telephone-stations in Germany.

To commemorate the 100th anniversary of the discovery of the Sandwich Islands by Cook, a statue of the great discoverer will be erected on Diamond Peak, a burnt-out crater near Honolulu.

SEVEN extremely interesting pictures are now being exhibited at Berlin by the painter, Herr J. L. Wensel; they represent scenes from the second German North Polar Expedition during the years 1869 and 1870, and are executed after sketches made on the spot by several members on the staff of the expedition.

THE Conference on the National Water Supply, in connection with the Society of Arts, will meet on the 21st and 22nd inst., and will be followed on the 23rd and 24th by a Conference on the Health and Sewage of Towns.

THE additions to the Zoological Society's Gardens during the past week include a Beisa Antelope (*Oryx beisa*) from North-East Africa, presented by H.H. the Sultan of Zanzibar; an African Leopard (*Felis pardus*) from Africa, presented by Mrs. Kirk; a Black Wallaby (*Halmaturus ualabatus*), a Laughing Kingfisher (*Dacelo gigantea*) from Australia, presented by Mr. D. W. Barker, jun.; a Sand Lizard (*Lacerta agilis*), a Smooth Newt (*Triton tæniatus*), European, presented by the Masters W. L. and B. L. Sclater; a Common Seal (*Phoca vitulina*) from British seas, a Cariama (*Cariama cristata*) from Brazil; a Guira Cuckoo (*Guira piririgua*) from Para; a Crested Curassow (*Crax alector*) from Guiana, a Bar-headed Goose (*Anser indicus*) from India, a White-faced Tree Duck (*Dendrocygna viduata*) from Brazil, a Red-billed Tree Duck (*Dendrocygna autumnalis*) from America, a Blue-bonnet Parrakeet (*Psephotus hæmatogaster*) from Australia, purchased; a Bennett's Wallaby (*Halmaturus bennetti*), born in the Gardens.

THE UNIVERSITY OF OXFORD COMMISSION

THE Vice-Chancellor has received from the University of Oxford Commissioners a Statement with respect to the main purposes relative to the University, for which, in their opinion, provision should be made under the Act, the sources from which funds for those purposes should be obtained, and the principles on which payments from the colleges should be contributed. The statement is somewhat similar to that published in reference to Cambridge some weeks since, only more detailed.

As to the main purposes relative to the University for which provision should be made under the Act, the first in order of these purposes is, in their opinion, the extension and proper endowment of the professoriate, and the better organisation of University teaching. As to which two principal objects should be kept in view:—1. The due representation at Oxford of every considerable branch of knowledge, the advancement of which can be effectually promoted by the University, as a place either of education or of learning and research; and 2. The due participation of the University itself, as distinct from its colleges in the direction and improvement of the studies of its undergraduate and other students.

The Commissioners are unable to adopt the views of those who would desire to transfer to the University the whole or the chief part of the teaching work now done by the colleges either separately or by means of intercollegiate arrangements. They think that among the recognised studies of the University there are some (such as natural science) for which the colleges cannot be expected to make adequate provision, either without, or by means of, those intercollegiate arrangements.

Many of the existing professorships are inadequately endowed, and ought to have their emoluments increased. Of a few the emoluments are in excess of what we think necessary. There are others the constitution, designation, and duties of which may, when they become vacant, be advantageously modified. The Commissioners also think that some new chairs should be established and adequately endowed.

The stipends of the professors (other than those of the theological faculty) should, in the Commissioners' opinion, be of varying amounts, according to the relation of their several

subjects to the studies of the University and to other circumstances material to be considered. Those of the following, among other Chairs should, they think, be augmented, so that the lowest of them should be not less than 700*l.* nor the highest more than 900*l.* per annum—namely, Astronomy; Geometry; Natural Philosophy; Chemistry.

They would also assign stipends, varying between the same limits, to the following Chairs, constituted by division or modification of existing foundations:—Physics—dividing between these two Chairs the subjects of the present Chair of Experimental Philosophy; Physiology; Human and Comparative Anatomy—dividing between those two Chairs the subjects of the present Linacre Professorship.

Stipends varying between the same limits should also be assigned to the following new Chairs, which they think ought to be established—English Language and Literature; Pure Mathematics; Mechanics and Engineering.

The stipends of the following Chairs should, they think, be augmented, so that the lowest of them should not be less than 400*l.*, nor the highest more than 500*l.* per annum—Medicine; Botany; Zoology; Geology; Mineralogy.

The evidence and opinions which the Commissioners have received lead them to the conclusion that it is expedient to develop as much as possible those branches of scientific instruction which are introductory and preliminary to medicine, rather than to attempt the establishment of a practical School of Medicine in Oxford.

It may be desirable to provide a reader in Human Anatomy, as assistant to the Professor of Human and Comparative Anatomy, with a stipend of from 250*l.* to 300*l.* per annum; and they think there should also be a reader (with a present stipend of 400*l.* per annum) in Invertebrate Anatomy, whose office, upon a vacancy in the Professorship of Zoology, should be united to that Chair, with such an increase in the emoluments of the professor as may make them equal to those of the Chair of Human and Comparative Anatomy, conditionally on his undertaking the additional duty.

Additional demonstrators appear to be required in several departments of natural science, who, in most cases may best be paid by fees, with supplementary grants when needful from the University chest.

There are several other purposes relative to the University which they regard as important, and for some of which definite provision ought to be made under the Act. Among these are:—

The foundation and endowment of scholarships or exhibitions tenable after a certain fixed period of residence in the University, for students in any special branches of study (including subjects which do not fall within the ordinary University course, such, for example, as medicine), which may be usefully promoted by such encouragement, under conditions properly adapted to make their enjoyment dependent upon the *bonâ fide* prosecution of such studies.

The encouragement of research, by the employment of properly qualified persons, under the direction of some University authority, in doing some definite work, or conducting some prescribed course of investigation, in any branch of literature or science; or by offering prizes or rewards for any such work or investigation.

The appointment and remuneration, from time to time, by the University authorities, of extraordinary professors or occasional lecturers in any subjects, either represented or not on the ordinary teaching staff of the University.

The last, and not the least important, of the main purposes relative to the University for which, in the Commissioners' opinion, provision should be made under the Act, is the creation of a common University fund, to be administered under the supervision of the University, in addition to its general corporate revenues.

They look to the creation of this fund (of which the formation must be gradual) as the proper resource for the supply of all the wants enumerated under the preceding head, except such of them as any college may propose to aid in supplying.

As to the sources from which funds for the above purposes should be obtained, they are of opinion that these funds must necessarily be obtained from the colleges.

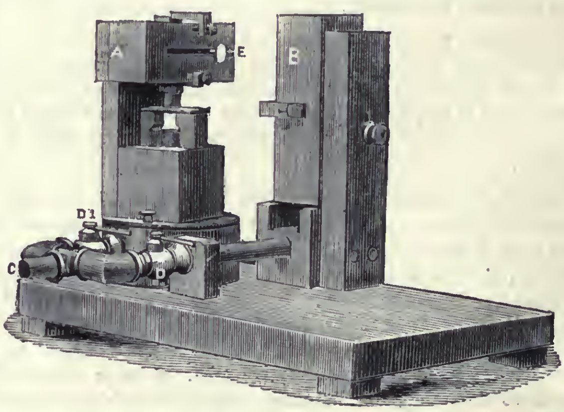
As to the principles on which payments by the colleges for the above-mentioned purposes should be contributed, it will be necessary to take into account the revenues, actual and prospective, of each college, and its actual and prospective wants for educational and other purposes, before they can form a judgment as to the amount which it should be called upon to contribute. They think it expedient to retain in Oxford a considerable number of prize fellowships (that is, fellowships not coupled with any specific duty or service to a college or to the University), for the encouragement and reward of meritorious students. Such fellowships should, they think, as a rule, be terminable; and their present impression is that their emoluments should be of uniform amount and should not exceed 200*l.* per annum.

The Commissioners have already received from some of the colleges proposals made, in a liberal spirit, in harmony with the views which they have expressed; and they are confident that they will receive such assistance from the University and the colleges generally, as may be necessary to enable them to determine when, and in what order of priority, provision shall be made for all the purposes specified in the first part of this statement.

AN IMPROVED METHOD OF PROJECTING LISSAJOUS' FIGURES ON THE SCREEN[1]

As is well known, the vibrations of tuning-forks when used for the production of Lissajous' figures, are kept up either by the constant application of the violin bow, or by the aid of an electro-magnet; the former method requiring the presence of two assistants, and the latter adding materially to the complexity of the apparatus, and not unfrequently failing to produce the desired result. The difficulty is overcome in the present apparatus by the substitution of harmonium reeds for the tuning forks, the entire instrument being easily controlled by one operator.

The apparatus consists of a base board on which are planted the two reed boxes A and B. The box A is placed horizontally in such a manner as to be capable of slight rotation in the horizontal plane, and also of adjustment in height, by means of the support to which it is attached being provided with a slot and set screw. The box B is permanently attached to the base board in the vertical position. The boxes are so placed that a pencil of light falling directly on E would be reflected to B about one inch from its top; they are furnished with clamping screws



for the attachment of the reeds. The boxes are entirely open on the sides facing each other, their margins being covered with soft leather on which the reed plates bed, making a sufficiently air-tight joint. Wind is supplied through the brass tube C which gives off a branch to each box, a stopcock DD′ being inserted in each branch. The reeds are similar to those used in the construction of harmoniums; they are mounted on brass plates which fit the reed boxes. The tongue of each reed is furnished, at its free end, with a small reflector of microscopic covering-glass (E) silvered by Liebig's process, a piece of cork or pith being interposed between the tongue and the reflector, so as to free the latter from the frame of the reed; the reeds are then tuned in the usual manner. It is not necessary that the reeds should be in absolute tune, as, within certain limits, their relative vibrations can be adjusted by means of the stop-cocks, an advantage of great value, believed to be solely possessed by this apparatus.

The reed in the vertical box gives the fundamental ratio of vibrations from which the intervals are built up. Two fundamental reeds are used interchangeably, one giving the double or

[1] Paper read at Lit. and Phil. Soc., Manchester, February 5, by J. Dixon Mann, L.K.Q.C.P.]

eight feet C of musicians, the other, being an octave lower in pitch, adds an octave to the intervals obtained from the first fundamental; thus, the third with the first fundamental becomes the tenth with the sub-fundamental. The horizontal box is furnished with a set of reeds giving all the intervals up to the twelfth, including the unison. The horizontal reeds are changed for the production of the different figures, the fundamental reed being retained.

A free space of half an inch was allowed between the supply pipe and the reed box, so as to afford a cushion of air capable of yielding to the elasticity of the tongue. The supply pipe is contracted at its termination to about one-third the size of the hole in the reed box through which the wind enters.

The apparatus is used as follows:—The base board being firmly clamped to a rigid table, one of the fundamental reeds is clamped in front of the box B: another reed, giving the desired interval, is similarly clamped to the box B; an elastic tube, about half an inch in diameter, is attached at one end to the pipe C, and at the other to an acoustic bellows. A fine pencil of light is now thrown on the mirror E, which is then adjusted by rotation of the box A until the light strikes the mirror of the vertical reed, from whence it is reflected on to a screen of tracing paper placed a short distance away; a condenser, interposed between the lantern and the mirror E, focuses the spot of light on the screen. On the bellows being put in motion the figure appears, and can be brought to a perfect stand in any phase of development, looped or cusped, by careful manipulation of one or other of the cocks. The entire apparatus should be as rigid as possible, and free from any vibration other than that produced by the tongues of the reeds, and also that the wind supply should be perfectly steady.

THE PARIS OBSERVATORY

As we announced some time ago, an important step has been taken for the reorganisation of the Paris Observatory. A decree of the President of the French Republic has appointed ten members of the new council of the Observatory, in pursuance of the provisions of the organic decree we referred to two months ago. The principal object of this new institution being to connect the observatory with the several large French administrations, three Government departments have sent two delegates each. The War Office is represented by Col. Laussedat, the director of the balloon service, and Commander Perrier, the chief of the Ordnance Survey; the Minister of Marine by two rear-admirals, one of them, M. Jurier de la Gravière, being a member of the late Council; the other is M. Clouet; the department of agriculture by M. Tisserand, Director of the National School of Agriculture, and M. Hervé Mangon, a member of the Institute and president of the Meteorological Society of France. The Academy of Sciences is represented by four members, carefully selected, viz., M. Dumas, the perpetual secretary, who is to be appointed president, and M. Liouville, the celebrated geometer, and two astronomers, M. Faye and M. Mouchez, both of them members of the Section of Astronomy. It must be noted that the Council of the Observatory, although vested with the right to present to the minister two candidates for the directorship of that establishment, are not to interfere with the solution of technical questions. A special council composed of all the astronomers *en titre* of the Observatory, are to meet once a month to solve them with the director of the Observatory. The first meeting of the Council of the Observatory took place on April 24, M. Dumas being in the chair. The members had been summoned in order to send to the Ministry a list of two candidates for the direction rendered vacant by the demise of M. Leverrier. The meeting was very short, and the members having been unable to agree, it was postponed to the 26th, when M. Faye wished to deliberate on the vexed question of the separation of meteorology and astronomy. This, however, was not allowed, when M. Faye protested and declared that he would bring the question before the Academy of Sciences at its next meeting, on April 29. After several scrutinies the Council decided to send in the names of MM. Mouchez, Loewy, and Tisserand as their nominees for the directorship of the Observatory, the last two having obtained an equal number of votes. Such was the result of the deliberations of the Observatory Council, which on the whole seem to have been conducted with becoming dignity. At Monday's sitting of the Academy, M. Dumas simply read M. Bardoux's letter, and summoned a meeting for to-day of a Committee of the Academy composed of all the sections in the mathematical sciences. A list of candidates will then be formed for proposal to the whole Academy, which will vote its candidates on May 5. It then remains with the Government to choose between the candidates proposed by the Council and Academy. M. Faye made no protest at the Academy meeting on Monday, though, our correspondent writes, he was expected to speak on the subject in a secret committee which met after the meeting of the Academy. We trust that throughout these important steps for the appointment of a successor to Leverrier all personal feelings will be suppressed, and the interests of the Observatory and of science alone considered.

SOCIETIES AND ACADEMIES

London

Royal Society, February 7.—"On the Diurnal Range of the Magnetic Declination as recorded at the Trevandrum Observatory," by Balfour Stewart, LL.D., F.R.S., Professor of Natural Philosophy at Owens College, Manchester.

The Observatory at Trevandrum was supported by His Highness the Rajah of Travancore, and its director was Mr. J. A. Broun, F.R.S., who has recently published the first volume of the results of his labours, giving the individual observations of magnetic declination, and deducing from them conclusions of great scientific value.

Amongst the other results published by Mr. Broun, are the diurnal ranges of the magnetic declination at Trevandrum for each civil day in the eleven years, 1854 to 1864.

In one respect the treatment of the declination observations at Trevandrum differs from that pursued at the Kew Observatory, inasmuch as in the former place, where disturbances are little felt, the diurnal ranges are from all the observations.

Variations of Long Period.

In order to investigate the long-period variation of the Trevandrum declination-range, I have treated these observations precisely in the way in which the Kew declination-ranges were treated (*Proc. Roy. Soc.*, March 22, 1877). By this method proportional values of the declination-range at Trevandrum have been obtained corresponding to weekly points for each year, and it is believed that these values are freed from any recognised inequality depending either on the month of the year or on the relative position of the sun and moon. If this method should be found to furnish nearly the same results in the case of two observatories so widely apart as Kew and Trevandrum, and with such marked differences in the annual variation of the declination-range, we may conclude that this separation of inequalities has been successfully accomplished.

The proportional numbers have next been dealt with precisely in the way in which the corresponding numbers were dealt with in the case of the Kew Observatory, that is to say, a set of nine-monthly values of declination-range have been obtained corresponding to similar nine-monthly values of spotted solar area.

The results of this treatment are exhibited in the diagram which accompanies this paper.

In Fig. 1 we have a curve representing the nine-monthly values of spotted area.

In Fig. 2 we have the Kew and in Fig. 3 the Trevandrum declination curve represented by nine-monthly values of the proportional numbers.

In Fig. 4 we have a curve representing the mean between the proportional numbers of Kew and those of Trevandrum.

From these figures it will be seen that a lagging behind the sun is a feature both of the Kew and the Trevandrum curves, while generally the prominent points in the Kew and Trevandrum curves agree well together in point of time.

On the whole it would appear that by taking the mean of the proportional numbers for the two stations, we get a curve that represents the solar curve better than one derived from a single station.

The whole period compared together represents both for the solar curve (Fig. 1) and the mean curve (Fig. 4), a series of three smaller periods, one extending from B to C and embracing the maximum; another extending from C to *c*, and a third from *c* to *e*; and this is as far as the observations common to both stations allow us to go in point of time.

It may be of interest to compare, by means of the tables, the period between the solar minimum of 1855 and that of 1867, with the period between the corresponding declination-range minima. The first of these declination minima occurred at Trevandrum (the

Kew observations not having then begun) on February 15, 1856, and the second of them occurred at Kew (the Trevandrum observations having been discontinued) on August 15, 1867. The period is thus one of eleven years and six months.

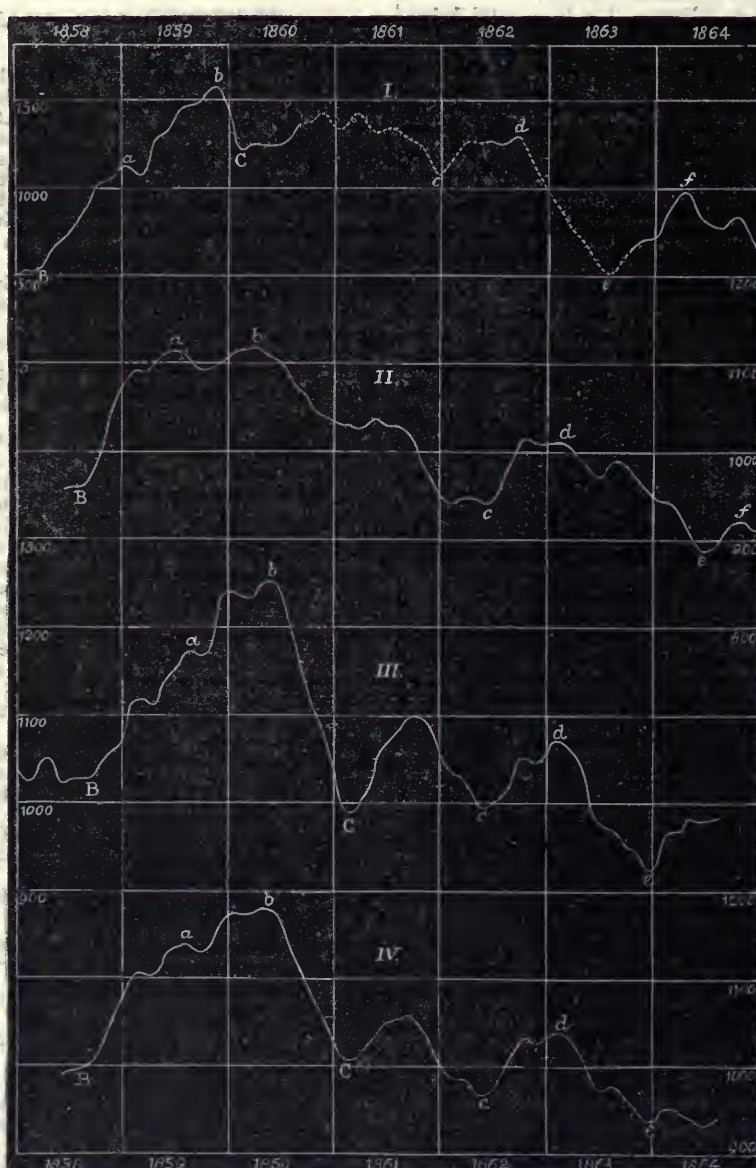
On the other hand, the sun-spot period is that between September 15, 1855, and March 15, 1867, being likewise eleven years and six months.

d. Variations which seem to depend on Planetary Configurations.

In a paper on the Kew declination-range already alluded to, it was shown that the planetary periods of most frequent occurrence appear to be well indicated by the results of sixteen years' observations. Indeed, for the two periods of shortest length—that of Mercury about the sun, and that of Mercury and Jupiter, it was found that half of the observations gave a result of the same character as the whole sixteen years.

From this we might conclude that these periods will probably (if they have a real existence) be indicated by the Trevandrum observations.

It will be seen from the following tables that the Trevandrum declination-ranges give results for these two planetary periods very similar to those given by the Kew observations.



Period of Mercury about the Sun.

(0° denoting Perihelion—65 sets for Kew—47 for Trevandrum.)

				Kew.	Trevandrum.
Between	0°	and	30°	+429	+263
,,	30	,,	60	+433	+223
,,	60	,,	90	+256	+237
,,	90	,,	120	+ 5	+300
,,	120	,,	150	−280	+150
Between	150°	and	180°	−439	−433
,,	180	,,	210	−413	−879
,,	210	,,	240	−279	−740
,,	240	,,	270	−140	−263
,,	270	,,	300	+ 13	+333
,,	300	,,	330	+158	+680
,,	330	,,	360	+278	+506

Period of Conjunction of Mercury and Jupiter.

(0° denoting Conjunction—63 sets for Kew—43 sets for Trevandrum.)

				Kew.	Trevandrum.
Between	0°	and	30°	+633	+453
,,	30	,,	60	+759	+270
,,	60	,,	90	+652	+129
,,	90	,,	120	+328	−118
,,	120	,,	150	−119	−384
,,	150	,,	180	−504	−467
,,	180	,,	210	−678	−487
,,	210	,,	240	−677	−407
,,	240	,,	270	−548	−122
,,	270	,,	300	−322	+223
,,	300	,,	330	− 10	+415
,,	330	,,	360	+343	+503

Zoological Society, April 16.—E. W. H. Holdsworth, F.Z.S., in the chair.—Mr. Sclater exhibited and made remarks on a typical specimen of the new fox lately described by Mr. Blanford as *Vulpes canus*, from Baluchistan.—The Secretary exhibited, on behalf of Mr. A. Anderson, F.Z.S., a bamboo stick with leather thong attached to it, such as is used in India for driving plough-cattle with, which had been taken out of a nest of the common Fish Eagle (*Haliaetus leucoryphus*), in December, 1876.—Prof. Westwood communicated a memoir on the Uraniidæ, a family of lepidopterous insects, with a synopsis of the family and a monograph of one of the genera, *Coronidia*. These insects were remarkable for their extreme beauty and the difficulty which had attended their systematic classification. Their relations with other groups of lepidopterous insects were discussed at considerable length, and their nearest affinities were shown to be with certain other moths belonging to the great division of the Bombyces, whilst their connection with the Hesperian butterflies, the Pseudo-sphinges, Erebideous Noctæ and Ourapterygeous Geometre was disproved by their general structure, the venation of their wings and their transformations. A synopsis of the species of all the genera was given, and a complete monograph with figures of the genus *Coronidia*.—Mr. Gwyn Jeffreys, F.R.S., F.Z.S., read the first part of his work on the Mollusca, procured in the expeditions of H.M. S.S. *Lightning* and *Porcupine*. It would be recollected that these expeditions immediately preceded that of H.M.S. *Challenger*, but were restricted to portions of the North Atlantic, including the Mediterranean. The Brachiopods formed the subject of the present paper. A table of all the Brachiopods known to inhabit the European seas was given, comprising ten genera and twenty-two species, of which latter four were for the first time described and six figured. The table also particularised the geological and bathymetrical range of all the species. Two plates accompanied the paper, and were furnished by Mr. Davidson.—Mr. G. E. Loder, F.Z.S., exhibited and made remarks on a mounted head of the Rocky Mountain Bison, remarkable for its soft, dark, and long hair on the forehead. This specimen had been obtained near Denver, Colorado.—A communication was read from the Marquis of Tweeddale, F.R.S., containing the eighth of his contributions to the ornithology of the Philippines. The present paper gave an account of some Luzon birds in the Museum at Darmstadt, which had been sent to him for examination by Prof. Koch of that place.—A communication was read from Dr. O. Finsch, C.M.Z.S., containing description of a new species of finch from the Feejee Islands, which he proposed to name *Amblynura kleinschmidti* after Mr. Kleinschmidt, by whom it had been found in the interior of Viti-Levu.—Dr. M. Watson read a paper containing a description of the generative organs of the male spotted hyæna (*Hyæna crocuta*), and a detailed comparison of them with those of the female of the same animal.—Messrs. Sclater and Salvin read a report on the collection of birds made during the voyage of H.M.S. *Challenger* at the Island of Juan Fernandez, at various points along the coast of Patagonia, and at the Falkland Islands.—A second paper by Messrs. Sclater and Salvin gave descriptions of three new species of birds from Ecuador, proposed to be called *Buarremon leucopis*, *Neomorphus radiolosus*, and *Aramides calopterus*.

WELLINGTON, N.Z.

Philosophical Society, August 18, 1877.—After the confirmation of the previous meeting's minutes, and the announcement of Mr. B. T. Chaytor and Mr. Robert Govett as newly elected members, the President (Mr. W. T. L. Travers, F.L.S., M.H.R.), read his paper on remarks as to the cause of the warmer climate which existed in high northern latitudes during former geological periods. This paper was a review of the progress recently made in our knowledge of the subject, and especially the bearing of Naysmith and Carpenter's examination of the moon's surface, and the work by Mr. Mathieu Williams on the "Fuel of the Sun." The author adopted the view that the gradual condensation of water on the earth's surface, consequent on the loss of its original cosmical heat, had produced the succession of phenomena resulting in the present distribution of life; that in consequence of the cooling having taken place first in the polar regions, it was there that the higher and latest-formed organisms must have first appeared. He adduced as proof of this the existence of fossilised vegetation within the Arctic regions which had almost a tropical character, and other evidence that during successive geological epochs the changing character of the fauna and flora in other regions showed that the climate had gradually become more and more temperate. Dr. Hector would only speak as regards the geological aspect of the author's paper. The fact that the oldest rocks we know are either hydrated or formed by the action of water as sediments proved that our geological records did not carry us back to a time when very high temperature prevailed. It was only, therefore, necessary to inquire into the evidence of a minute secular cooling afforded by the succession and distribution of animals and plants during former epochs. He considered this evidence very unsatisfactory, and not leading in the direction the author required. The former existence of temperate plants in high latitudes took place at a very late period in the earth's history, and long after some temperate regions had possessed a fauna and flora similar to that at the present time. There had, in fact, been several repetitions of the abnormal distribution of animals and plants on which the author founded his argument, and consequently of the climate; so that these changes could hardly be referred to the progressive cooling of the globe as a whole. The inferences made had chiefly been drawn from late tertiary strata, but in the case of New Zealand there was evidence that the same type of vegetation had survived since the early part of the cretaceous era, a period twenty times as great as that which had elapsed since the supposed sub-tropical fauna inhabited Central Europe, or the temperate flora flourished in the Arctic regions. From this it was surely to be argued that the cause had not been one ot universal operation. Concerning the former Arctic flora the real difficulty was not the question of temperature so much as the absence of light in that region for six months of the year if all other conditions of the earth remained as at present, except a general higher surface temperature. Many speculations had been put forward on this subject; one of the latest, by John Evans, was that the earth was solid with an oxydised crust, separated from the central nucleus by a viscous layer of unequal thickness in which chemical combination, or, as it may be called, the "rusting process," was still active. The elevation of mountain masses by the fracture of the crust would act like weights on a gyroscope and lead to a gradual displacement of the outer crust with reference to the axis of rotation of the interior bulk of the earth, which astronomers required us to believe to be immovable. He also pointed to recent researches of Prof. Duncan regarding reef-building corals, which at the present time are confined to a narrow equatorial belt, but in eocene times that belt appears to have had a distribution oblique to the present equator. If this were established it would offer a still greater difficulty in the way of accepting the view that the changes in distribution of climate were due to the secular cooling of the earth as a prime cause. Mr. Carruthers thought it not yet proved that there was a central heat, and certainly not that it could influence climate. He thought the balance of evidence was against the theory of central heat. If the earth had once been hotter it would have become smaller in cooling, and its velocity of rotation would have increased; but this was contrary to fact, as the rotation had been retarded by about three hours since exact observations were first made. With regard to what had been said about the thickness of the earth's crust, the existence of tides proved that it must be so great as to be absolutely rigid. He considered it quite possible for plants to live in darkness if they remained dormant, like geraniums, which are placed in a dark cellar during the winter.

September 1, 1877. — Mr. W. T. L. Travers, president, in the chair.—Mr. Coleman Phillips read his paper on a peculiar method of arrow propulsion as observed by the Maoris. The author gave an interesting description of how

the arrows were thrown by means of a string, which he illustrated before the meeting with a model. He expressed surprise that, as far as he was aware, nothing was known of the bow among the Maoris, a weapon so commonly used by natives of other islands. Mr. Grace, who had been in New Zealand from his youth, said that the bow and arrow was a common weapon in the interior with the Maori youths, and he believed that it was originally used by the natives. It was, however, found by them to be an inconvenient weapon in the bush, and hence their reason for adopting the plan mentioned by Mr. Phillips. The Maori scarcely ever threw a spear by hand; they used the string twisted round a fork in the spear. The notch mentioned by the author was new to him.—The President read a paper on grasses and fodder plants by Dr. Curl, being a continuation of a paper by the same author read last year, and printed in vol. ix. of the *Transactions.*—Mr. Carruthers read a paper on a system of weights and measures, in which it was proposed to change the radix of counting from 10 to 16, and to adopt the latter number as the radix for all weights and measures.

PHILADELPHIA

Academy of Natural Sciences, November 13.—The agricultural ants of Texas, by Rev. H. C. McCook.—On a stone axe, by Mr. J. Ford. This was found in a bluff fifty feet above the level of the Mississippi, and embedded twenty feet deep in solid limestone, without fissure or crevice, giving evidence of great age.

November 27.—Remarks on American species of *Difflugia*, by Prof. Leidy.—On the aëronautic flight of spiders, by Rev. H. C. McCook.

VIENNA

Imperial Academy of Sciences, February 7.—"Monographia Pulmonariarum," by M. Kerner.—On bixin, by M. Etti.—A centrifugal air-ship, by MM. Szigyarto and Kuczera.—On the originals of v. Born's Testaceis Musei Cæsarei Vindobonensis (1780), found in the Imperial Zoological Museum, by M. Brauer.—On new neuroptera, by M. Steindachner.—On a peculiar spinal cord band in some reptilia and amphibia, by M. Berger.

February 14.—Construction of tangents at the contact line of a rotation surface and the developables described outwards and round it from a point, by M. Drasch.—Completing additions to the general mode of determination of the focus of contours of surfaces of the second degree, by M. Pelz.—On the action of bromine on phenoldisulphoacid, by M. Schmidt.—On the products of decomposition of a gum-ammoniac of Morocco by melting hydrate of potash, by Dr. Goldschmidt.—Telephone signalling apparatus, by M. Puluj.

ROME

R. Accademia dei Lincei, February 3.—New researches on the ossiferous caves of Liguria, by MM. Gasteldi and Ferroti.—Discovery of arms of stone and bronze in Calabria, by M. Ruggeri.—Geological and palæontological studies on the middle cretaceus of Southern Italy, by M. Seguenza.—On benzylic santonate, and on tribenzylamine and its chloroplatinate, chlorhydrate, sulphate, alum, and nitrate, by M. Panebianco.—On the new anomalous anastomosis between the trochlean nerve, the supra-orbital and the sympathica cavernosa, by M. Berté.—New general theorem of mechanics, by M. Cerruti.

PARIS

Academy of Sciences, April 22.—M. Fizeau in the chair.—The following among other papers were read:—Researches relative to the action of dry oxalic acid on primary, secondary, and tertiary alcohols, by MM. Cahours and Demarçay. This is in completion of a former study (C. R. vol. lxxxiii. p. 688). The experiments were made with methylic alcohol, primary and secondary octylic alcohol, trimethyl-carbinol, and dimethyl ethyl-carbinol. The action of dry oxalic acid on tertiary alcohols, which consists in splitting them into hydrocarbons and water which unites with the acid, establishes a very marked distinction between them and primary and secondary alcohols, which, in like circumstances, are transformed always into oxalates.—Report on a memoir by Lieut. Pinheiro of the Brazilian navy, on a sondograph. This instrument is for giving information regarding banks in rivers. A wooden rod is fitted at its lower end with a hollow roller to roll on the bottom and collect small portions of the material; at the top it is articulated round a horizontal axis carrying a graduated arc (which shows the various inclinations) and also a toothed wheel, which, though a pinion and eccentric, gives a straight motion to a style, tracing a continuous curve on a moving band of paper.—M. Gaiffe exhibited a manometric safety steelyard. It is mounted on steel pivots, and connected with a piston having ten square millimetres of surface. It indicates with precision the pressure of the boiler. Annexed is an alarm whistle communicating with a valve box by a graduated rod.—A letter from M. André was read, announcing the arrival at Ogden, Utah, of the party sent out to observe the transit of Mercury. The U.S. Government had given them the use of the nearly finished observatory at Ogden, and any instruments they wished; the photographic instruments used by the American Venus transit expedition were put in their hands. A telegraph wire connects Ogden with Washington.—Observations of solar spots and protuberances during the first quarter of 1878, by M. Tacchini. The number of spots has continuously diminished since the beginning of last year, so that the minimum appears to fall, not in 1877, but in 1878. The protuberances, too, have been very few and small: 2·1 on an average daily, with a height of half a minute; they occupy only 3·5° of the solar limb. In distribution they extend over a large zone, but with the peculiarity of two characteristic maxima beyond the principal zones of spots, *i.e.* between 30° and 60° in both hemispheres. The nebulous structure predominated. There were no isolated metallic eruptions.—On observations of Mercury, made at the end of last century, by Vidal at Mirepoix, by M. Bigourdan. These are shown to be as accurate as was possible with the means at Vidal's disposal.—Results of experiments made at various points of Algeria, in industrial use of solar heat, by M. Mouchot. The reflectors he finds best are made of a plate of silver, or brass electro-plated with a thin layer of silver. At Algiers the heat received per minute by his solar boiler was 7 calories in April, 8 in May, and 8·5 in June and July. M. Mouchot tabulates the results obtained in various localities; they range from 9·8 cal. to 5.—On a large fossil reptile (*Eurysaurus raincourti*), by M. Gaudry. The remains of this were come upon by workmen in a quarry near Vesoul, as far back as 1861. A Dr. Gevrey happened to pass and brought some of the blocks of bone to Vesoul, where they have been forgotten seventeen years. The Marquis de Raincourt having seen them perceived their interesting nature. The remains are estimated to have covered a space five metres in length. The animal has affinities to the plesiosaurians, but it is not a true plesiosaurus, for its head is so heavy and its teeth are so large that it could not have had a very long neck. The cervical vertebræ, too, are narrower and convex behind. The cranium was flattened and the teeth were directed outwards. The nostrils must have been placed far back.

CONTENTS

THURSDAY, MAY 9, 1878

PHYSICAL SCIENCE FOR ARTISTS

I.

WE have it on the high authority of Lord Beaconsfield that the English School of Artists is arriving at a pitch of unexampled excellence, and that English art is in the future to be the cynosure of an admiring world.

It is Lord Beaconsfield's opinion that the time has arrived in which we may speak of a school which has flourished for a century with some accuracy of deduction as to its principal features. The principal features of the English school are, he thinks, now recognised. "All will admit that it is a school of great originality. All will admit that, in some provinces of painting, it has certainly established a reputation which may be rivalled by some nations, but which can be surpassed by none. Its power of portraiture is recognised in the most classic galleries. As far as landscape painting is concerned, it has achieved the highest aim of both branches of the art—whether ideal, like the enchanted castle of Claude Lorraine and the classic groves and solemn temples of Poussin, or whether it has competed with the freshness of Hobbema or Ruysdael, English art can match the *chefs d'œuvre* of every country."

This is high praise, and we may gather from it that so far as the reproduction of form and colour goes our artists have arrived at the highest knowledge and skill. Of late years also, we are told, the English school has given an indication of aiming at a higher range of imaginative composition than has hitherto prevailed; and this Lord Beaconsfield holds is natural, because if there is an imaginative nation in the world it is the English nation. "It is the nation that has produced the greatest number of poets—the greatest number of illustrious poets—and, therefore, the British artist has a heritage of imagination which ought to be to him a fund of inspiration." Nor is this all. "He has also another advantage which no great school has yet possessed—he has a larger range of subjects. What the pictures of antiquity were we know very little. We know very well that Zeuxis painted a curtain that deceived his patron, but if that were a test of his school it might, I believe, be stood by the commonest scene-painter of the nearest theatre. With regard to the Italian masters, we know their admirable works abound; they established, not one school, like England, but many schools. Those schools produced many pupils, and their prolific works charmed and instructed the world. But if you look to the great creations of the Italian schools, you will find, generally speaking, as far as subjects are concerned, their range was extremely limited. They drew their inspiration from two religions—the Christian and the Pagan; and every one must feel, when he examines a gallery of Italian art how much it is to be regretted that such genius and power should not have commemorated the great acts of their own history. . . ."

Under these circumstances Lord Beaconsfield takes a very favourable view of the English school. He believes that "there is a feeling which will not be satisfied in the works of art if art does not aim at the production of the highest modern style of imaginative creation." That our artists will shine here the noble speaker is convinced. "I rely on the fact that there never has been a limit to the increasing excellence of English achievement when a fair and just opportunity was offered to it; and, therefore, I do look forward to a period of which, I think, we have many symptoms and encouraging circumstances about us, when imaginative art will be characteristic of the English school, as well as that sense of humour and that *exquisite feeling of nature* and intellectual delineation of portraiture to which I have before referred."

The result predicted by Lord Beaconsfield is of course a consummation devoutly to be wished, and if it be true that Art is Nature passed through the alembic of Man, then this highest style of imaginative creation should largely increase the number of students of science in this country, because, although Lord Beaconsfield was careful not to say too much about Nature, she is there all the same, and the laws which underlie the phenomena which it is the function of art to embody should, at any rate, possess some interest to the artist, and if he is to surpass Nature, he must not hope to do this by evading her.

In art as in science, imagination must have a basis to work upon, and the surer the basis the more will the imaginative effort which transcends it be in sympathy with those hidden powers of the mind and those hidden feelings which it is the function of art to bring into play.

What little I know of the history and development of art would seem to show that in the early days at all events the artist was second to none in his appreciation of the science of the time. Geometry was rapidly applied to perspective, anatomy to form, and although the dwellers in Italy had the finest examples of ancient art to appeal to, it is not difficult to trace the rise of such men as Leonardo da Vinci and Michael Angelo to the direct influence of the study of anatomy first introduced at the University of Bologna. Da Vinci was, as is well known, almost as famous for his knowledge of science as for his productions in art. Indeed the anatomical studies carried on in the wonderful medical schools of Italy during the Middle Ages may be said to have left a greater mark on the world from an art point of view than they have done in the domain of the science of surgery.

Galileo, when he took so large a share in founding the physical science of to-day, was a student of medicine; the wonderfully regular swing of that famous lamp at Pisa suggested to him in the first instance a method of observing the flow of blood through the veins. The idea that here was a perfect method of dividing the flow of time—the idea of the pendulum clock—did not come till afterwards. Still the teaching of the medical school was no more to Galileo than it had previously been to Leonardo da Vinci or to Michael Angelo.

Now what is the condition of things to-day? We might be in the same position with regard to physical science—the science of colour—as Da Vinci and his contemporaries in the 15th century were with regard to biological science—the science of form. The whole range of physical science—a branch of knowledge which has existed for two-and-a-half centuries, but which has lately been developed enormously precisely in those directions of the greatest value to the artist, has not yet been annexed by the students of art.

So far as I can see there is not among artists gene-

rally—among those even who acknowledge their obligations to mathematical and biological science in the regions to which I have referred—the notion that they have anything to learn from physical science—physical science being reduced, at all events it will be convenient that in what I now say I shall take it as reduced, in the main, to optics. There seems to be a sort of notion that there are no laws underlying the phenomena of air, and sky, and sea; that while the shape of a horse's leg is defined by law, the order of colours, for instance, in a rainbow, depends upon the play of blind chance. Indeed I have been informed—and I may tell the story here because it hammers my point home better than anything I could say—that an eminent artist, now living, who had painted a rainbow practically inside out, when the picture was returned to him in order that the colours might be corrected, was so indignant with this attempt to interfere with this special development of the "highest style of imaginative creation," to use Lord Beaconsfield's words, that he charged the trifle of 20*l.* for attempting to place the colours in the order in which monotonous nature perversely insists they shall stand.

This is a general attitude, not only of artists, but of would-be teachers of art, and these latter piteously make tempting officers of the whole range of theology for science to work her wicked will upon, if only art may be spared from her contaminating touch. This is not, however, the universal attitude, as I can abundantly testify. Some of our modern painters do most enthusiastically enter into the study of physical science not only for its own sake, but in order to embrace it in their art. It has been my great privilege during the last few years to discuss with painters of the highest eminence questions bearing on art which have arisen from my own investigations in another region of work, and in the study of which the works and observational powers of the artist have been of the greatest value to me.

It is as a result of these many conversations that I have determined to put on paper a sketch of some of the many points in which I think the interest of the operation of nature's laws is as great from an artistic as from a scientific point of view. I shall, I hope, be able to throw these notes into order, but I shall content myself at first with giving an idea of the result of such studies upon art criticism. Whole reaches of art will remain untouched by physics, and its influence will be chiefly felt by the landscape-painter. It is only those who are ignorant of the development of art who will look with suspicion upon the new tests of truth with which artists can supply themselves—with the new ways of tracing effects to causes. Art criticism must gain considerably, for in place of jargon we may in time find common sense, and when once this basis is secured then the more secure will be the "highest style of imaginative creation" resting upon it.

I shall best indicate what I believe will be the influence of the study of optics in the future on art, by stating, by way of introduction, in its most naked form the result of an appeal to the newest branch of knowledge as a test of the truth to nature of several of the pictures in this year's Academy.

The recent results obtained by the workers in spectrum analysis have added so much to our former knowledge of the actions which go on when light is given out, or reflected, or absorbed, that almost all the optics the painter really requires conveniently lies round the most recent work in molecular physics, for the reason that it is the action of molecules which builds up the world with which the artist has to deal.

The instance I shall take in this paper is the following one. One of the smallest of the developments of the new branch of optics supplies us with facts which can be embodied in a simple working hypothesis. The approximate truth of this can be brought to the test by the various colours of the sky. When I say "working hypothesis," I use a term well known to men of science to indicate a train of thought to work upon and test. It is a first approximation to a general grouping of many facts, and it is perhaps as much generated by imagination as by work. It is not a hypothesis in the ordinary sense of the word, because it has not borne sufficient tests, and it especially is not a thing to be dogmatic about (and by this I do not mean to imply that there is anything whatever which ever should be) but still I think it will serve my turn.

Although I have never painted a picture, and am no art critic, yet I have criticised the pictures in this and former years with the most intense pleasure from the scientific point of view. This year I have limited myself to sky colour, and I have prepared two lists, one, including those pictures which I think in harmony with nature, and the other those which represent phenomena which, however probable in any other planet, are, I think, physically impossible in this.

I have done more. I have tested the hypothesis by the pictures. I have gone over those in which I was chiefly interested from my narrow point of view with two artist friends of great distinction, and I have asked them whether the view at which I have arrived in each case was correct. The test I had applied had failed me in no instance.

Here then are the most salient examples included in my lists. I dealt with pictures, not artists, and carefully avoided seeking the artist's name in any case; but here I must bring them out, in order to refer to the pictures with sufficient completeness.

First, then, to deal with those pictures in which cloud and sky colour are, I think, correct:—

3. "The Timber Waggon"—C. E. Johnson. Accurate study of the absorption of light by a slightly hazy atmosphere.

63. "A Summer Flood"—H. R. Robertson. Colour of cumulus clouds glowing with the reflected light of sunset, perfect.

105. "The Cornish Lions"—John Brett. Remarkable picture: the colours and the atmospheric absorption, and therefore transformation of the colour, perfect.

153. "Evening"—R. C. Leslie. Wonderfully true rendering of a very rare effect.

230. "Estes Park, Colorado, U.S."—Albert Bierstadt. Very fine atmospheric study. The vapour rolling down the valley leaves its effect on the picture marvellously.

267. "Wandering Shadows"—P. Graham, A. Magnificent picture. Notice the effect of the atmo-

sphere laden with aqueous vapour on the colour of the hill in the background.

268. "The Alps at Rosenlaui"—V. Cole, A. Gloriously true. The fading of colour in the distant bosses is perfectly rendered—the depth of the atmosphere can be gauged.

306. "Struyve Rocks, coast of Arran"—Geo. E. Hering. A red sunset, nearly perfect in colour from top to bottom; if the yellow had faded into green it would have been better. Compare red with 353.

324. "Conway Marsh"—Jos. Knight. Sunset green, and deep blue hill admirable, but I doubt the colour of the foreground.

405. "Gleaners"—H. R. Robertson. Red, yellow, green, good. Moon nearly right, which is wonderful. (This by the way).

587. "Shining after Rain: Loch Etive"—Geo. E. Hering. The work of a careful observer.

615. "The Lowing Herd winds slowly o'er the Lea"—H. W. B. Davis, R.A. Perfect sunset (poor moon!). (Again by the way).

647. "An Autumn Walk"—A. E. Emslie. Good red and yellow.

739. "Sunset on the Jungfrau, Mönch, and Eiger"—Jas. W. Smith. The blue below and red above on the snow perfectly rendered.

788. "The Written Valley, Wilderness of Sinai"—Henry A. Harper. Good, but not so good as 739.

I next come to those pictures which I think are inaccurate in colour.

86. "Christiana with her Family, accompanied by Mercy, arrive at the Slough of Despond: Mercy finds a way across"—R. Thorburn, A. Impossible cloud colours. Clouds bluer than sky and atmosphere nowhere.

146. "Solitude"—P. F. Poole, R.A. Impossible green sky and cloud.

201. B. Riviere, A. Unnatural moonlight and impossible pea-soup shadows. The softness and colour of the latter suggest that Mr. Riviere has never studied moonlight.

231. "David, the Future King of Israel, while a Shepherd at Bethlehem"—J. R. Herbert, R.A. Colour impossible both in quantity and quality.

240. "A Dream of Ancient Egypt: the Morning of the Exodus"—Andrew MacCallum. I should like to hear the painter lecture on the connection of the colours of bodies with the light which falls upon them.

298. "Jarl Hacon in the Pentland Firth"—J. Hope M'Lachlan. High blotches of red over green and yellow impossible, and brick-dust beams of light proceeding from nothing still more impossible.

309. "The Sunrise Gun, Castle Cornet, Guernsey"—Tristram Ellis. Sky colour good; impossible colour of water under sky conditions given.

353. "After the Rain"—W. H. W. Foster. Unnatural sunset, colour and distribution of light wrong.

424. "The Last Journey"—Clara Montalba. Impossible green sky; the sun is neither setting nor set.

483. "An Autumn Sunrise"—Cecil G. Lawson. Interesting as a foretaste of the future when the sun shall have cooled.

525. A. Dixon. Green hopelessly wrong.

542. "The Dee Sands"—J. W. Oakes, A. Sky colours impossible with so high a sun.

555. "The Last of the Wreck"—E. Ellis. Green clouds!

630. "An Incident by the Wayside"—Mark Anthony. Impossible blue sky.

These, then, are the pictures I shall use as texts in my future notes. J. NORMAN LOCKYER

THE AMERICAN STORM WARNINGS[1]

I HAVE now to direct attention to "Atlantic Storms,—Whence they come and where they go?" All storms that cross the Atlantic Ocean to the coasts of Europe come from the equatorial zone of the Atlantic from the Pacific Ocean, or are developed from depressions on the American continent by peculiar operations of the law of atmospheric movements. The most prolific source of storms for the field of observation just sketched is the Pacific, but all the disturbances coming thence do not necessarily originate there. As I have stated, storms pass over the Pacific from the Asiatic as they do from the American continent over the Atlantic, but generally in more northerly latitudes. Their number cannot be accurately determined until a similar system of observation to that now in operation from the West Indies to Newfoundland is organised on the Eastern coast of Asia. As it is we are dependent on observations made along the Pacific coasts of the United States, British territory, Mexico, and the Central American States, for information regarding the arrival of storms from the westward on this continent. Fortunately the observers are now numerous enough to constitute an effective guard against the possibility of even a small storm centre passing inland unnoticed. These coast observations furnish reliable evidence of the fact that storms arrive on this continent from the Pacific much in the same manner as Atlantic storms reach Europe. It is my purpose to trace as closely as possible the movements of the various types of storms that originate in or cross the Atlantic from west to east, and I will begin with those whose first appearance is observed on the Pacific coast of the United States.

It has been noticed that storm areas approach these Pacific coasts as large depressions with a comparatively low energy of rotation around their centres. But when the area reaches the line of the coast or cascade range of mountains in Oregon and Washington territory, its outline is changed from the distorted circular to that of the irregular elliptical, and the northern end of the latter figure is carried toward the coast line more rapidly than the southern one, causing, as a rule, the first rainfall in the line of first contact with the land. Therefore, over Vancouver's Island and Western Oregon a rapid condensation of atmospheric moisture takes place which so

[1] Continued from p. 7.

speedily exhausts the air volume immediately affected by the storm of its humidity, that the lines of equal annual rainfall on this section of the coast are very close together, marking a decrease of precipitation inland. The energy of rotation increases here as the pressure at the storm centre falls. This energy concentrates at the northern end of the depression, and the area of low barometer is drawn, as it were, around the centre so formed as it passes eastward over the first range of mountains. After passing over all the intervening ranges of the great plateau toward the line of the Rocky Mountains in Montana and the British territory northward thereof, the storm as a moving atmospheric vortex is attended by only a very little rain or snow. The region over which it passes cannot furnish any supply of humid air, and the storm becomes again disorganised into a great depression during and after its passage over the mountains, until its centre has reached the eastern slopes. But here it enters a new region so circumstanced in its topographical relations with the east and south, as to derive a full and uninterrupted flow of humid air from the great river valleys, the lake regions, and the distant Gulf of Mexico. There are no intervening mountain barriers between these sources of humidity and the north-western prairies to interrupt the atmospheric flow toward the depression extending over them, but the storm reorganises slowly at first as the conditions necessary to induce a strong indraught of air to its centre are of very gradual development. When, however, they come into requisite combination, the indraught winds increase, and coming from the north-east and east, are deflected southward and south-eastward by the mountains, until a feeble but decided vortex is developed in the centre of the depression. The centripetal winds now begin to increase with the inflow of humid air, and the newly organised storm-centre moves eastward along its track, toward the region of the Mississippi Valley or the lakes. In doing so it descends the gradient of the plains through air of increasing density, and acquires greater energy every mile it advances. High pressures to the northward and southward of the storm-centre constantly feed it with fresh volumes of air, which being of different conditions of temperature and humidity, produce the rainfall that generally begins when the eastern margin of the depression enters the Missouri Valley. In the great region of the plains the storm finds free scope for development as well as an unfailing supply of atmospheric moisture. It usually attains its greatest energy when passing over Iowa, Illinois, Ohio, and Kentucky, toward the Upper Ohio Valley, and the narrow neck of territory between Lake Ontario and the Pennyslvanian section of the Alleghany Mountain Range. This mountain wall influences the course of the storm by deflecting it toward the north-east from the Middle Ohio Valley region, and thence over New England to Nova Scotia. The districts eastward of the Alleghany Mountains and southward of New York are rarely traversed by storm-centres coming as I have described, from the north-west, but receive the rainfall of the eastern margin of the storm as its centre passes north-eastward beyond the mountains, into the St. Lawrence Valley or the New England States.

But the mountains cause a profuse rainfall on their western slopes, and when the storm reaches the Atlantic the precipitation has been nearly exhausted. Its energy, therefore, decreases, when crossing from Oswego to Portland or Eastport, Maine, and does not recover until it receives from the Gulf Stream Region a new supply of humid air. I have endeavoured to describe the course of a storm-centre from the Pacific to the Atlantic, across the Continent, and have made no detailed explanation of the relation to its movement of the areas of high pressure. This I regard as of the highest importance, and will treat of fully under a special head. The course of the storm across the Atlantic, as well as its movement over Europe, will be governed only, I may say, by the high pressures. These being distributed from south to north, in a series of continuous, but movable zones, mark the directions of the storm's advance so clearly, as to enable an observer at this side of the ocean to predict with general accuracy, the section on the European coast on which the storm-centre will arrive, as well as the time of its arrival. Another type of Pacific storm is that which arrives on the southern and central section of the California coast as a great depression, and entering the continent, pours its rains over California, and becomes divided into two sub-areas of low barometer by the Sierra Nevada range. One of these sub-areas, and nearly always the largest, takes a south-easterly direction across Southern Nevada, into Arizona, and crosses the Rocky Mountains in New Mexico to Northern Texas, where it is organised into a storm in the same manner, but much more rapidly, as the previously described area crossing into Montana. The other sub-area passes from Central California to Idaho, and thence across the Rocky Mountains, into the Yellowstone River Valley in Montana, pursuing a track, thereafter, which sometimes brings the depression into the Lower Missouri Valley, but usually towards the Upper Lake Region. This sub-area of low pressure also becomes organised into a storm, but one of much less energy than that of Northern Texas. This can be accounted for by the fact that the crossing of the mountains by both sub-areas being almost simultaneous, the northern depression cannot receive any considerable atmospheric flow from the southward, as it is intercepted and drawn toward the southerly vortex. It sometimes occurs that the two centres of disturbance unite in a common depression west of the Mississippi River, but usually they preserve their identity, and become separated gradually by an intervening zone of relatively high barometer developed between them by their joint influence. The northern centre moves away to the north-east, over the lakes and Canada, with diminishing energy, but the southern storm centre advances into the Lower Mississippi Valley, and soon dominates the weather conditions over all the region southward of the lakes. In this position its isobars extend eastward to the Georgia coast, and even into the Atlantic, but the centre moves towards the Ohio Valley, westward of the Alleghany Mountains. The consequence is that a section of the depression near the Atlantic coast is cut off by the high range of the Alleghanies, and another sub-area is formed which is speedily organised into a distinct storm centre by the impinging of the ondraught winds from the east, north-east, and south-east on the mountains, in the same manner as I have already described.

As the centre of the main disturbance moves up the great central valley the subsidiary centre east of the Alleghanies moves with it, and where the mountains decrease in elevation the two centres draw towards each other so as to have a common encircling isobar of 29·60 inches, and sometimes even less. When they reach the latitude of New York, storms of this type commonly leave the coast between latitudes 38° and 42°, attended by an area of high pressure immediately to the northward, and followed by one from the south-west. The courses of these storms across the Atlantic are generally in comparatively low latitudes, and they arrive on the British Coasts from the west or south-west with moderate rains and winds backing from the north-east to the north-west.

Another type of Pacific storms is the one which originates in the tropical zone of that ocean, and strikes the Mexican coast, moving directly across that territory into Southern Texas, and along the Gulf Coast over Florida and Georgia to the Atlantic. The energy of such storms is frequently very great, and they retain, even after crossing the Mexican plateau, many of their original cyclonic features. When they move northeastward through the Mississippi Valley they are always attended by heavy rains and electrical disturbances. Local storms or tornadoes are frequently developed on their south-eastern margins during the spring and summer months, and are always very destructive.

These Mexican storms, so called to distinguish them from the disturbances that move over Northern Texas from the California coast, will sometimes, but not often, cross the Alleghany Mountains from Tennessee to Virginia, and pass into the Atlantic northward of Cape Hatteras. Their courses across the Atlantic are generally southerly as compared with those of storms leaving Nova Scotia. They arrive on the British and French coasts from the south-west, but are now and then carried in a north-easterly direction, passing to the Norwegian coasts northward of Scotland, and thence over the Scandinavian Mountains into North-Eastern Russia and the Siberian Seas.

The cyclone, or great storm that originates in the equatorial zone of the Atlantic, by which I mean the region embraced between the equator and 15° N. lat., possesses characteristics which mark it as the most destructive atmospheric disturbance known to meteorologists. Of course these storms are developed in the equatorial zones of other oceans, but are not of such immediate interest to us as the Atlantic cyclones. I am convinced that the conditions which combine to develop nearly all areas of low pressure have an equatorial origin, the exceptional cases being due to local liberations of terrestrial heat during earthquakes and to the heating of volumes of air over great areas of sandy desert. North Atlantic cyclones may be divided into four classes, namely: Those that originate near the Cape Verde Islands and make their northward curves east of the 35th meridian, and do not affect the American coasts, but enter the European area over Morocco and Spain, passing eastward over the Mediterranean Sea. They are of comparatively rare occurrence. Secondly, those that originate about the 40th, and curve northward east of the 80th meridian, affecting the American coasts only by the induced marginal winds. Thirdly, those that originate immediately east of the Caribbee or Windward Islands, and perform their northward curves between the 80th and 90th meridians, so as to pass through the eastern part of the Gulf of Mexico, and over Alabama, Florida, Georgia, and the Carolinas toward the North Atlantic. Fourthly, those that originate nearer to the equator than the others referred to, and make the tremendous sweep from the middle of the ocean between the Venezuelan coast of South America and that of West Africa, over the West Indian Islands to the Texas coast, and there curving northward and eastward, sharply pass over the southern sections of the United States and into the North Atlantic from the vicinity of Cape Hatteras.

Of the first-named class of cyclones, little need be said beyond the reference already made. They represent the most serious dangers to be encountered by vessels bound to West African or South American ports, or passing over the Cape route to the Indian Ocean. The second class of cyclones, of which we have examples in the great storms of October 12, 1780, August 17, 1827, and August 12, 1837, and the later one as traced by the United States Signal Service Bureau, which commenced about August 18, 1873, take northerly courses. The only land station where these can be accurately observed is that at Bermuda; therefore information regarding their energy and movements must be collected from the logs of ships that cross their tracks. It is believed that these storms are developed only in the midsummer, and are not of frequent occurrence, but on these points we have very little reliable information. I am, however, inclined to accept the statement as to their infrequency.

The third class of cyclones we are more familiar with, because it embraces that type of equatorial storm which we most frequently experience. Examples from the earlier meteorological records are the storms of August 10, 1831, and October 6, 1846. With these we have the recent one of September 21, 1877, and which was signalled to London by the *Herald* Weather Bureau. The passage of this storm over the South Atlantic coast of the United States was attended by many disasters, wrecks, and inundations. Its course towards Europe was in comparatively low latitudes until it approached the Bay of Biscay, when it moved sharply north-eastward, causing heavy gales and rains, with thunder and lightning. The latter effects were very marked in Scotland.

The fourth class of cyclones, such as those of June 23, 1831, and September 27, 1837, and later on September 21, 1875, known as the great Galveston cyclone, are usually of extraordinary violence. Among the first successes of the *Herald* Weather Bureau was the correct prediction of the course of this storm when it was moving westward over the Carribean Sea. Only on one instance within my observation has a cyclone of the third class passed northward on the western side of the Alleghany Mountains, and then the storm exhausted its energy in Canada, but its depression, though much contracted, reorganised into a minor disturbance when it passed into the Atlantic, off the New England coast. The tendency of cyclones to lose their force by the extension of their area of low pressure is more decided than in any other type of storm. This will account for the low degree of

energy in disturbances evidently of equatorial origin when they reach the Pacific coast of the United States and the coast of Spain. Unless the direction of the zone of high pressure along the south margin on which they move forms an angle of more than forty-five degrees with the equator, the storm has a tendency to pass through it, and in doing so expends much of its energy.

JEROME J. COLLINS

(*To be continued.*)

GAS AS FUEL

ATTEMPTS have been made from time to time to use gas as a means for heating; these attempts have more frequently failed than succeeded, chiefly by reason of the mechanical difficulties to be overcome.

It is pretty generally agreed that, on account of the ease with which the supply of a gaseous fuel can be regulated, the completeness with which such a fuel can be burned, the comparative readiness with which cleanliness can be maintained while using this fuel, and by reason of its high heating power, and for other reasons, gaseous fuel is to be much preferred to fuel in the solid form.

The most perfect gas for heating purposes would be that, the constituents of which should be all combustible, should be possessed of high thermal powers, and should produce, on burning, compounds of small specific heat. No gas which has yet been produced for use as fuel completely fulfils these conditions.

Common coal-gas contains such non-combustible bodies as carbondioxide and nitrogen, and among the products of its combustion is water, a body of large specific heat, and also requiring a considerable amount of heat to convert it into vapour. The complete combustion of coal gas also necessitates a comparatively large supply of air, and this, again, involves special mechanical appliances. Nevertheless, coal-gas has been proved to be, for certain purposes, a cheaper, more effective, and more easily managed fuel than coal, wood, or other forms of solid heat-giving material.

That steam is decomposed by hot carbon with the production of a gaseous mixture of considerable heating powers, has long been known, and several attempts have been made to utilise the products of this decomposition. These attempts have met with no great success on account of the cost of the plant required to work the manufacture and of the difficulties of the process. Long-continued experiments have, however, been carried on, and it would appear from a paper recently communicated to the Society of Arts by Mr. S. W. Davies, that these experiments have been crowned with a very fair measure of success.

The great difficulty was a mechanical one: it has been very simply overcome. Superheated steam is produced in a coil placed within a cylinder and is driven by its own tension in the form of a jet into the lower part of an anthracite fire. The jet of steam carries with it air sufficient to actively maintain the combustion of the anthracite; the gases issue at the top of the apparatus and pass into the mains. The fire is fed from the top by an arrangement which allows of the process being continuous. Water is forced into the coil under a pressure varying from 15 lbs. to 40 lbs. on the square inch. The whole apparatus is compact and simple.

The products of the decomposition of steam by hot carbon are mainly hydrogen and carbon monoxide; traces of marsh gas are also formed. Could these gases be produced free from admixed non-combustible bodies we should have a gas of very high heating powers. But the temperature of the glowing carbon must be maintained by the introduction of oxygen, that is, in practice, by the introduction of air. The problem how to introduce air sufficient to keep up vigorous combustion, and at the same time to maintain the decomposition of the steam, appears to have been satisfactorily solved; but the introduction of air means a lowering of the heating power of the gas produced, inasmuch as four volumes of nitrogen are brought in along with every volume of oxygen supplied. By passing the gas through a series o vessels containing hot carbon the nitrogen may be very much diminished in amount, and the heating power of the gas proportionally increased.

The gas produced by the decomposition of steam by hot carbon always contains traces of carbon dioxide which is non-combustible; the amount of this compound may, however, be reduced to 3 or 4 per cent. by regulating the depth of the layer of hot carbon through which the gases pass, and by maintaining the temperature of that carbon at a high point. But the maintenance of a high temperature throughout a mass of carbon can be accomplished, under the conditions of the manufacture, only by introducing a rapid current of air, which again means a dilution of the gas produced.

If, therefore, means could be found for feeding the anthracite fire with oxygen, a gas of very high heating power might be produced. A supply of oxygen at a cheap rate is a great desideratum; the gas exists in practically unlimited quantity in the atmosphere, but an easy and successful method for separating it from the nitrogen with which it is there mixed is still only hoped for by the chemical manufacturer. Were a supply of oxygen forthcoming, mechanical difficulties would present themselves before it could be utilised in the production of "water gas." The introduction of too small an amount of oxygen would mean the non-decomposition of the whole of the steam and the cessation of the combustion of the anthracite; the introduction of too much oxygen would mean the production of carbon dioxide in considerable quantity. But by regulating the size of the steam jet and of the blast-pipe, these difficulties might probably be overcome.

As the gas is now produced all danger of explosion is removed.

The heating effect of the gas as at present manufactured is about one-fifth that of ordinary coal-gas, for equal volumes; but the cost of the gas is so much less than that of coal-gas, that a given amount of heating work may be done—according to the figures given in the paper referred to—by using the new gas, with a saving of from one-third to two-thirds of the expenditure which would be involved were coal-gas employed.

Although the new gas is not perfectly adapted for the purposes for which it is to be used, yet there can be little doubt that we are now a step, and a very considerable step, nearer the final solution of the problem. Doubtless improved furnaces, and improved apparatus generally for burning the improved fuel, will be introduced.

The production of a cheap gaseous form of fuel is a great gain; so also is the invention of a means whereby the large stores of anthracite coal in this and other countries can be utilised.

Of all the forms of carbon experimented with in the production of the new gas, anthracite was found the best. Anthracite is difficult to burn; the ordinary forms of furnace do not admit of such a complete oxidation as is required in order to maintain the combustion of anthracite. But the blast of air carried into the gas generator of the water-gas apparatus by the steam jet insures the presence of a large quantity of oxygen, and therefore the combustion of the anthracite. Whether a simpler means could not be adopted for the combustion of anthracite is a question worthy of consideration. That a steam jet can be thrown into an ordinary furnace charged with anthracite, and the combustion of the coal be thereby insured, has been shown to be possible. Nevertheless, the production of combustible gas from the anthracite is to be preferred, for many reasons, to the consumption of the solid fuel.

The fact that we shall soon probably be in a position to make use of our stores of anthracite, is one of very considerable importance from an economic point of view. In possessing large quantities of anthracite we possess a valuable commodity, but if we cannot realise a use for that commodity it ceases to be a source of wealth to us.

Further, large quantities of anthracite are known to exist in some of the British Colonies and in the United States; the utilisation of these would mean an increase in the commercial enterprises owned by Englishmen abroad, or supported by English capital; it would also probably imply an increase in the tonnage of shipping, and would thus tend to increase our "international wealth."

Whether it be regarded from the point of view of the chemist, or of the economist, the introduction of a cheap gaseous fuel manufactured from anthracite, marks a point of no little importance in the advance of manufacturing industries.

The experiments detailed in the paper by Mr. Davies show that the new gas is especially adapted for use in cooking operations in large private establishments, in clubs, hotels, barracks, &c. It is known that cooking can be more cheaply and more rationally conducted with the aid of gaseous than of solid fuel; if the new fuel does all that it promises to do, judging from the actual trials already made, its introduction will be welcomed by the artistic cook no less than by the scientific chemist, and by the political economist. M. M. PATTISON MUIR

FOSSIL FLORA OF GREAT BRITAIN

The Fossil Flora of Great Britain; or, Figures and Descriptions of the Vegetable Remains Found in this Country.

Illustrations of Fossil Plants, being an Autotype Reproduction of Selected Drawings prepared under the Supervision of the late Dr. Lindley and the late Mr. William Hutton, between the Years 1835 *and* 1840—*and now for the first time published by the North of England Institute of Mining and Mechanical Engineers.* Edited by G. A. Lebour, F.G.S. (Newcastle-upon-Tyne, 1877.)

THE publication, in 1831, of the first number of the "Fossil Flora of Great Britain," by Dr. Lindley and William Hutton, marked the beginning of a new era in the history of English Palæo-phytology. Much had been previously done on the Continent. The magnificent *Flora der Vorwelt* of Sternberg had laid a solid foundation for such studies, and the *Végétaux Fossiles* of Adolphe Brongniart, then in progress of publication, was not only widening those foundations, but was systematising the study, as his "Prodrome" had developed the first principles of the philosophy of the primæval Flora. The late Professor Phillips had further recorded additional discoveries amongst the Oolitic plants of Yorkshire, in his "Geology of the Yorkshire Coast"; but there yet remained a wide field for exploration, especially amongst the plants of the Carboniferous age, in which England was especially rich; and Phillips and Brongniart were very far from having exhausted the newly-discovered plants of the Yorkshire Oolites. Hence when the two able authors above named commenced the publication of their "Fossil Flora," they found a vast mass of new materials awaiting their investigation. In endeavouring to estimate the true value of their work, we must not regard it from our present standpoint, but from that of the time at which they began their labours. At that period, though collections of fossil plants were numerous, they were scattered over the country in isolated cabinets, and no one knew much about what those cabinets contained. Hence the first work demanding attention was to ascertain what the forms and general relations of these fossil plants were, and the pages of the "Fossil Flora" gradually gave the needful information so far as it was then obtainable. The two authors named figured and described such distinct fragments as fell into their hands, and thus made available for the students of a later period a vast mass of hitherto unknown material. This important publication went on for several years—but at length the two authors became weary of their costly venture. The number of persons actively interested in the study of fossil plants was not sufficiently great to cover the expense of the publication, which consequently came to an abrupt end. In 1839 the late Dr. Lindley told the writer of these lines that he saw no reason why he should employ his purse for the benefit of the geologists who failed to give him the needful support, and he acted upon the conviction thus expressed.

In endeavouring to measure the true value of the work of Lindley and Hutton to modern science, we must not forget the date of their labours. At the earlier part of the time when the publication of the "Fossil Flora" was in progress, little or nothing was known of the internal organisation of any fossil plants. But at length two instructive fragments were obtained in England—one of a Lepidodendron, and the other of a Stigmaria—both of which examples revealed a measure of minute internal organisation. Witham's "Observations on Fossil Vegetables," published in 1831, contained figures and descriptions of the first of these specimens, the now well-known Lepidodendron Harcourtii, and the Stigmaria was figured and described in the "Fossil Flora." These two specimens were the beginnings of a rich harvest, which is even yet but very partially reaped, but which has already prepared the way for a revolution in the processes and results of Palæo-phytological studies. But though the authors of the "Fossil Flora" thus obtained some glimpses into the possible future of their science,

they did little more. Like good and true men they did the best they could with the materials within their reach. They found various dissimilar fragments of apparently distinct forms of fossil plants which they named, figured, and described. They thus introduced a certain degree of order and definiteness into what had hitherto been a *rudis indigestaque moles*. This work benefited not only contemporary but succeeding races of geologists. Such labours as these are the necessary preliminaries to the more exact determinations of more advanced science. Work like this has to be done in the early stages of every branch of natural science, and no great harm arises from the multiplication of genera and species, if we only keep in mind the fact that such nomenclature is but provisional;—a mere ticketing of special forms for convenience of future reference. The names do not indicate very much more than the fancy designations given to various "makes" of cloth in a Manchester warehouse—*i.e.* convenient terms by which the business transactions of buyer and seller are facilitated. Mischief only arises from this essential method when we make these provisional nomenclatures the basis of ambitious philosophical speculations; when, for example, because a plant is designated by the name of Palmacites, we conclude that Palms flourished in the carboniferous age. Keeping in mind the true use of a provisional nomenclature we find it indispensable to further progress. When some inquirer, more advanced than his predecessors, demonstrates that *Sigillaria A* and *Sigillaria B* are merely the upper and lower parts of a common stem, it is useful to him to be able to indicate by his terms A and B what the types are that bear this mutual relationship.

The *scientific* worthlessness of very many of the generic and specific definitions and names of fossil plants is now becoming obvious to all advanced students of Fossil Botany. Yet the assignment of these names and definitions to such fragments as fell in their way is the chief result of the publication of the "Fossil Flora." To the philosophy of the study its authors added very ittle. They left the supposed relations of the great types of vegetation to each other pretty much where they found them. They seem to have accepted equally what was true and what was false in the philosophy of Adolphe Brongniart. No one important discovery will be handed down to the future associated with their names. Fragments from various parts of the same plant took rank at their hands as independent species. Little or no attempt was made for variations due to age and conditions of growth. Nor were they to be blamed for this. We are still to some extent in the same predicament—only, thanks to the warnings of Sir Joseph Hooker and others, we now know what we have to aim at. We have to try to accomplish for plants what Burmeister did for the Trilobites. But if the use of merely provisional names is to be continued, it is very desirable that we should possess some means of distinguishing between such a nomenclature, and one that represents philosophic truths and may be employed as the basis and instrument of philosophical speculations. Nothing of the kind has yet been attempted beyond the "incerta sedes" of Brongniart. Yet I think it would not be difficult to invent some technical sign that would answer this end. For the present it can only be left to the judgment of each individual observer to determine what names are of scientific value and what are not.

But the most essential truth which these later days are teaching us is the importance of the study of internal organisation; and especially of that of the reproductive structures, if fossil botany is to take its proper rank as a definite science. Nothing can be more dangerous than a reliance upon mere resemblances or differences of external form. We have a ready illustration of this in the numerous verticillate-leaved plants of the Carboniferous beds. So far as mere external forms are concerned, Calamites, Asterophyllites, Sphenophylla, and Annulariæ, with a host of less known modifications, bear a close resemblance to each other—and if a few Galiums, Asperulas, and other similar living exogenous forms could have been thrown in amongst them they would probably have been equally undistinguishable from the rest. The result is that the nomenclature and classification of these Carboniferous plants is in hopeless confusion. True, we are slowly emerging from this chaos, because we are learning to distinguish some of these forms from the rest through their widely differing features of internal organisation—and every fresh plant in which we do so diminishes the bulk of the chaotic mass that still needs reduction to order. Though so much has already been done in this way, we are yet only on the threshold of the study. At the same time we are moving in the right direction. Such localities as Autun, St. Etienne, Oldham, and Halifax have furnished, and are likely further to furnish, important materials—each locality having revealed characteristic forms of vegetation peculiar to it, mixed with other forms common to all the localities. It is to be hoped that other similar storehouses will be opened out, revealing fresh forms of structural organisation, since it is upon organisation alone that a sound classification of fossil plants can be based.

The recent republication of the "Fossil Flora" is almost an exact fac-simile of the original work—even to its title-page. Copies of the old edition being rarely obtainable this re-issue will be valuable to a large number of young geologists. At the same time it is desirable that something should be done to distinguish between statements still to be relied upon, and such as represent now exploded errors. This might have been done by the introduction of editorial notes—but instead of this, its accomplished editor, Mr. William Carruthers, is about to issue a supplementary volume, giving the existing state of our knowledge of many of the objects represented in the original work. This may well be expected to constitute a valuable addition to the volumes already issued.

The second publication named at the head of this notice has an affiliated relationship to the "Fossil Flora." When Hutton died he left behind him numerous drawings of fossil plants, obviously prepared for publication, many of them having connected with them manuscript annotations of various kinds. A selection from these has been published in an elegant volume issued under the auspices of the North of England Institute of Mining and Mechanical Engineers. It is obvious that many of these drawings represent plants of more doubtful nature than the majority of those published in the "Fossil Flora." It might be expected that the more definite types would be first selected for publication. But this is precisely

what appears to me to constitute the value of the volume in question. We have but too frequently, though very naturally, figured and described the more definable types, the more obscure and intermediate forms being left for a further consideration, which sometimes never comes! Yet these obscure examples often teach most important truths. Had all writers paid due attention to such intermediate varieties, the science of Palæo-Phytology would have been less afflicted with premature "classifications" than has been the case. Hence the spirited society that has published these posthumous Huttonian memorials is entitled to the thanks of all Palæontologists.

W. C. WILLIAMSON

TAXIDERMY

Practical Taxidermy; a Manual of Instruction to the Amateur in Collecting, Preserving, and Setting up Natural History Specimens of all Kinds. By Montagu Browne. (London: *Bazaar* Office, 32, Wellington Street, Strand. No date.)

ACCORDING to the dictum uttered, or supposed to have been uttered, by one of our leading ornithologists, "The worst use you can make of a bird is to stuff it," and in nineteen cases out of twenty this saying is true, for, from a real naturalist's point of view, comparatively little can be got from the stuffed and mounted specimen not only of a bird but of almost any other animal. Nevertheless, there is a very large class of persons who are not real naturalists, and to them the skin of a beast, bird, reptile, or fish, duly prepared and embellished with glass eyes, stuck up with wire through its legs in a glazed box, and surrounded by imitation foliage, dried and dyed herbage, is a joy for ever, though perhaps not even to them a thing of beauty. For this large class the present book is intended, and it will probably attain its object, notwithstanding that how far the animal stuffer's trade is to be learned from any book without actual demonstration seems to be questionable. The author's practical knowledge of his business is, we doubt not, considerable, and it would have been better had he let alone some of the matters not really relating to it upon which he descants. His very first sentence tells us that taxidermy "is derived from two Greek words, a literal translation of which would signify the 'skin art'"—a statement which beats the time-honoured explanation of Hippopotamus, from *hippos*, a river, and *potamos*, a horse, inasmuch as *taxis* has as little to do with art as with the Queen's taxes—and then goes on to inform us, from Herodotus, the *Penny Cyclopædia*, and other trustworthy authorities, how the Egyptians made mummies, which is all as delightful as so ghastly a subject can be, but is certainly somewhat superfluous as "Instruction to the Amateur" in "preserving and setting up Natural History Specimens." Hardly less unnecessary is Chapter II. devoted to "Trapping and Decoying Birds and Animals," whereby we may remark that the author is of that persuasion which denies the animal nature of birds. But we may pardon him this and other offences for what he says (pp. 14, 15) against the needless destruction of the rarer "birds and animals," and thence to Chapter X. is much more to the purpose. We are sorry to see, however, that he is addicted to the usual taxidermist's mannerisms, most of which are fatal to good and artistic mounting. Paint, for instance, however thin, *on* bills and legs is an abomination. If colour is required it ought to be supplied by subcutaneous injection, which in the majority of cases can be easily and successfully done. Artificial twigs of wire and tow, dusted over with powdered lichens and the like, are nearly as objectionable as the external application of paint. As regards the stuffing of heads of large mammals the instructions given are really good, but we suspect that a satisfactory result cannot be obtained without far more experience and closer study of nature than the author would have us think necessary. We must reproach him, moreover, for not giving a hint to the learner as to the best mode of preparing the "skin" of a bird so as to prevent its head from breaking off. This is done by inserting a long lock of cotton-wool of tow into the cranium (from behind, of course) making it fast there by tight packing, and then twisting the remainder of the lock into a kind of loose cord, which does not distend the skin of the neck, enables its length to be adjusted as may be required, and finally affords a coherent and effectual support, whereas the ordinary mode of ramming bit after bit of stuffing into the neck has exactly the opposite tendency.

Mr. Montagu Browne speaks with complacency of the achievements of English "artists" in taxidermy; but it seems as if his acquaintance with foreign works was limited to the comical creatures from Würtemberg in the old Exhibition of 1851. We venture to say that there is hardly a museum on the Continent which has not its specimens mounted in a style that no professional in these islands can equal—certainly not surpass. When we look at that really awful group of the boa and the peccary, recently erected in the British Museum, we blush for the handiwork and ignorance it displays. The impression it gives is that the boa, being crammed into a cylindrical form, is quite inflexible, and that the peccary, though not a learned pig, is fully aware of the fact, so, feeling sure that there is no chance of his being crushed by his enemy, he rather likes the adventure than not.

The question of the use of arsenic in preparing skins we cannot discuss at any length. Our author declares that *Tineæ* and *Dermestæ* laugh it to scorn, even if they do not, as he believes, like the Styrians, "fatten on it" (p. 44). We shall only say that we prefer it, and know of a case in which a collector in the tropics, having exhausted his stock of the poison, was compelled to prepare some of his specimens without it, which specimens were some years afterwards attacked and greatly injured by insects; while others, obtained before his store gave out, and duly arsenicated, remained unharmed, though lying side by side in the cabinet with the specimens that suffered. Arsenical soap, it is true, does not keep either feathers or fur safe, simply because it cannot be applied to them, but it certainly preserves the skin according to our experience, and every travelling collector should unquestionably use it. Corrosive sublimate is effectual for a time, but the best preventive is a well-fitting cabinet—care being taken that infected specimens are never introduced to it. In conclusion let us caution our readers not to be misled by the similarity of the

author's name into confounding the present book with one on the same subject published many years ago by Capt. Thomas Brown.

OUR BOOK SHELF

The Gold-Mines of Midian and the Ruined Midianite Cities. A Fortnight's Tour in North-Western Arabia. By Richard F. Burton. (London: Kegan Paul and Co., 1878.)

CAPTAIN BURTON has managed to make a wonderfully interesting and really valuable book out of his fortnight's visit to the ancient land of Midian, on the north-east side of the Red Sea, on and to the south of the Gulf of Akabah. Long ago he had good reason to believe that in this region gold was to be found, but only in March and April of last year was he able to test his surmise, under the auspices and at the expense of the Khedive. The result of this visit is that he is satisfied that there exists a real Ophir, a regular California, extensively worked in ancient times, and whose valuable product is probably not unknown to the tribes who haunt it at the present day. Not only gold exists there, but vast deposits of iron, with copper, tin, and other metals—in fact a welcome treasure-house for the impecunious Khedive. Capt. Burton has hopes that modern Midian, now almost a desert, may yet rival the ancient land from whose people the Israelites, in the exercise of their divine vocation, carried off "the gold and the silver, the brass, the iron, the tin, the lead." Capt. Burton made a minute inspection of some of the ancient sites, and has a good deal to say on the archæology of the region, as well as its zoology, botany, and geology. But the book is not nearly all on the land of Midian. From the time that the author left Trieste for Alexandria and Cairo, by Suez to Midian, till his return, he saw many things on which, in his own digressive and parenthetical style, he has much to say that is worth listening to. Capt. Burton has just returned from another visit to Midian, and no doubt we shall soon have another work or an enlarged edition of the present.

To the Arctic Regions and Back in Six Weeks, being a Summer Tour to Lapland and Norway, with Notes on Sport and Natural History. By Capt. A. W. M. Clark Kennedy. Map and numerous Illustrations. (London: Sampson Low and Co., 1878).

THE title of Capt. Kennedy's pleasant volume is rather misleading; before looking into it we thought he would take us as far as Spitzbergen at least, and felt somewhat "sold" when we found his journey ended at Tromsö, in the north of Norway, which, though within the Arctic Circle, is not usually spoken of as in the Arctic Regions. Still Capt. Kennedy's book is thoroughly readable, and though it will add little to our knowledge of Norway or of the Lapps, will prove valuable to any one contemplating a visit to that now much-frequented tourist ground.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Eastward Progress of Terrestrial Magnetism

As the progress of weather eastwards is one of the subjects now engaging attention, while the possible connection between meteorological and magnetical phenomena is another, we are led to ask if there be no traces of an eastward progress in certain of the phenomena of terrestrial magnetism.

I cannot yet affirm that such is the case, but it may interest your readers to know that, as far as a preliminary investigation goes, there are some indications of such a progress when we compare together the Declination-ranges at Kew and at Trevandrum. It will, however, require a more thorough discussion before the fact can be considered as at all established.

Manchester, May 4 B. STEWART

The Phonograph

SINCE writing my former letter on the phonograph (NATURE, vol. xvii. p. 485) I have had the advantage of seeing some of the work that Prof. Fleeming Jenkin is doing with his own instrument, which must, I think, be more sensitive than the one I examined. This work convinces me that the phonograph has already risen beyond the rank of lecture illustrations and philosophical toys, to which I assigned it in my last, and that it promises to lay some permanent foundations for the more accurate investigation of the nature of speech sounds. Prof. Fleeming Jenkin, by a most ingenious arrangement, which I must leave him to describe in his paper to the Royal Society of Edinburgh, obtains vertical sections of the impressions made on the tin-foil by the point of the phonograph, magnified 400 diameters. Some of these original tracings I had the pleasure of seeing yesterday, and they are full of interest. I have termed them "speech curves." They differ considerably from the phonautographic speech-curves of Léon Scott and Koenig, which only succeeded with the vowels, and from the logographic speech-curves of Mr. Barlow, which only succeeded with the consonants, in so much as they succeed with both. In such a word as *tah*, for example, intoned rather than sung, but not simply spoken, as the vowel would otherwise not last long enough for subsequent study, we have first the "preparation," in which the curve gradually, but irregularly, rises, then the "attack," where there is generally a bold serrated precipice, with numerous rather sudden valleys; next the "glide" where there is a perfect tumult of curvatures arising from the passage of voice through a continually changing resonance chamber, producing a rapidly and continuously changing but indistinct series of vowel sounds, which gradually settle down into the "vowel" proper. In the vowel, if well intoned, the curve remains constant for a considerable number of periods, beautifully reproducing itself, but, as the intoner becomes exhausted, "vanishing" away gradually to silence, the distinctive peculiarities of the curve disappearing one by one, till a dead level is again reached.

Then Prof. Fleeming Jenkin subjects this vowel curve to "analysis," reducing it to the separate "pendular" curves of which it can be composed. This corresponds to determining the "partial" tones (*parzialtöne, theiltöne* of Helmholtz, of which all but the lowest are called *oberparzialtöne, obertheiltöne*, and by contraction *obertöne*, whence the unfortunate English word *overtones*, which is constantly confused with *partials*, thus assuming a part for the whole) out of which the whole "compound" tone is formed. The first two partials are much stronger than the rest, the second often stronger than the first (hence the frequent confusion of octave?), the others generally very weak, although exceptionally one of the higher partials may be stronger. As many as five partials, as far as I remember, were traced out in the analysis Prof. Jenkin showed me, which he had just received from Edinburgh. The results differ materially for different speakers. Also there is a peculiarity in the "phase" with which the different partials enter into combination. Helmholtz showed that this difference of phase would materially alter the form of the curve, but would not alter the appreciation of quality by the ear depending upon the actual partials and their degrees of loudness alone.

The phonograph, as I have said, resembles rather a worn "print" than a "proof" of the human voice. This means, of course, that the delicate upper partials, on which all brilliancy depends, are absent. In some respects this is advantageous for the very elaborate inquiry which Prof. Fleeming Jenkin has instituted, for it enables him to catch the bold outlines on which genera depend, without being at first bewildered by the delicate details which give specific differences. Our speech sounds are, of course, individual, and what is recognised as the same speech sound varies in the same speaker within the limits of its genus, almost every time it is used. We shall do much if we establish the genus. The extent of Prof. Jenkin's researches, as he contemplates them, and the care with which

his initial experiments, tracings, and analyses have been conducted, lead us to hope that we have at least got an instrument which will enable us to solve the elementary problems of phonetics that have hitherto almost baffled us, although it is not suited, as yet, to fix those delicacies of utterance which were my own special object of investigation.

April 30 ALEXANDER J. ELLIS

ON repeating the experiments with the phonograph narrated by Mr. A. J. Ellis in NATURE, vol. xvii. p. 485, upon a different instrument, I have found the results of my experience to differ in several respects from his. Doubtless each instrument possesses its own individual characteristics; hence it will be the more needful to exercise caution with respect to generalisation, especially as the existing instruments are few and in the hands of few observers. Mr. Ellis has been careful to state the nature of the instrument with which his results were obtained, and the name of Mr. Stroh is a guarantee for the construction of the mechanism. The instrument with which I have been working is of homelier make, and not provided with a driving-train or governor, but simply turned by hand. The same disc—a three-inch ferrotype plate—serves as receiver and transmitter of the voice. The foil used has been, if anything, a little too thin for the purpose.

On trying the sounds *aabaa*, *aadaa*, &c., I found the consonants clearly distinguishable, except the sibilants. *Aajaa*, which is stated by Mr. Ellis to be faultily delivered by the instrument, was perfectly recognisable, and could be distinguished from *aadaa*. Neither was there any confusion between *jack* and *dack* or *tack*; but *jacques*, with the soft *j*, was sounded out by the instrument as *hāāk*. My phonograph makes the clearest possible difference between the words *bout* and *bite* when carefully spoken, the diphthongal sounds coming out beautifully as *bāāōōt* and *bāāēēt*. On reversing the motion of the handle, *tēēāāb* and *tōōāāb* were unmistakable. The double nature of some of our consonantal letters is very clearly demonstrated by this process of reversal of motion, as Messrs. Fleeming Jenkin and Ewing have already shown. To the sounds they name let me add that of *ch* in the word *cheque*, which we ordinarily pronounce *tshek*. This word gives a very peculiar sound when reversed in the machine.

The greatest difficulty—that of getting an instrument to acknowledge the sibilants—is a difficulty that all who have worked with phonograph, phonautograph, or telephone, admit. The remedy mentioned by Prof. Mayer, that of using a mouthpiece with a very small hole, has the inconvenience of diminishing materially the loudness of the articulation of the machine. I have found it better to fasten a strip of card or watchspring across the opening, edgeways, so that the voice impinges on the edge of the strip. With this device sibilants are improved; the word *scissors* becomes practicable, though "*Scots*" is still intractable. One of Mr. Stroh's instruments, which was shown at the Crystal Palace during Easter week, gave *s* and *z* fairly. In a familiar phrase the *s*es are not much missed: *Steady, boys, steady*, is given with less marked defect of speech than if uttered as *thteady, boyth, thteady*. Another point of interest that has not, I think, been yet mentioned by observers is, that the marks corresponding to the vowel sounds differ when the mouth is at different distances from the vibrating plate, but that yet there is no difference in the vowel subsequently emitted by the machine; a result which confirms the previously known independence of the vowel sound of the phase of its component partials. For some time I thought my phonograph guilty of dropping its *h*'s (though not made within the sound of Bow bells), but when that letter is spoken rapidly in a word it is recorded faithfully. *Happy land* is well heard in the instrument; and *How do you do?* is also aspirated. Curiously enough, this sentence is spoken almost as well backwards as forwards (except the aspirate), especially if spoken to the machine with a strong Scottish accent. It is remarkable how useless an instrument without a clockwork regulator is for reproducing even the simplest airs: they are simply lost in noise. Altogether the study of speech by the phonograph is most interesting, and will furnish some most valuable data to students of language and of acoustics. It is impossible to witness its performance without a tribute of acknowledgment to the extreme ingenuity and skill of its inventor, Mr. Edison.

SILVANUS P. THOMPSON

University College, Bristol, May 1

On the Use of the Virial in Thermodynamics

THE ingenious experiment and the deductions from it, described by Mr. S. Tolver Preston in NATURE, vol. xvii. p. 31, throw a flood of light on the subject of availability of heat-energy, which altogether alters the basis upon which the hitherto imperfectly expressed conditions of the use of this form of energy will be made to rest. Mr. Tolver Preston has, in fact, discovered that discriminating "sprite," or being whom Prof. Clerk-Maxwell imagined ("Theory of Heat," 1875, p. 328) singling out the fast-moving, and separating them in a space by themselves (without any expenditure of energy), from the slow-moving molecules of a gaseous mass; or what is nearly equivalent to this, he has at least shown how *some* fast-moving and *some* slow-moving particles of a mass of gas originally in equilibrium, both as to temperature and pressure, *will naturally* be so guided amongst each other, that their joint energy will become more available than it was before. But it has, perhaps, not occurred to Mr. Tolver Preston and to some of your readers, that this power or faculty of rendering heat-energy available, which mutual diffusion of heterogeneous gas-masses, either through a porous septum or in their own contiguous layers possesses, is a consequence of the general form of efficacy belonging to force, of which Prof. Clausius pointed out the existence in his important propositions on the "virial,"[1] as he has termed one of the two members, of which this kind of mechanical tendency of force is the sum. The other member of a force's "*radiality*" (as it may be termed) "with respect to a given point," is the *vis viva*[2] of the material particle upon which it acts, in a space of which the selected point is the origin. In description of this newly-discovered natural tendency of a force with respect to a given point or focus, it is enough to say that while the statical *moment* of a force, or the product of the distance of its point of application from a point or fulcrum by the resolved part of the force perpendicular to this distance tends to increase uniformly the *moment of momentum* (defined similarly with that of *force*) of the particle upon which it acts, so does the "radiancy" of a force, or the product of the distance of its point of application from a given point or "focus," *together with the vis viva* of the particle upon which it acts, tend to increase the "radiancy of momentum" of the particle described in the same way as the radiancy (or the first term of the radiality) of the force, as just defined. We may speak of the radiancies of equal and opposite reactions, or of force-pairs, in the same way that we deal in statics with the moments of couples; with similar general properties of their equilibrium, including the resolution of the total radiancy (like the impulse, the horse-power, and the moment) of a system of forces, into an internal and an external part with respect to the centre of mass of a material system upon which it acts; and there are principles of conservation of moment and of radiancy of momentum about any point, taken as centre, of all the force-pairs whose moments and radiancies balance each other on a material system. Only the system's *vis viva* referred to the centre is in the latter case the rate of change of its radiancy of momentum relatively to it. It is in the same way that the conservation of the motion of the centre of mass, and the conservation of energy, are principles of nullity or of inaction of two other forms of force-agency balancing each other on a material system (the impulse of forces, and the product of their impulse by the virtual velocity of their point of application, or their "horse-power") to which we are obliged to have special recourse to resolve the particular varieties of questions of the "transfer of energy" which occur in mechanics. But it is remarkable that the radiality of a force-pair includes the *vis viva* of its mass-couplet as one member of its mechanical efficacy, and a surprising example of an agent (evidently the agent of heat-distribution) here presents itself in which *vis viva* itself is one of the active elements of the mechanical variation or compulsion! Its total tendency in any body acted on internally only by directly reacting force-pairs is the total *vis viva*, and the sum of the virials of these force-pairs, diminished, if the body is subjected externally to a uniform pressure normal to its surface, by three times the well-known product of this latter pressure by the volume of the body (written $-3\,pv$).

[1] *Poggendorff's Annalen*, vol. cxli. (1870), p. 124. But Clausius, it should be remarked, gives the name "virial" *to half* of the quantity which I have described below as the "radiancy" of a force. An exposition of Clausius' new mechanical expression, the virial, with an explanation by its means of the process of condensation of vapours into the liquid state, was given by Prof. Clerk Maxwell in his lecture to the Chemical Society on the molecular theory of the constitution of gaseous and other bodies, in 1875. (See NATURE, vol. xi. p. 357.)

[2] Using this word for *twice* the quantity usually described as a particle's "kinetic energy."

In the mutual inter-diffusion without change of temperature of two gases of different densities through a fixed porous diaphragm, although no energy is withdrawn from or communicated to either of the gases, the rate at which single molecules take their equal measures of gas volume through the partition being very different, like their velocities, the measure of gas-volume which accumulates on the side of the denser gas soon raises the pressure there, increasing the intensity of the mechanical tendency $3pv$ on that side of the partition, while the same kind of mechanical tension diminishes on the other, and the temperature on each side of the partition is at the same time unaffected. As the porous diaphragm by its immobility (which prevents one of the sets of molecules from doing any work upon the other) resists the resulting force upon it, its counter-tendency is entirely derived from the increase of its own external virial, which has sprung up (at no expense of work, supposing the diaphragm to be perfectly rigid) in maintaining everywhere in spite of their impacts the common temperature or mean energy of both sets of molecules. Were the diaphragm away, it is evident that the rapid flow of rare gas-volume across the confines between the two gases towards the denser side would cause the centre of mass of the gas-layer, in which the mixture begins, to move bodily away from the denser gas, just as the diaphragm would do if it were free to move; and the *bodily motion* so given to the medial gas-layer will, as a form of external radiality in the layer, arising from the heterogeneity, require, in order to be constantly neutralised in the whole body of the mixing gases, such a redistribution of their temperature and density to be taking place at every instant throughout the two bodies of gas placed in communication, that their centre of mass as a connected (but otherwise *isolated*) system may never undergo any change of place during the mutual diffusion. The space originally occupied by the rarer gas will accordingly become the hotter, and that by the denser the colder portion of the whole volume which the gases continue to occupy when they are mixed.

It is in the same way that we can explain the action of regenerators in such air-engines as Stirling's and Ericson's, in passing through which gases change their temperature and volume (and therefore their tendency or "radiality," $E-3pv$), at constant pressure, the counter-tendency being at the same time lodged or relaxed during the process in the regenerator, where it must be kept by non-conduction in the *tense* state (of actual heat-energy and virial combined) of "radiality" (corresponding to the similar "heat-tension" of the gas by which its heat-energy exchanges are secured. The property of the usual non-conductivity required in the regenerator, is one of indifference of the molecules of a substance to the radiality (or to the sum of the sensible heat and the virial), of neighbouring molecules, or in which different values of the quantity $E-3pv$ of small neighbouring parts of the substance equalise themselves with difficulty through the mass. But perhaps it is not the inter- but only the *intro*-molecular forces that furnish the radiality (and "virial") that determines the transmission of heat? If the former forces balance each other, which they do when the body is not vibrating by its elasticity, the virial of the intro-molecular forces only, together with the *vis viva*, may be "conservative" with regard to heat-energy, and may be employed in its transmission? Since radiancy of momentum is not heat-energy, we see that this natural effect of force radiality, or of virial and *vis viva* combined, can only be converted finally into actual heat-energy by some mechanism peculiar to the molecular structure of the solid and liquid bodies in which the heat-energy transmission takes place. Some kind of heat-engine apparently effects this process, for example, at the confines between the vapour and the liquid, when steam is condensed into water, but it is certainly a non-reversible one when the water-spray is colder than the steam which it condenses; and in the conduction of heat by solid bodies the process is also a non-reversible one; we only know the part which sensible heat, as temperature, or *vis viva*, acts in promoting heat conduction; and the virial by which it is perhaps also carried on, and which with *vis viva* conserves radiancy of momentum, may also be a fellow-regulator of the operation of which we have no certain knowledge, and over which we certainly have no direct control. But that it should invariably tend to lower the availability of heat, by heat conduction among the comparatively fixed molecules of liquid and solid bodies, will not, perhaps, when the internal motions of molecules are better understood, be more difficult to demonstrate from some theory of its action, than that it should sometimes serve to raise the availability of thermal energy by its action on heterogeneous gas masses. A. S. HERSCHEL

College of Science, Newcastle-on-Tyne

P.S. — Maturer reflection, since the first impression of surprise which Mr. S. Tolver Preston's announcement of the new experiment to which it relates caused me to express at some length, and perhaps unguardedly, in this letter has shown me that the properties of the virial, easily as they may conduct to some important results, do not, in this case, supply a complete solution of the problem of the final state of two gas masses at the same pressure but of two dissimilar densities on temperatures left to diffuse into each other in a confined space. The equal pressure on all parts of the inclosure predetermines the fixity of the centre of mass during the process, and consequently an unequal distribution of density, and therefore of temperature finally, when the mixture is complete; but the equation of the virial or the principle of conservation of the radiancy of momentum supplies no certain information what must be the law of this final distribution, one of its terms, $3pv$, or the virial of the inclosing pressure, being capable of undergoing unknown variations during the progress of the diffusion; and although the stationary condition of the mass at last implies that this term will not be permanently changed, yet both its value and that of the system's total moment of inertia round its centre of gravity (the *acceleration* of whose magnitude is the rate of change of the radiancy of momentum) may vary in the interval, with the result of leaving the latter moment of inertia permanently altered to an extent and in a way which cannot be defined. When in the simple case of a perfect gas the condition of the virial fails to afford positive information regarding the law of conduction and transference, or of rest and repose of heat in them under various distributions of temperature, it can hardly be expected that the same principle will furnish useful and definite results regarding heat-transmission through solids, and other kinds of bodies of which the modes of molecular aggregation are almost totally unknown. A. S. H.

Time and Longitude

THERE is an old and instructive problem which I have lately propounded to several people, and have been struck by the great variety of answers given to it.

Although we often lose sight of the fact, it is nevertheless true that any given day or year does not begin all over the world at the same moment, but, commencing first at some point in the east, it travels round westward with the sun, so that two different years are often coexistent at the same moment, and it is easily possible for two events to occur a few hours apart, and yet that which happened first to occur in 1878, and the later event in 1877. In the same way each day of the week starts somewhere to the eastward of us and dies somewhere in the west. Taking, then, any given day of the week as Monday, the problem is—When and where did last Monday first commence, where did it end, and how long did it exist? Or, to put a similar question, Where did the year 1878 first commence, and at what Greenwich time?

I will simply state my belief that last Monday commenced in New Zealand somewhere about noon on Sunday, but not at noon, its commencement at that time and place being in no way connected with its position as our antipodes, but being a mere accident of civilisation. If the whole northern hemisphere should become civilised and inhabited, the day would then almost certainly commence at Behring's Straits, and would last forty-eight hours. A person crossing Behring's Straits east or west would gain or lose a whole day just as he now does by sailing round the globe; so that he might easily cross over and spend a few hours of to-morrow with his friends and return in time for dinner, or might enjoy the New Year's Eve on two successive days.

If the Pacific Ocean became inhabited land, a meridian would have to be chosen as a starting point for the day, and a person stepping across this imaginary line would gain or lose a day. At the same moment that Sunday morning was commencing on the one side of this line, Monday morning would be commencing on the other, and there would be constantly two different days going on side by side with twenty-four hours' difference of time between them, though only a few yards apart. It would be possible for a person standing astride this line to have for an instant one foot in Monday morning, the other foot in Monday night, and his body in the previous Sunday.

I purposely avoid giving any reasons, and do not assert that all my views are correct, but I throw out the problem as an amusing one for argument and discussion, as it abounds in

apparent paradoxes. At the same time it cannot fail to be instructive. LATIMER CLARK

May 7

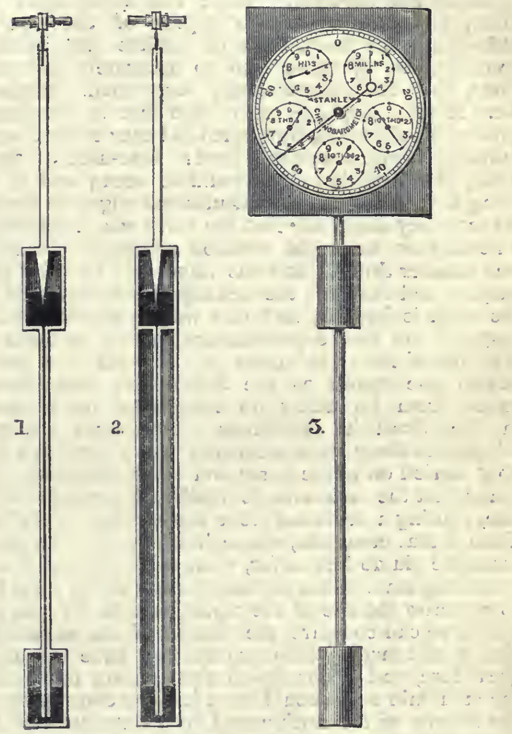
Cumulative Temperature

ATTENTION has been called in your valuable paper to the idea of registering cumulative temperatures by means of a pendulum, by M. von Sterneck, vol. xvii. p. 308, and this has called forth several letters. One gentleman has put forward my name as having devised means with some success. In an instrument exhibited at the Royal Society *soirée*, 1876, I could have left the matter resting at this point, but I am induced to write by the letter of your correspondent, "B," in vol. xvii. p. 486, who says, "The chief merit in this matter will belong to the person who puts the idea into a working form which can be proved capable of giving accurate results." As I think that I have fairly attained this end, or at least pointed out the way to it, with your permission I will describe the means which appears by the correspondence interesting to many of your readers. In my cumulative temperature clock the important element, the *pendulum*, is constructed as follows:—A steel cylindrical tube 32 inches long, 1¾ inch internal diameter, is hermetically closed at both ends. A rod is attached to one of the ends, which is placed uppermost, to connect this pendulum with the clockwork in the ordinary manner. An air-tight division is made across the tube or chamber at 5 inches from the upper end. A small tube leads from this division to the bottom of the chamber. A conical plug is inserted in the upper chamber, to be hereafter described. A screw plug is placed under the small tube in the outer tube to enable the upper chamber to be filled with mercury. When the pendulum is so constructed, the lower screw plug is removed, and the upper chamber and leading tube filled with mercury by means of a small funnel. In this full state the mercury is boiled, and the whole inverted. It then becomes a steel *barometer*. To convert it into a thermometer, a small air-hole is made in the outer tube (this is not shown in the engraving), and this hole is closed up with a small air-tight cock filled with a porous material. When this is screwed on and turned off, it is isolated from atmospheric pressure, and the mercury rises into the upper chamber by any increase of temperature causing expansion of air in the tube, and sinks in the same manner by loss of temperature, so that the pendulum becomes simply an air thermometer. The pressure of the air by expansion within the tube in the rising of the mercury changes the centre of oscillation of the pendulum and accelerates the clock, and *vice versâ*.

The clock is specially constructed to count beats only in units, tens, &c., up to ten millions, and the number of beats per day, week, month, or year, becomes the unit of temperature for the period. The exact length of time of each pendular oscillation being governed by the temperature at the time, the method becomes equal to one accurate observation at every second of time.

The difficulties of construction and refinement required upon this general description are of two kinds, mathematical and mechanical. The models that I exhibited at the Royal Society's *soirée* were imperfect, being of blown glass. The difference of oscillation per day for 1° Fahrenheit, was in these about 50, as taken at the Lambeth Observatory by the late Col. Strange. In the steel instruments described there would be about 100 oscillations additional per day for the rise of each degree centigrade. The mechanical difficulties are simply constructive. To obtain perfectly vacuum proof chambers, and to follow correctly the outline of the plug to be immersed in the vacuum-chamber. Also the adjustment of the correct volume of mercury, and the density of the contained air, by means of the cock, and the application of heat or cold to the outer case. The mathematical requirements are corrections. Thus: if the chambers were simply cylindrical, the mercury that rose by the pressure would have a different oscillation value for every point of space through which it rose. This might be corrected to equal scale value by making one or both the mercury-chambers conical, but it is much more simply done by inserting a conical plug in the upper chamber. There would also be a correction for the expansion of the mercury and the steel case, and from any irrationality in the expansion of the contained air. The whole of this correction being derived from heat might be made by one correction in the immersed plug. Prof. Stokes, Sec. R.S., kindly offered to calculate the exact form of this plug for me from data I was to supply. But I was ill shortly after this, and unable to attend to the matter, so I let it drop, but have the clocks and pendulums ready to complete some time hence.

I send a diagram engraving which shows the principle of the pendulum No. 2, for cumulative temperatures. No. 1 is for



taking cumulative pressures upon the same system, if the science of meteorology should require such exact means of obtaining permanent records of pressure and temperature for long periods as for months or years. WM. F. STANLEY

South Norwood, April 22

THE INTERIOR OF THE EARTH[1]

SIR GEORGE AIRY remarked that the nature of the subject was different from any upon which he ever lectured before, in regard to its indefiniteness and to the difficulty he should have if he considered it to be his duty to lead them definitely up to some point. He could only give them some idea of the theory to which he wished to lead them, and in doing so he would advert collaterally to a good many points which might be valuable. He proposed to divide his address into three parts. The first would relate to the measures of the earth; the second to observations on temperature; and the third to the manner in which they might suppose the earth to have been formed, especially with regard to the nebular hypothesis; and after that he would add some remarks on the conclusions to which these lead.

He described the process called triangulation, by which a large part of the contour of the globe is covered, and by which it is possible to lay down a map on which the distance between any one point and any other point is ascertained to within a few inches; how that this was valuable in ascertaining the dimensions and figure of the earth with the aid of the zenith sector, an instrument for measuring the apparent distances of stars from the point overhead. He showed on a large globe the principal lines of measurement which had up to this time

[1] Abstract of Address at the Cumberland Association for the Advancement of Literature and Science, by Sir George B Airy, K.C.B., F.R.S., Astronomer-Royal. Revised by the author.

been made for this purpose. From these measurements, there was no doubt that the earth is very nearly a sphere of 8,000 miles in diameter, or 25,000 miles in circumference. When he spoke of the surface of the earth, it must be understood that he spoke of the sea-level. Above that level stand mountains, and below are the depths of the sea. But although these inequalities of surface are taken into consideration by those who go accurately into calculations, they are comparatively very small. Suppose he were to make a sphere twenty-five feet in diameter, representing the earth, how much did they think the mountains would rise above the level? One-fifth of an inch. Well, of course that never could be seen; and it was a thing that in all ordinary calculations might be neglected. So that they might say that the earth was a sphere, with an exception he would mention presently. Then there was another thing which was important to their present subject, and that was the density of the matter of which the earth is formed; and this was a matter which had engaged the best experimenters in two or three ways. The first of the experiments of this kind is a very celebrated one known as the Schihallien experiment, so called from its being an experiment on a mountain in the Scottish Highlands (Perthshire) particularly adapted to these measurements, which are most favourably carried on in the north and south direction. It was found that the mountain Schihallien disturbed the plumb-line, causing a deviation from the vertical of 11″ or 12″. Then if that mountain, whose dimensions we can measure, turns the plumb-line so far, what is the proportion of its attracting mass to the attracting mass of the earth? And as we know the size of the mountain and the size of the earth, we can compare the density of the mountain and that of the earth. This process was gone through with great care, and it was found that, taking the density of the mountain as we could trace it by its constituent rocks, the density of the earth would be about four and a half times that of water, or about twice the average density of the surface rocks. The earth had density everywhere, but was more dense towards the centre than the outside. The next experiment is known as the Cavendish experiment. Here was a very light rod of deal, six feet long, suspended by a fine copper or silver wire (which is the most delicate suspension we can have) forty inches long, within a wooden case to defend it from currents of air. At each end of the lever was hung a ball two inches in diameter, and by a simple contrivance a pair of leaden spheres, weighing together perhaps 300lbs., were brought simultaneously into the neighbourhood of the balls (but outside the case), on opposite sides, so that they might attract the small balls; and the experiment was varied until, by a series of calculations, the density of the earth was ascertained, and gave a greater result than before, namely, that the average density of the earth was about 5½ times that of water. Then the third experiment was one which he made himself in the Harton colliery, near South Shields. That was by seeing how much the force of gravity was altered by going to a great depth, the force of gravity being ascertained and compared at the top and bottom by the swinging of a pendulum. From that a calculation was made, and it gave the density of the earth as six times that of water. He believed the best calculation was that founded upon the Cavendish experiment, and was quite willing to take something like 5½ times the density of water as the average density of the earth, including every part of it. There were consequences which followed from that which were certainly very striking. As this density was rather more than double that of the surface rocks, it showed that towards the centre the earth was more condensed than at the outside. But there was one result of the calculation which rather startled him when he made his own experiment on the subject. Since these rocks press upon each other more and more the further you go down, what is the pressure upon the square inch when you approach the centre of the earth? Many gentlemen there would have heard of a pressure of 50 lbs. or 100 lbs. on the square inch, and perhaps the greatest pressure we know is that by which tough Aberdeen granite is crushed—10,000 lbs. to the square inch. But it must be 30,000,000 lbs. to the square inch in the centre of the earth; and it is an astounding thing to imagine what consequences may follow. We have no idea of any such degree of pressure, and cannot therefore conceive what its consequences may be. Perhaps thereby gas may be squeezed into gold or platinum, and powder to solid, or solid to powder—we cannot tell what it does. That enormous pressure, and our total ignorance of it, is one of the difficulties and troubles of this case. He thought the general state of the earth would be understood from what he had said, and now he came to the rotation of the earth. The earth revolves, as everybody knows, in the course of a day; and everybody knows also, from the housemaid who whirls her mop to the greatest philosopher, that rotation will swell out the middle of the earth. Calculations have been made upon that, and the result is that the diameter of the earth in the equatorial direction is greater by about 1-300th part than the diameter in the polar direction. When they found that the measurement of the dimensions of the earth agreed so well with that conclusion, it led them to the further conclusion that the earth is, or has been, in a fluid state. In corroboration of this, he would mention a singular circumstance which occurred in our Indian Survey. In proceeding northward from Cape Comorin, the curvature of the earth agreed very well for many hundreds of miles with that found in other parts of the earth (with due reference to the elliptic form of the earth). On approaching the Himalaya Mountains, the plumb-line was sensibly attracted by the mountains. The late Archdeacon Pratt investigated, from the form of the mountains and the density of the rocks, the disturbance of the plumb-line, and found that it ought to be much greater than it really is. Sir George explained this by supposing that the whole of that country is floating upon a dense fluid, and that the thick mass of the lighter mountain-matter sinks deep in the fluid, and that the displacement of denser matter neutralises almost entirely the attraction of the lofty mountains. The form of the earth is not such as would be taken by a solid structure, but such as would be taken by a fluid mass with solids floating upon it.

In the second part of his address, Sir George Airy referred to what is known about the temperatures. They knew something of the rate at which temperature travels through the earth. The experiments on this point had begun, as many good experiments have begun, with the French, who fixed thermometers with very long stalks to the depth of twenty-five feet in the ground. These experiments were followed up, after some time, with similar thermometers at the Observatory at Edinburgh, and about the same time at the Observatory at Greenwich, and there the deeper thermometers were read every day. The first and most conspicuous result of these experiments is the retardation of the seasons. At the depth of twenty-five feet, high midsummer heat occurs at December, which shows that it takes five months for the heat to travel down that depth. If you compute it further, it takes 100 years to travel a mile; so that if the crust of the earth is 100 miles thick, it will take 10,000 years for the transmission of heat through it. This showed that really, after all, we may have a great deal of heat below us, and that it will not come to us for a very long time. It will come at last, but it will come travelling up slowly, and in the meantime the radiation from the surface of the earth will carry it off very rapidly. So that it is quite possible that with a cool surface there may be a great deal of heat below. In every part of the earth there is evidence of intense heat in former times.

The extent of volcanic action is partly lost on the earth by the effects of air and water; but when they looked at the old rocks, they found there had been volcanic action almost everywhere. In our limestone rocks, for instance, there are the basaltic veins, which in some parts go by the name of toadstone, which are certainly the result of volcanic heat enough to produce fluidity. Almost everywhere they found that there were volcanic streams intermixing with all the rocks; and even although the surface of the earth had been free from volcanoes in a given district for a time, yet there had always been volcanic action very near, enough to force in veins of lava from time to time. It seems, therefore, that we are entitled to say that we have always been near a great deal of heat—probably we have been much nearer it than at the present time, but still we are near enough to experience a great deal even in these countries. Repeated experiments have been made on the increase of temperature as you go down in mines, and the conclusion has been come to that the temperature rises one degree Fahrenheit, sometimes in sixty and sometimes in 100 feet. There is a mine in Cornwall in which he had walked in a stream of water at the bottom actually scalding to the legs! and everybody knows what quantities of water there are in the hot springs. And then there is the great display of the volcanoes, which come from a great deal of heat somewhere; and in places where volcanoes are extinct we can trace a sort of basaltic continent, so to speak, up to the very mouths of the craters from which the lava has come. So that there has been in all former ages undoubtedly much more heat than at present. There was another matter on which he would desire to speak, but with no great boldness, and that was the change in magnetism. The subject of terrestrial magnetism is one of the most obscure in the world; nevertheless, looking at the direction in which it always is towards the colder parts, and tracing its general phenomena, it may be effected by thermo-electricity, and that may be produced by the constant wear going on in the interior of the earth, where the fluid lavas are consolidating themselves. Within a few years the voyage of the *Challenger* has been made, and he had little hesitation in saying it was one of the most important in the scientific history of the world. In crossing the great seas they sounded to great depths, and measured in a satisfactory way the temperature of the water down to the depth of five miles. They always came to cold at the bottom; and there are great controversies whether the cold can come in deep sea streams from the frozen regions of the north. He thought that had some influence; but he thought the bottom of the water and the ground at those great depths is cold—he did not think that part of the earth partakes of the same heat as other parts; that he only expressed as his opinion, in which, of course, he might be met by the disbelief of a great many persons. That was the state of things as we know it regarding the temperature of the earth—that there is evidence everywhere that there has been enormous heat almost all over the earth. Some parts of the crust of the earth under the deepest seas are still perforated by volcanic islands. In some places the heat comes very near the surface. That he looked upon as an important fact, leading them to a theory of what the state of the earth really is.

On entering upon a matter which was undoubtedly one of the boldest speculations in modern science, which was the formation of the earth—he could not say its creation, but the way it got into its present shape—he had to premise that the theory on which he had to speak, which is known as the nebular hypothesis, is the conception of a very bold and vigorous intellect indeed. Laplace it was who remarked that all the planets and satellites revolved in the same direction round the sun, and all of them turned on their axis in the same direction: and it was difficult to deny that there must be some general cause for this. It naturally occurred to Laplace that if we can find something which is contracting its dimensions, and which has a little rotation to begin with, then with every contraction of dimensions that rotation would become more rapid, till it might go to any degree, depending upon the condensation of its various parts and its density before. Then can we come to look at any matter which is being thus condensed, and which might so form systems such as ours, with sun, planets, and satellites? There are a series of bodies in the sky which did not attract much attention in former days, mainly because telescopes were not so large, but which are now catalogued by thousands. These are the nebulæ. The name denotes their cloudy appearance. They are small bodies among the stars, sometimes appearing to have stars in them, or to be connected with stars, and sometimes not. They have the strangest and most capricious shapes imaginable. If this nebula is contracting its parts together so as to form a world, that rotation in the course of condensation will become so rapid that it may form suns and planets and earths around it; and on this supposition there is no difficulty in making a complete solar system cut of such a mass as that of the nebula in Orion. Observations made lately by the largest telescopes—those of Lassell and Lord Rosse, both of which are remarkable telescopes of the largest class—have brought to light a number of nebulæ possessing a spiral appearance; and they seem to have some bearing on the supposition that the nebulæ are contracting and getting into a rotatory state. But these changes go on so slowly that they had not been able to answer with certainty for any of the changes of which he now spoke. The whole thing is theoretical, and yet, as it seemed to him, in the highest degree probable. Supposing this to be the case, these nebulæ would rotate, and in their compression would get very hot. There is no doubt that condensation would produce enormous heat, and it seems we have there sufficient explanation of the great heat we find below the surface of the earth and in other places. We suppose that the stars generally have been formed from the condensation of nebulæ; and there is a circumstance which was worthy mentioning. A series of observations founded upon optical experiments has come to light within late years which has done more to reveal the secrets of nature than anything before—this was by means of the spectroscope. By voltaic action sparks may be produced which derive their character—sparks like those of an electrical machine—in a great measure from the metals from which they spring. A spark springs from metal to metal, and the character of the metals gives different characters to the sparks. We have one set of these spectra produced by iron, another by nickel, others even by hydrogen gas, and so on, and these are observed and catalogued with great care. When we come to observe the light in the stars in the same manner, we find there are no two stars alike; some of them have the same spectra as that given from iron, and others have spectra from a number of different things; and we are actually able, by legitimate reasoning from this, to say from what the stars are made—what metals and other things they are made of, and, as a generat thing, there are no two stars alike. So that in this nebular hypothesis we are not bound to say that the nebulæ are all of the same materials, and we conceive that by comparing the bodies which we know in the solar system with those of the stars, we may arrive at an idea of the variety of materials of which the planets are composed. We cannot find anything different in comparing the light of the planets, because they all derive their light from the sun, and they do not present any difference of appearance in the spectrum. But we can draw conclusions from their relative density. As he had said to them, the average density of the earth is probably five and a half times that of water. They knew

that the sun is only once that of water. What the sun is he could not tell, but it is a very poor light creature indeed. The density of Mercury is perhaps rather greater than that of the earth. The density of Venus is much the same as that of the earth, and the density of Mars is also much the same as that of the earth. Then after that comes a shower of little planets, about 200 of which have been observed up to the present time, and he could not tell what they are made of. Then there are Jupiter and Saturn, which are no heavier than water. So that it appears clear that, assuming the formation of these things by the condensation of nebulæ, on the theory he had mentioned, the different parts of the nebulæ which have contributed to the solar system are very different. Well, that being considered as established, it follows that in the constitution of our earth there may be parts of very different density. He should say that the high and prominent parts of the land are made of something light, and the heavy and dense parts are those covered by a considerable quantity of water, which have sunk deep into the central lava on which, he conceived, all things are resting.

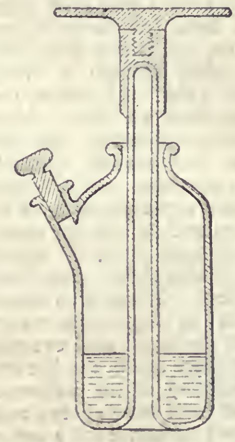
And now he had come pretty nearly to the end of his theory, and he would show them what he feared they would call an absurd representation of what he conceived the state of the earth to be. [The lecturer drew attention to a diagram of an "ideal earth," roughly showing his theory—some parts of the crust of the earth being thick and coloured darkly to indicate density; some thick and not so dense, and all admitting of volcanic eruptions from the interior, which was represented as lava.] Remember that everything here is exaggerated. It is not intended to be a correct representation. It is a caricature of the most extravagant kind; but if it conveyed to them the broad ideas that had impressed themselves upon his mind, it would be doing the right thing. He thought a large proportion of the centre of the earth is fluid and hot, and he thought that upon this there were certain divers classes of something like solid matter. In all these parts there are cracks or chinks through which volcanoes burst out where the cover of the earth is very thin. In some places you have two or three volcanoes together. There is one instance in Europe, where we have Etna, Stromboli, and Vesuvius. In this diagram he had condensed to the best of his conjectural power his supposition as to what the state of the earth really is; and if any one chose to find fault with it he would not quarrel with him. He only gave it as a sort of inference from a number of things he had said.

A NEW INSULATING STAND[1]

SIR WILLIAM THOMSON has frequently dwelt on the great importance of insulating, with the utmost care, any apparatus intended for researches relating to static electricity; he has shown that the atmosphere and other gases have but little effect in dissipating an electric charge, even when moist, and that it escapes mainly in consequence of the deposition of a layer of moisture upon the insulating supports which renders their surface conducting. In all Sir W. Thomson's electrometers there is an arrangement for drying the insulating surfaces by means of sulphuric acid, either free or absorbed by pumice. This method admits of very general application:—Any body, as for example apparatus constructed for the observation of atmospheric electricity, may be most perfectly insulated by supporting it on glass rods inserted in glass cylinders containing free sulphuric acid or pumice moistened with it. In order to do this the lower end of the rods must be either inserted into cylinders of lead or else fixed to the bottom of the jar by means of a substance not acted upon by sulphuric acid, for example, melted sulphur or paraffin; melted sulphur is liable, on account of its temperature, to crack the jars, notwithstanding the precaution of previous heating; paraffin, on the other hand, softens in the course of time, and the glass rods do not retain their vertical position. Notwithstanding these disadvantages excellent insulators may be thus extemporised as occasion may require.

[1] By M. E. Mascart, Professor of Physics, Collége de France, Paris.

For permanent use it is advantageous to employ insulators specially constructed, as shown in the accompanying figure; it consists of a bottle having a narrow neck,



through which passes a tubular continuation of the bottom, about 4 mm. less in diameter than the internal diameter of the neck, so as to leave a space of 2 mm. (about) between them. The top of this hollow rod is closed, in order that a brass tube may be cemented upon it, into which may be screwed any apparatus, as, for example, a disc as shown in the figure, a sphere, a crutch on a hoop, &c., &c. In the shoulder of the bottle is a neck, closed with a ground-glass stopper, through which sulphuric acid may be poured, in the first instance, and renewed from time to time. As the space between the hollow rod and the neck of the bottle is very small, the air in the bottle does not change very rapidly, and the sulphuric acid remains efficient for a long time. It is only necessary to run off a portion of it occasionally by means of a siphon, and to add fresh; as this may be done without disturbing the apparatus, the insulation may be maintained for any length of time. Moreover, for an insulator to be used occasionally, an addition is made of a vulcanised rubber cap, which slides on the glass rod to close the neck of the bottle when not in use.[1]

A double pendulum of pith balls supported by such an apparatus maintains its divergence, after being charged with electricity, for a very long time, even in a theatre filled with an audience. One may show by a simple experiment the great efficacy of this apparatus in comparison with insulators of glass exposed to the air, even when carefully varnished with shellac. If a pair of pith balls, suspended by a thread of cotton, is hung upon the latter support, and the metallic foot is placed on an insulator, and connected with a charged condenser, no divergence of the pith balls occurs in the first instance, but little by little the electricity is propagated along the glass rod, and then the threads near the support begin to separate, and soon after the balls diverge and remain at a certain distance from each other.

The electrometers of Sir Wm. Thomson are sometimes so perfectly insulated that the loss of a charge of electricity does not amount to $\frac{1}{100}$th part in twenty-four hours. By means of the insulator described above, one may obtain an insulation of like order for *bodies supported in the open air*, and thus diminish to a great extent one of the chief sources of error usually met with in experimenting with static electricity.

[1] These may be obtained of various sizes, one litre, half-litre, quarter-litre capacity, at Alvergniat Frères, 10, Rue de la Sorbonne, Paris.

RHEOSTATIC MACHINE

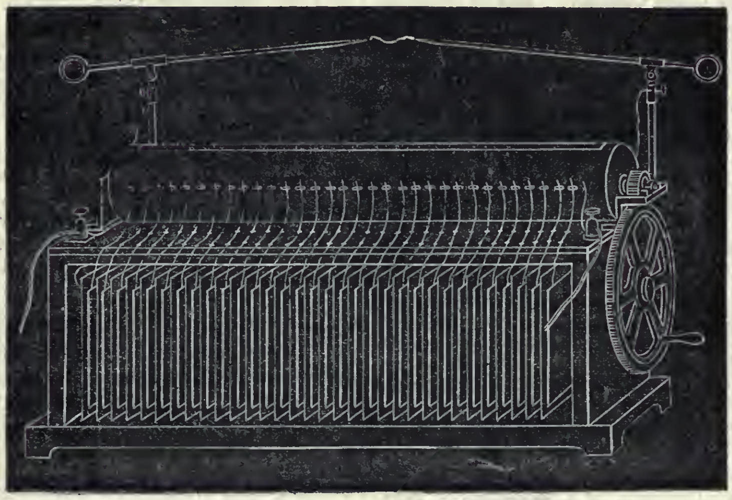
IT is known that Franklin made use of a series of Leyden jars or fulminating plates, arranged in the form of a cascade, to obtain strong discharges of static electricity; that, on the other hand, Volta, Ritter, Cruikshank, &c., were able to charge condensers by means of the pile, and that these results gave rise to researches, conducted both by calculation and experiment, on the part of a great number of physicists.

I have been led to study, in my turn, the static effects of voltaic electricity, by means of a secondary battery of 800 couples which I at present possess; and I have devised an apparatus which shows the intensity that these effects may acquire.

After having proved how easy it is with this battery to charge rapidly an insulating plate condenser, sufficiently thin, of glass, mica, guttapercha, &c., I combined a certain number of condensers, formed by preference of mica covered with tinfoil, and arranged them as couples of the secondary battery itself, so as to be easily charged in *quantity*, and discharged in *tension*.

All the pieces of the apparatus must be carefully insulated. The commutator is formed of a long cylinder of hard caoutchouc, provided with longitudinal metallic bands, intended to unite the condensers at the surface; and traversed at the same time by copper wires, bent at their extremities, for the purpose of uniting the condensers in tension. Small plates or metallic wires formed into springs are placed in connection with the two armatures of each condenser and fixed on an ebonite plate on each side of the cylinder, to which a rotatory movement can be given.

If we put the two sides of the apparatus into communication with the secondary battery of 800 couples, even several days after having charged it with two Bunsen elements, and if we set the commutator in rotation, we obtain, between the branches of the excitator, on which the armatures of the extreme condensers abut, a series of sparks entirely similar to those given by electric machines provided with condensers. By employing an apparatus of only thirty condensers, each of three square decimetres of surface, I have obtained sparks four centimetres in length.



The tension of a secondary battery of 800 couples is not necessary to produce marked effects with this apparatus. By putting in action only 200 couples, we have sparks of eight millimetres, and we may, without doubt, by diminishing still more the thickness of the insulating plates and multiplying the number of condensers, obtain effects with a source of electricity of less tension.

It is to be remarked that the discharges of static electricity, furnished by this apparatus, are not in directions alternately positive and negative, but always in the same direction, and that the loss of force resulting from the transformation must be less than in the induction apparatus; for, as the voltaic circuit is not closed a single instant on itself, there is no conversion of a part of the current into heat.

We may maintain the apparatus a long time in rotation and produce a considerable number of discharges without the secondary battery appearing sensibly weakened. This is because each discharge employs only a very small quantity of electricity, and because, as above stated, the circuit of the battery is not closed by a conducting body. The electricity of the source simply spreads over the polar surfaces presented by all the condensers, in proportion as they are discharged. This emission constantly repeated must nevertheless end by discharging a certain quantity of electricity; and when the instrument is charged by a secondary battery, we must ultimately exhaust, under the form of static effects, the limited quantity of electricity which the current of the battery can furnish.

Thus then, by another method than that of induction, properly so-called, by means of a simple effect of static influence renewed without cessation, we effect the transformation of dynamic electricity, so that this apparatus may be designated by the name of "rheostatic machine."

GASTON PLANTÉ

GEOGRAPHICAL NOTES

THE Berlin Geographical Society celebrated in characteristic German fashion the fiftieth anniversary of its foundation last week. Berlin, as our readers know, is not the only German city possessing a geographical society; indeed it has two. In Hamburg and Bremen are two excellent societies of this class, while the Continent, generally, is overrun with them. Russia has about a dozen, Belgium has at least two, Brussels and Antwerp, Holland one if not more, France at least half a dozen, Italy two or three, and the Scandinavian countries their own share. We do not consider it a disadvantage that in maritime countries there should be more than one geographical society, and we think it might be beneficial if even in our own country associations corresponding to the French societies of commercial geography were established in our chief ports, Liverpool, Glasgow, Bristol, Leith, Dundee. These might be branches of or affiliated to the London society, and might catch much that never reaches the latter. They might, moreover, do considerable service in encouraging the

merchant service to obtain and bring home information that would be useful to science, and might, by means of lectures and otherwise, foster a scientific spirit among our commercial population. Much good is done in this way, we believe, by the societies of Marseilles, Bordeaux, and Lyons. Two new geographical societies have, we learn, been established in France, at Metz and Montpellier. The French are evidently doing their best to remove the reproach so frequently cast at them, of being more ignorant of geography than even the English.

THAT the Continental societies go in for earnest work is evident from the weighty journals published by most of them. The *Mittheilungen* of the Hamburg Society for 1876-77, for example, is a thick volume of 400 pages, containing a number of papers of considerable scientific value. Besides several papers on Central and South America, there is a long series of letters by Dr. Pfund, filling nearly half the volume, written during his travels in Kordofan and Darfur, along with Colonel Prout, of the Egyptian staff. Other African papers are by Dr. Paul Ascherson on his travels in the Lybian desert in 1876, and one of much value by Herr Fischer, on the present condition of the Galla Country. In the *Deutsche Geographische Blätter*, the organ of the Bremen Society, Dr. Oskar Lenz discusses at length the trade conditions in Equatorial West Africa, with special reference to Stanley's discoveries; Dr. Lenz does not believe that the Ogovai is connected with the Congo. Mr. W. H. Dall is contributing to this journal a series of papers on his own and other recent researches in the Aleutian Islands, while Dr. A. Ziegler has an interesting paper on Regiomontanus and Martin Behaim. Turning to Italy the energetic Roman Society has begun the publication (apart from their always interesting *Bolletino*) of *Memorie*, containing at length the most important papers read at the Society's meetings. The first part contains a lecture by the president, Signor Cristifero Negri, on scientific geography, which shows what has more than once been said, that geography is really the meeting-place of all the sciences. Then there is a paper on the geographical distribution of camels, by Prof. Luigi Lombardini, and a well-arranged series of instructions to explorers by various specialists, edited by Signor A. Issel. Nor must we forget the American Society, with its seat at New York, and which is the medium for a good deal of valuable information that might not otherwise reach the light of day. Chief-Justice Daly's presidential address always contains an admirable and exhaustive summary of the year's work; and this year it is quite as full and interesting as usual, nothing in the domain of geography of any importance remaining untouched, special prominence being of course given to the various surveys of the United States. Thus it will be seen, that under the name of geography, much varied and really valuable work is being done, and that dilletanteism has really but a small place in it, at least abroad.

AN expedition, comprising twenty-five miners and others, has started for New Guinea. This news is telegraphed from Sydney, and we earnestly hope that the expedition is under proper direction, both for the sake of the natives, who have so far been friendly to white men, and for the sake of further scientific discovery.

THE TRANSIT OF MERCURY

THE weather on Monday was so unfavourable that the observations of this interesting phenomenon were mostly unfortunate in England. In France some valuable observations seem to have been made. Our Paris Correspondent writes that the observations taken by M. Janssen at Meudon Observatory were wonderfully successful considering the state of the atmosphere. He was able to make use of spectrum analysis in order to determine the composition of Mercury's atmosphere. He was able to see Mercury before it had begun to make its first entrance on the disc. This observation is a confirmation of the phenomena observed in 1874 at Yokohama on the occasion of the Transit of Venus. Two photographs are excellent, and will lead to a determination of the diameter of the planet. At the Paris Observatory the transit was also seen.

When Capt. Mouchez saw Mercury the disc had been indented to the extent of 2″ of degree, about ⅛th diameter of Mercury. When it was seen by the brothers Henry it was half on the disc. The difference of time is about 10″ later at the National Observatory. The brothers Henry also saw the interior contact at about 3h. 23m. and some seconds. The exact time cannot be given yet. The contact was decidedly bad owing to the clouds.

At Algiers and Bordeaux the observations were bad. At Ogden, Utah, United States, the delegates sent by the French Government, M. André, of Lyons, and M. Angot, of Paris, obtained seventy-eight photographs of the transit. Satisfactory observations and photographs of the transit were taken at the Government Observatories at Washington and West Point, U.S.

Mr. J. J. Cole writes to the *Times* from Mayland, Sutton, Surrey, that the sun was clear from 3.5 to 3.25, and the whole ingress was steadily observed with a refractor of 6 inch aperture and three others smaller. The Greenwich mean times of external and internal contact were taken, and were confirmed by Mr. Bawtree near with unexpectedly small differences.

At Aberdeen the transit was observed by Lord Lindsay, Mr. Ranyard, Dr. Copeland, Mr. Carpenter, and Herr Lohse, and photographed by Mr. Davis. A thin cloud covered the sun at the time of first contact. No ring of of light was seen round the part of the planet off the sun's disc. External contact was observed spectroscopically by Lord Lindsay, who detected the approach of the planet by the eclipse of the C line thirteen seconds before its limb encroached upon the continuous spectrum of the photosphere. Mr. Ranyard observed the continuous spectrum below C line, but saw no trace of the planet until it was on the sun's disc. No change in the solar spectrum was observed at the limb of the planet. Dr. Copeland, Mr. Carpenter, and Herr Lohse obtained both contacts and measures of diameter.

Mr. C. G. Talmage writes as follows to the *Times* from Mr. Barclay's Observatory, Leyton, Essex:—

"Owing to the prevalence of clouds the times of external and internal contact at ingress were not observed here. The first view I obtained was at 3.43, when Mercury had advanced some considerable distance on the sun's disc. The duration of clear sky was then so short that there was not sufficient time to obtain micrometrical measures of distance from the sun's limb. For about eight or ten seconds the sky was absolutely clear, and then I noticed that Mercury was surrounded by a bright ring, darkening off to the periphery, which was exceedingly well defined. The distance between the limb of Mercury and periphery of ring was about two-thirds of the planet's diameter. I used the full aperture of ten inches, with a diagonal power of eighty."

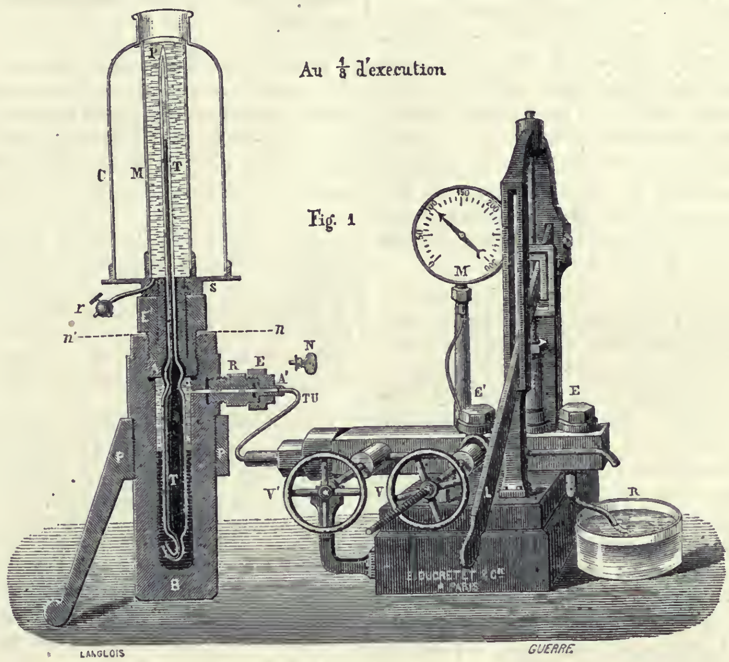
DE CAILLETET'S APPARATUS

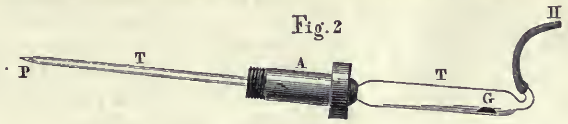
WE have already (vol. xvii. p. 265) spoken at length of M. Cailletet's method of liquefying the last of the gases, and at the same time we referred to the fact that students of science in France had not been forgotten by the accomplished experimenter. We described briefly a portion of an apparatus for use in laboratories for this experiment, and are now able to give an illustration of the complete laboratory apparatus as manufactured by Ducretet and Co., of Paris. The figure shows the apparatus one-eighth the size of reality.

To work this apparatus it is necessary to take off the liquefying tube T and all the pieces of the upper part; also the lateral screw E′ and its tube A′; then, after having screwed on to the joint R the piece N, which serves as a stop-valve, the mercury should be turned dry and quite pure, into the wrought-iron reservoir B, up to the level of the edges N N′. The sides of this iron reservoir are very resistant and are able to support strong pressures.

The tube T having been filled with gas for liquefaction, it is gently forced into the mercury of the reservoir B; the part N being taken out, the mercury which flows out is collected. When the tube A rests on the leather of the bottom of the length of the reservoir, the screw E′ is re-screwed very tightly. The apparatus is inclined a little to get rid of the excess of mercury, in order that its level may remain below the lateral hole by which the pressure is introduced.

The support S with refrigerating envelope M is then re-screwed on the upper part of the *ajutage* A; it rests





upon leather. The safety-bell-jar C is movable; it is intended to stop the pieces of glass should the tube T be broken. The stop-cock *r* lets the water flow from the envelope M. The lateral screw with the tube A′ is re-fixed, to which is soldered the small metallic tube T V, by which pressure is introduced.

The hydraulic pump, which Ducretet and Co. have constructed specially for this apparatus, is for the purpose of compressing water about the mercury contained in the reservoir. The two valves E E′ may be introduced by the orifices closed by the screws E E′; the valves may be thus tested and easily changed without undoing any part. The reservoir of water R is placed outside; it is then easily seen to and kept full.

Before setting the hydraulic pump to work, we withdraw as completely as possible the screw-plunger piston V by moving the fly-wheel. The action of the lever L enables us to obtain easily a pressure of 200 atmospheres. This pressure may then be increased by the gentle introduction of the plunger-piston V. The liquefying tube T is of thick glass; it has a resistance of about 400 atmospheres, but it is better not to exceed a pressure of 300 atmospheres. The second screw V′ is intended to produce expansion.

NOTES

ONE of the most effective methods of acquiring a headache is a good round of sightseeing, especially in a museum, collection, or picture-gallery; it is quite a comfort to get among a collection of any kind, the sight or catalogue of which does not make one ill by anticipation. Happily the headachy feature is generally absent from the collection of objects exhibited at the Royal Society *conversazioni*, and in this respect and because of its great interest, the collection brought together last Wednesday week was quite a model. Prof. Snellen's two modes of testing for colour-blindness ought to have been the first thing looked at, because then the guests would have been in a position to estimate the value of their observations. The inspection caused much amusement, and in some cases astonishment. The "Mechanical Chameleon," to exhibit the mixture of two colours in any proportion, was interesting, as was also Woodward's new rectangular prism illuminator, to be used with immersion lenses. The President's photographs of scenes and objects in the Rocky Mountains were specially attractive. Other objects which attracted considerable attention were—A large Holtz electric machine (by Ladd) consisting of twelve rotary and twelve stationary plates, thirty inches diameter, exhibited by Mr. W. Spottiswoode, Treas.R.S. A microspectroscope with improvements,—(1) quick movement of the slide carrying the slit; (2) scale for registering position of slit; (3) arrangement for comparing three spectra, and for splitting a single spectra; (4) new form of

comparison stage, made by Mr. A. Hilger. A dynamo-electric machine, speed 800 revolutions, power 1·75 H.P. required to work it, effect 1,200 candles' light, exhibited by Messrs. Siemens Bros. The telephone harp, with visible records of sound through vacuum tubes, exhibited by Mr. F. A. Gower. Apparatus for showing figures in light from vibrations caused by sound, exhibited by Mr. Henry Edmunds. A metallic thermometer, invented by Mr. H. Bessemer; and apparatus for the automatic registration of the number of hours of sunlight, made for Kew Observatory, exhibited by Mr. J. Browning. A phoneidoscope, an instrument for observing the coloured figures reflected from liquid filters under the action of sonorous vibrations, made and exhibited by Tisley and Co. Composite portraits, made by combining the likenesses of different persons into a single resultant figure: (1) optically, (2) photographically, exhibited by Mr. Francis Galton, F.R.S.; we hope to publish a paper on the subject next week. We need not say that the phonograph in operation, under the superintendence of Mr. Preece, was specially attractive, and that Winkler's Lunar Landscape, and the beautiful photographs and paintings exhibited, lent a delightful variety to the collection.

THIS fine spring must render folks eager for their summer holidays, and many a plan must be thought over under its influence. Mr. Marshall Hall suggests that if one or two points and dates were fixed as rendezvous, mineralogists, geologists, botanists, entomologists, *et hoc genus omne*, might be very likely to accumulate and compare notes. For one such point Mr. Hall suggests the Hotel Bauer, at Sierre in the Valais, which can be reached in two day from London, *viâ* Paris, Pontarlier, Vallorbe, Lausanne, &c. For examination of the Lötchenthal and the Val d'Anniviers this would be a good place to start from, whilst travellers having mountain business could go about it north to the Oberland and south to the Pennine Alps. "If any other man knoweth a better place let him impart."

ANY ONE who has seen the graceful snout of a salmon or a trout, especially if he has looked upon it after an hour's exciting spin on a river or Highland loch, will be filled with disgust and, if an angler, with grief, on beholding the horrible head of a smolt figured in the *Gardeners' Chronicle* of May 4. It is positively loathsome. And this is the effect of the disease which has been proving so destructive to the helpless creatures in some of the Northern rivers, especially the Esk, Eden, Kent, and even the Tweed we believe. Mr. Worthington Smith has been making some inquiries into the nature of this disease which is killing not only salmon and trout, but eels, flounders, and other fish. He finds it to be a fungus (*Saprolegnia ferax*), which attacks mainly the head, tail, and fins. The scales appear to be covered with a fine white cottony bloom, which at length blinds the fish, envelops the gills, or even entirely closes the gills and mouth. Mr. Smith thinks the reason for the extraordinary abundance of the fungus this year is the unusual mildness of the winter. It seems only to attack the fish in fresh water, those in the estuaries escaping. We trust for the sake of our food supplies as well as on account of our genial friends the anglers, not to mention the poor fish themselves, that some means will be found of preventing the spread of the disease.

PROF. WIEDERSHEIM, of Freiberg University, writes us that through the kindness of Prof. Rutimeyer, he is in a position to describe a Labyrinthodont from the Trias, belonging to the palæontological collection at Basel. While hitherto nothing but the skull and some of the bony scales from the epidermis have been known, this specimen is completely preserved, whereby we obtain for the first time a full and clear insight into the organisation of the entire skeleton of this remarkable amphibian. But not only the skeleton with head, vertebral column, the shoulder and pelvis, down to the last phalanges of the fingers, are on view at the museum at Basel, but also a fine cast of the cranium and the spine, by which the extremely low organisation of the central nervous system of these animals is proved. Prof. Wiedersheim will publish a minute description of the remains in the Reports of the Swiss Palæontological Society.

AT the close of 1877 the amount subscribed for a statue to Linnæus was 44,276 Swedish crowns. This sum being insufficient, further contributions were obtained in Stockholm of 30,000 crowns, and the municipality of that city has undertaken to defray the expense of the pedestal and of the erection of the statue. The total sum available is estimated at 100,000 crowns (5,500*l*).

THE Berlin Ethnographical Museum has lately been enriched by a valuable collection of all the articles used by the two tribes of the Ostiacs and Samojedes in North Siberia. These objects were collected by Dr. Finsch during his voyage in 1876, and will soon possess no slight value, as the peculiarities of these people are rapidly vanishing in contact with Russian civilisation.

PROF. VON SIEBOLD, one of the oldest and best known of German zoologists, celebrated last week in Munich the fiftieth anniversary of his reception of the doctor's degree. The King of Bavaria presented him, on the occasion, with the cross of the Order of St. Michael; deputations were sent by the Munich University and Academy of Sciences, and greetings were sent by numerous foreign universities and societies.

THE Berlin Royal Academy of Sciences has granted the sum of 400 marks (20*l*.) to Dr. Ludwig Graff, Professor of Zoology at the Forest-Academy of Aschaffenburg, for the completion of his "Monograph of *Turbellaria*." Dr. Graff is now at work at the Zoological Station of Naples.

AT the annual meeting of the Royal Institution of Great Britain the Annual Report of the Committee of Visitors for the year 1877, testifying to the continued prosperity and efficient management of the Institution was read and adopted. During the last twenty-five years the number of members paying annually (five guineas) has increased from 344 to 544. The real and funded property now amounts to above 84,500*l*., entirely derived from the contributions and donations of the members. Forty-one new members paid their admission fees in 1877. The principal officers were re-elected.

AT a special general meeting of the Birmingham Natural History and Microscopical Society, held on the 30th ult., Dr. Cobbold, F.R.S., was unanimously elected an honorary vice-president of the Society.

M. F. SOENSON has presented to the Swedish Academy the results of his experiments on the electric conductivity of solutions of various alums. These show that in all cases the conductivity increases directly with the concentration of the solution, and that while less intense than in solutions of the simple alkaline sulphates, it is always more intense than in solutions of aluminium sulphate. The green modification of chrome-alum possesses a greater conductivity than the red variety.

THE French Association for the Progress of Sciences is preparing for its next session, which will take place on August 28 at Paris. The Bureau has been completed and is composed as follows:—President, M. Fremy, Professor of Chemistry at the Polytechnic School and Museum of Natural History; Vice-president, M. Bardoux, the Minister of Public Instruction; Secretary, M. Perrier, of the staff, Director of the Ordnance Survey, Member of the Bureau des Longitudes and Council of the Observatory; Vice-secretary, M. Comte Saporta, Correspondent of the Institute; Treasurer, M. Masson, the scientific publisher; Secretary of the Council, M. Gariel, Engineer of Ponts et Chaussées. The session will take place at the École des Beaux Arts, a very exten-

sive building containing many magnificent rooms for sections. For all inquiries relating to the Paris meeting letters must be directed to M. Gariel, Secretary of Council, 76, rue de Rennes, Paris.

AT the general monthly meeting of the Royal Institution of Great Britain, the Secretary announced that the managers had granted the use of the lecture-theatre to the Sanitary Institute of Great Britain for their anniversary meeting on July 3 at 3 o'clock, when an address would be given by Mr. Frank Buckland, M.A., on "The Pollution of Rivers, and its Effects upon the Fisheries and the Supply of Water to Towns and Villages."

MESSRS. BLACKWOOD have published a fifth edition of Prof. H. A. Nicholson's "Manual of Zoology." While the plan of the work is essentially the same as in former editions, the entire work, the author states, has been submitted to careful revision, and large portions of it have been almost entirely rewritten.

A COMMITTEE has already been formed in Holland, under the presidency of Prince Alexander of the Netherlands, to celebrate, in a fitting manner, the 300th anniversary of the eminent philosopher and statesman, Hugo de Groot, who was born on April 10, 1583.

A MEDALLION representing M. Thenard, the celebrated professor of chemistry, who was during a long time Dean of the Faculty of Sciences and Chancellor of the Paris University, has been sculptured on the walls of the Sorbonne courtyard. It was inaugurated on the occasion of the meeting of the Sociétés Savants. It bears the date of 1877, the centennial year of M. Thenard's nativity. M. Thenard died in 1857.

THE Jardin d'Acclimatation at Paris has just succceeded in obtaining an East Indian tapir, an animal rarely found in European collections, although the South American variety is comparatively common.

AN interesting work has just appeared in Stuttgart from the pen of Dr. R. Andree, on "Ethnographic Parallels and Comparisons." The author has chosen over twenty various subjects, and has gathered together on these topics an enormous amount of material from all the races on the globe. Among these subjects are constellations, cairns, measures of value, mothers-in-law, the vampyre, skull worship, the umbrella as mark of dignity, &c. In view of the rapid invasions of European culture in every direction, the author considers it of the utmost importance to complete as rapidly as possible the collection of all objects necessary to preserve a complete picture of the material and intellectual condition of the uncivilised peoples now existing.

MR. LUGGER, the curator of the Maryland Academy of Sciences, left Baltimore on April 4 for the purpose of prosecuting explorations in the West Indies and in Demerara in behalf of the Academy. In the course of his mission he will endeavour to procure living plants for the conservatory of Druid Hill Park and material for the zoological investigations of the Johns Hopkins University.

DESPITE numerous misfortunes, Berlin still continues to surpass all other European cities in its collection of anthropoid apes. The Zoological Gardens have just received from Borneo a healthy pair of orang-outangs, which, added to the one already in their possession, make an exhibition of rare interest.

ON the evening of April 23 Vesuvius showed signs of internal disturbance, sending up a column of flame at short intervals from the crater.

A FRENCH physician, Dr. Quimus, has lately made an elaborate study of a new disease, prevalent among telegraphic *employées*, and closely resembling writers' cramp. It is more common among the female operators.

FROM the last quarterly list of the members of the Institution of Civil Engineers, we gather that this increasing body now consists of 1,033 members, 1,759 associates, and 16 honorary members, together 2,808; besides a class of students attached numbering 520.

THE German Fischerei-Verein, of the activity of which we have made frequent mention, is engaged now in introducing the Californian salmon extensively into German waters. Of 300,000 eggs sent across the ocean, 25,000 arrived in good condition, and the resultant fish have been divided between the rivers of the Danube valley and those of the Rhine. 300,000 young eels from Normandy are being introduced into the Prussian streams.

AMONGST the few halls of the Paris Exhibition which can be considered as quite ready we must notice the excellent school exhibition of the City of Paris, which is situated in the central part of the palace.

A NEW remedy for diarrhœa in men and animals is said to have been discovered in New Zealand, where it has long been in use among the Maories. It consists in a decoction made by pouring boiling water on the green leaves of a shrub called roromiko by the natives. The liquid, though slightly bitter, is said to be not unpleasant to the taste. It is asserted that two doses of this decoction will always effect a cure even in bad cases.

A JAPANESE (native) paper states that a resident at Osaka has been endeavouring to manufacture oil from crude camphor, for which purpose he has built a large factory in that town. The oil he makes is described as being cheaper and better for purposes of illumination than kerosene.

ONE of the curiosities of industry, according to the *Japan Herald*, is the manufacture of boots by the Japanese for sale in the United States, a trade which is of quite recent origin, but has already attained considerable proportions. Oddly enough most of the leather used is imported into Japan from the United States.

A PRODUCT of the South Sea Islands, "copra" which is the dried kernel of the cocoa-nut, is being turned to a new account. Hitherto it has only been used for making oil, but now it has been discovered that the residue, after that process, is valuable as food for cattle and sheep.

THERE have been not a few signs recently that Spain is awakening from her long lethargy with regard to progress of all kinds, and one more comes to us in Nos. 6 to 25 (with the exception of No. 13, which has not come to hand), of the *Boletín de la Institucion Libre de Enseñanza* (Madrid), which make us acquainted with the Proceedings down to February 28. These recent numbers give us information as to the rules and objects of the Institution. By Article 1 the Institution is "Consagrada al cultivo y propagacion de la ciencia en sus diversos órdenes." By Art. 3 the number of Fellows is unlimited. By Art. 15 "La *Institucion* es completamente ajena á todo espiritu é interés de comunion religiosa, escuela filosófica ó partido político; proclamando tan solo el principio de la libertad é inviolabilidad de la ciencia y de la consiguiente independencia de su indagacion y exposicion respecto de cualquiera otra autoridad que la de la propria conciencia del Profesor, único responsable de sus doctrinas. Art. 16: La Institucion establecerá, segun lo permitan las circumstancias y los medios de que pueda disponer: 1. Estudios de cultura general (ó de segunda Enseñanza) y profesionales, con los efectos académicos que les concedan las leyes del Estado; 2.

Estudios superiores científicos; 3. Conferencias y cursos breves de carácter, ya científico, ya popular; 4. Una biblioteca y los Gabinetes dotados del material correspondiente; 5. Un boletin para publicar sus documentos oficiales y trabajos científicos; 6. Concursos y prémios, y cuanto contribuya á promover la cultura general y sus propios fines." These extracts from the statutes, ratified May 31, 1876, will sufficiently show the aims of the Institution, and show also what is being done for the cultivation of science in Madrid. Running through Nos. 10–15, 18–21, is a list of 728 shells, in the natural history cabinet, arranged on the method of Dr. Woodward's "Manual of Conchology," and in Nos. 22, 23 are catalogues of plants in herbaria from the Province of Avila and from the Philippine Islands. In Nos. 24, 25, a classification of rock specimens. The contents of the several numbers are of the same general character as we indicated in our former notice. The papers on Haeckel's morphology are continued, and the same professor (A. G. de Linares) has papers on the classification of geometrical figures, and on some recent publications on crystallography and mineralogy. The syllabuses are given of courses of lectures on two or three languages, on mathematics (arithmetic and synthetic geometry) and other subjects. We can only wish success to this the first (we believe) society of the kind that has been formed in Spain.

No plant perhaps has a more varied adaptation than the bamboo. In every country where these gigantic grasses grow they are put to a multitude of uses. It is not then because the bamboo is incapable of being converted to any other use that so much attention has been given to it of late with the view of turning it into a source of supply for paper material. It is more on account of its rapid growth, the ease with which it can be propagated and its abundant yield, together with its wide geographical range, that such interest has been roused in it, for the several species of bamboo are found in most tropical parts of the world. If, however, it should become a regularly recognised paper material there is no doubt that our supplies would be obtained chiefly from the East and West Indies. With regard to its growth in the latter country there seems to be a prospect that it may prove successful for cultivation in plantations specially formed for growing the plants for paper stock. There are, of course, extensive natural resources of bamboo, but it is thought that by cultivation and a system of irrigation the yield would be greatly increased and the cost of keeping up such a plantation would, after the first two years, be almost *nil*. It is by no means improbable that the bamboo will in the course of time become an important paper-making commodity.

A STRANGE meteorological phenomenon was recently observed at Logelbach, in Upper Alsatia. The rising sun seemed to be surrounded by a vast column of fire. An eye-witness describes the occurrence in *La Nature*. When he began his observations, the column had already reached a height of 25 or 28 degrees. Its breadth remained constant, and amounted to 2 or 2½ degrees. Its colour was greyish red, and at its upper end orange; the dull and cloudy sky formed a fine contrast with the brilliant phenomenon. From 6.30 A.M. till 7 o'clock its brilliancy remained much the same, while its extent towards the west increased by about 4 or 5 degrees. At 7 o'clock the sun's disc appeared above the horizon, and its tint was an intense red. The whole sky now seemed to be a gigantic rainbow, all the shades of which appeared in horizontal layers, forming a splendid background to the bright red and orange vertical column. A minute later the sun lost its red tint and the column gradually decreased; for five minutes it formed a band of 5 degrees in height, and then disappeared altogether.

DECADE V. of the "Prodromus of the Palæontology of Victoria," by Mr. Frederick McCoy, of the Geological Survey of Victoria, deals, by means of well-executed lithographic illustrations and text, with numerous fossils of the tertiary and Upper and Lower Silurian formations.

THE recent numbers (26-31) of Bentley and Trimen's "Medicinal Plants" fully maintain the excellence of the earlier ones. Among the admirable plates of well-known plants in these numbers may be mentioned those of *Aconitum ferox*; the opium-poppy, *Papaver somniferum*; the liquorice, *Glycyrrhiza glabra*; the indigo, *Indigofera tinctoria*; the camphor, *Cinnamonum camphora*; and the sabine, *Juniperus sabina*. The only one in these numbers which does not strike us as so happy, is that of the common marjoram, *Origanum vulgare*.

WE regret that the name of M. Milne-Edwards somehow got among the catalogue of the eminent men whom we named last week as having gone over to the majority during the existence of NATURE. We are glad to say that M. Milne-Edwards, though as old as the century, is as active as ever.

THE addition to the Zoological Society's Gardens during the past week include a Lion (*Felis leo*) from Africa, presented by Mr. J. D. Massey; a Vervet Monkey (*Cercopithecus lalandii*) from South Africa, presented by Mr. G. W. Twining; a Macacque Monkey (*Macacus cynomologus*) from India, presented by Mr. J. M. Neil; a Black-eared Marmoset (*Hapale penicillata*) from South-East Brazil, presented by Mr. Walter M. St. Aubyn; a Common Cormorant (*Phalacrocorax carbo*), European, presented by Lord Braybrooke; four Green Lizards (*Lacerta viridis*) from the Isle of Jersey, presented by Mr. F. E. Lawder; a Black Ape (*Cynopithecus niger*) from the Celebes, a Brazilian Tree Porcupine (*Sphingurus prehensilis*) from South America, deposited; two Lesser Birds of Paradise (*Paradisea papuana*) from New Guinea, two Black Storks (*Ciconia niger*) European, purchased; two Black-faced Spider Monkeys (*Ateles ater*) from East Peru; a Common Cassowary (*Casuarius galeatus*) from Ceram, a Golden-winged Woodpecker (*Colaptes auratus*) from North America, received in exchange; a Great Kangaroo (*Macropus giganteus*), an Eland (*Oreas canna*) born in the Gardens.

ACADEMIC LIBERTY IN GERMAN UNIVERSITIES[1]

IN taking possession of the high functions to which the vote of my colleagues has raised me, my first duty is to renew here, publicly, the expression of my thanks towards those who have given me this proof of their confidence. Its value is all the greater in my eyes because it has been given to me notwithstanding the few years I have passed among you and notwithstanding my function of professor in the natural sciences which form, in the curriculum of university education, a foreign element, the introduction of which has caused the modification of several points in the ancient organisation of the faculties, and will yet induce others in the future. The department of physics to which I have devoted myself is exactly that which contains the theoretical foundations of all the other branches of the natural sciences, and which presents in the most striking form the characteristic features of their methods. Thus I have several times already been compelled to propose to the University modifications in the rules previously followed, and I have had the pleasure of being always backed by the hearty support of my colleagues and the University Senate. Since you have chosen me to direct the University during the course of the next year, it is a proof, in my eyes, that you do not regard me as a rash innovator.

The object, the method, the immediate aim of the natural sciences may at first sight appear altogether distinct from those of the moral sciences; it seems to men accustomed to occupy themselves exclusively with the immediate expression and the proofs of the intellectual life, that they have nothing to learn from the results of these sciences, and that they have for them only a remote interest. But, in reality, as I have already

[1] Rectorial Address of Prof. Helmholtz, F.R.S., at the University of Berlin.

endeavoured to show in my rectorial address at Heidelberg, there is a very close relationship between the two orders of sciences; they pursue the same final end by processes which, at bottom, are the same. If the greater part of the researches in the natural sciences have not for their immediate object an intellectual advantage, on the other hand, it should not be forgotten, that the power of the pure intellectual method is here shown much more clearly, and a penetrating analysis of phenomena makes known the true and the false with much more precision than can be the case in the complex problems of the moral sciences.

Side by side with the development of this new branch of scientific activity, almost unknown in antiquity, the changes which have supervened in political, social, and even international relations, also exercise an influence which must be taken account of. The circle of our students is enlarged; the transformation of public life entails new exigencies; the various branches of science are more and more subdivided; it becomes necessary to add to libraries other means of study more and more considerable and more and more varied. It is difficult to foresee what new wants and what new exigencies we shall have to face in the near future.

On the other hand, it is not only in our own country that the German universities have a place of honour: they attract the attention of the civilised world. Students speaking the most diverse languages flock to them from the ends of the earth. A false step may make us fall from this high position, and it would afterwards be difficult to regain it.

In these circumstances it is our duty to seek to discern clearly what has hitherto been the internal principle of the prosperity of our universities, what essential element of their organisation must be maintained intact as a thing sacred and inviolable, and in what direction our efforts should tend when reforms become necessary. I do not consider myself authorised to pronounce on these questions in a definitive manner. The point of view of each of us is necessarily a little exclusive; the representatives of other sciences may, from other points of view, advance different considerations. But I think that, in order to arrive at definite and fixed conclusions, it is necessary that each one seek to express exactly what are his particular ideas on these questions.

Over all Europe, in the Middle Ages, the universities had their origin in unions, free and private, of students grouped under the influence of celebrated masters. These unions regulated their own affairs. In recognition of the public services they rendered, the Governments soon accorded them guarantees, privileges, and honours, notably the right of examining their members and of conferring academic degrees. The students of that epoch were, for the most part, mature men, who resorted to the universities for the purpose of being instructed and without any immediate practical end. Soon they commenced to send young men also, placed very often under the care of older students. Each university was divided into more restricted associations, known under the names of Nations, Bourses, Colleges. The older graduate members of these associations, the *Seniores*, administered the special affairs in each of them, and met in general assembly to discuss the affairs common to all the university. We may see even to-day in the court of the University of Bologna the list and the arms of the members and *Seniores* of the various Nations which formerly composed it. The oldest graduates were regarded during their whole life as members of the association; they preserved their right of voting, a custom which has been continued almost to our own days, or which exists still in the college of the doctors of the University of Vienna and in the colleges of Oxford and Cambridge.

Thus, a free union of independent men, all brought together, masters and pupils, by the pure love of knowledge, the one anxious to know the treasures of intellectual culture left by antiquity, the other labouring to communicate to the new generation the enthusiasm for the ideal which had kindled their souls; such was the origin of the universities, whose organisation, in its principles and its details, was founded on the most complete liberty. We must not, however, believe that they admitted the liberty of education in the modern sense of the term. The majority showed itself very intolerant to differences of opinion. More than once those who found themselves in the minority were compelled to quit the university. This occurred not only when the Church intervened or when political or metaphysical questions were agitated. The faculties of medicine themselves, and at their head that of Paris, the most celebrated of all, would not tolerate any deviation from what they regarded as the doctrine of Hippocrates. They expelled from their midst those who practised the medicine of the Arabs or who admitted the circulation of the blood.

The transformation which led the universities to their present situation was due principally to the action of the State, which provided them with material assistance, and, in exchange, assumed the right of interfering in their affairs. The progress of this development was not the same in the various countries of Europe; it was determined in part by the political situation, in part by the peculiar character of each nation.

Those which underwent the fewest changes were the two old English universities of Oxford and Cambridge. Their large revenues and the political tendency of the English to respect all acquired rights have preserved them almost absolutely from alteration, even on points where changes would have been extremely desirable. These two universities preserve even to-day the character of schools intended to recruit the clergy, formerly the Roman Catholic clergy, now that of the Anglican church. The laity participate in the education which is there given, in so far as that may contribute to general intellectual culture; but they must submit to the discipline and the mode of life which were formerly considered suitable for young clerics. They live together in kinds of colleges, under the surveillance of a certain number of elder graduates (Tutors) belonging to the same college; for the rest they follow the manners and customs of the wealthy classes of England. They can only go about in a certain costume, of a somewhat ecclesiastical cut, with special *insignia*, indicating not only their academic grades, but also their social rank. The education, in its basis and method, is that of our gymnasia, but a little more developed; in certain points only it approaches more the *repetitions* organised in our universities; thus, it is limited to the programme required for examination, and the students are bound to study certain books, indicated beforehand. The work of the students is controlled by very detailed examinations, which must be passed in order to obtain the academic degrees, and in which very special knowledge is required, but only in certain very narrow subjects. All the old degrees of the academic dignities, the baccalaureate, the licentiate, the mastership in Arts, the doctorate, are obtained by tests of the same kind. The lessons are generally given by the Tutors above referred to. But they do not teach by virtue of an official delegation like the masters in our gymnasia; there are rather special masters chosen by certain groups of students. There are few professors, and they give only a small number of lectures to a scanty auditory, and usually on a very special subject. These lectures do not constitute an essential part of the education; they serve at the most to furnish to some students, having a special interest to make great efforts, the occasion for more profound study. The various colleges are, moreover, completely separated from each other; the examinations, the conferment of degrees, the nomination of professors are the only matters common to the whole university.

It is only quite recently that students not belonging to the Church of England have been admitted, and that some little attempt has been made to provide for professional education in law and medicine. Among the professors of the English universities, there is a great number of very distinguished men, and who have a place in science. But the right of taking part in their election is not reserved to the Fellows actually forming a part of the corporation; it belongs equally to all the former Fellows, even when they have no longer any connection with the university, when they have no interests in common with it, and when they may be engaged in the struggles of political and ecclesiastical parties. The result is that party considerations, personal connections, and friendship, often exercise more influence on the elections than scientific merit. From this point of view the English universities have preserved all the intolerance of the middle ages. The professors are not required to reside in the university town; they may fix their abode in any part of the kingdom; they may even fill other functions at their convenience, often, for example, that of parish priest; it is enough that they give their lesson at the university once a week, sometimes even more seldom.

The English universities devote a very small portion of their enormous revenues to the endowment of chairs and to filling them with masters having an indisputable authority in science, and this little is badly employed. But they possess another institution which appears called upon to render the greatest service to scientific studies, although hitherto it has done very little in this respect; this is the institution of Fellowships. The stu-

dents who have passed highest in the examinations are authorised to remain in the quality of Fellows in their college, where they are lodged and boarded; they receive, besides, a pension of 200*l*., which assures to them the liberty of devoting all their time to science. Oxford has 557 places of this kind, Cambridge 531. The Fellows may act as tutors to the students, but they are free not to use this privilege. They are not, moreover, obliged to live in the university town; they may spend their pension where they please, and preserve it during an indefinite period. Save in exceptional cases, they only lose it when they marry, or when they accept some employment. They are the legal successors of the old student corporations, by and for whom the universities were founded and endowed. But beautiful as the plan of the institution may be, fabulous as may be the sums devoted to it, the services which it renders to science are of the most mediocre in the judgment of all unprejudiced Englishmen. This is probably owing to the fact that these young persons, although they are the *élite* of the students, and find themselves in conditions exceptionally favourable to work, have not been, during the course of their studies, sufficiently profoundly penetrated by the vivifying spirit of science, to experience that enthusiasm and that passion which impels men to make personal efforts.

The English universities render, from certain points of view, very important services. They make their students cultured men, although little disposed to pass the political or religious limits of their party, and, in fact, they do not go beyond these; the Tories dominate at Oxford, the Whigs at Cambridge. We ought, above all, to seek to rival them in two things. In the first place, they develop in a very high degree among their students, at the same time a lively sense of the beauties and the youthful freshness of antiquity, a taste for precision and elegance of language; this is seen in the fashion in which the students manage their mother tongue. There is here, I fear, one of the weakest sides in the education of youth in Germany. In the second place, the English universities pay much more attention than ours to the physical well-being of their students. These live and work in spacious, well-aired buildings, surrounded with lawns and with masses of trees; their pleasures consist specially in games which, exciting a passionate emulation, favour the development of the vigour and dexterity of the body much more efficaciously than our military and gymnastic exercises. It must not be forgotten that if we deprive young people of the open air and of the opportunity of developing their vigour, they are all the more led to seek unhealthy distractions in the abuse of tobacco and strong drinks. We must admit, besides, that the English universities accustom their students to serious and energetic work, and make them preserve the habits of well-bred people. As to the *moral* effect of a rigid surveillance, it must be tolerably illusory.

The Scotch universities, and some small English universities of recent formation, as University College and King's College, London, and Owens College, Manchester, approach more to the German and Dutch type.

The French universities have followed a different, almost absolutely opposite course. In consequence of the tendency of the French to upset, in virtue of logical theories, all which is the product of a historical development, their faculties have become simple establishments of instruction, special schools preparing for a career, and in which the programme of education is subjected to fixed rules. They are completely distinct from the institutions devoted to the progress of science, such as the Collége de France, the Jardin des Plantes, l'Ecole des Hautes Etudes. The faculties are absolutely separate from each other, even when they are placed in the same town. The course of study is determined with precision; numerous examinations serve to control the results. French education is limited to what is clearly and solidly established; it gives an exposition of this, well ordered, carefully elaborated, easily intelligible, without entering upon doubtful questions and without going to the bottom of things. The masters charged with distributing it only need to have acquired much. Thus, in France, it is almost a mistake on the part of a young man possessing a talent full of promise, to consent to become professor in a provincial faculty. The French system is well suited to give to students of moderate capacity knowledge sufficient to follow the routine of their profession. They have not to choose between different professors, and, consequently, they swear *in verba magistri;* there results a propensity to doubt nothing and to be self-satisfied. If the professor is good, that suffices for ordinary cases, where the student has only to imitate what he has seen his master do. It is only in extraordinary cases that it may be seen if he has really acquired penetration and judgment. For the rest, the French nation is well endowed, lively and ambitious; this makes up for many of the faults of the system of education.

In the French universities—and it is a characteristic feature of their organisation—the situation of a professor is absolutely independent of the assent of his pupils. The students belonging to the faculty in which he is professor are bound to follow his lessons; the very high fees which are paid go to the treasury of the Minister of Public Instruction, and serve to cover the fixed salary of the body of professors; the State contributes to the expenses of the universities only to a very small extent. If, then, the professor has not really the passion for education, and if he has not the ambition of attracting a large auditory, he may remain indifferent to the success of his instruction and take it easy. Outside the lecture-rooms, where they take their courses, French students live without being subjected to any surveillance, without *esprit de corps*, and without particular habits, confounded with young people of the same age who follow other careers.

The development of the German universities has followed a course intermediate between these two opposite paths. They were too poor in private resources not to accept eagerly the help of the State in presence of the more and more costly demands of education. Consequently at the epoch when modern states tended to consolidation they were not in a position to defend their ancient privileges, and they had to submit to the directing influence of the State. Consequently for all the important affairs of the universities, the supreme decision was, in principle, reserved by the State, and in times of political and religious disturbance an inconsiderate use was often made of this supremacy. In most cases, however, the universities were favourably treated by the governments newly arrived at independence. They required intelligent functionaries, and the glory of their university threw upon them a certain *éclat*. The administrative functionary came, for the most part, from the universities and remained attached to them. Thus, in the midst of the tumult of war and of political convulsions, in all these states struggling with the tottering empire and occupied in consolidating their recent independence, while nearly all other special privileges disappeared, the German universities succeeded in retaining a much more considerable part of internal liberty (and indeed the most precious elements of this liberty) than was the case in conservative England and in that France which is feverishly chasing after liberty.

Among us the old conception of the student remains the same; he is always considered as a responsible young man who pursues science of his own accord, and who is free to regulate as he pleases the plan of his studies. If, for a small number of careers, it is still necessary to follow certain courses, this obligation is not imposed by the university as a university, but by the authority which will at a later period admit the candidate to follow these careers. Moreover, students have to-day, and had formerly, with few exceptions, full liberty to choose among all the universities of the German tongue, from Dorpat to Zurich, Vienna, and Graz. They may choose, besides, in each faculty, among the masters who teach the same subjects, without taking account of the distinction between ordinary professors, extraordinary professors, and privat-docenten. It is even allowable for them to obtain their instruction from books to any extent they may desire; it is, in fact, very desirable that the works of the great men of the past should constitute an essential part of study.

Outside the universities no surveillance is exercised over the conduct of the students, provided they do not come into collision with the agents of public security. Except in this case, the only control to which they are subject is that of their comrades, which prevents them from doing anything against the honour of the body. The universities of the Middle Ages were close corporations, exercising over their members a jurisdiction which was extended to the right of life and death. As the students found themselves for the most part on foreign soil, this special jurisdiction was necessary, not only to withdraw them from the judgment of the authorities of the country, but also to be able to allay the conflicts which arose among themselves, and to maintain in the corporation sufficient good order and good breeding to insure the maintenance of the hospitality offered. Under the influence of the modern political organisation, this academic jurisdiction has gradually given way before the ordinary jurisdiction; the last vestiges will soon disappear, but the necessity

always subsists in such numerous meetings of lively and eager youths, of submitting to certain restrictions calculated to preserve the tranquillity of their comrades and that of the citizens. It is to this necessity that, in cases of conflict, the disciplinary jurisdiction of the University authorities responds. However, this end is still more surely attained by the sentiment of the honour of the body, and it is gratifying to have to acknowledge that this consciousness of their moral solidarity, and of the obligations of honour in the case of every one resulting therefrom, remains alive among German students. I do not mean by this to approve of all the special prescriptions of the code of honour of students. There are among the number certain remains of the middle ages of which it would be good to get rid, but this is a thing which can only be done by the students themselves.

(*To be continued.*)

STRIDULATING CRUSTACEANS

At the November meeting of the Entomological Society of London, the president, Prof. Westwood, directed the attention of the Society to a letter in NATURE (vol. xvii. p. 11) from Mr. Saville Kent, on the above subject, *à propos* of Mr. Wood-Mason's recent discovery of the existence of stridulating apparatus in scorpions.

Mr. Wood-Mason remarked that structures in Crustacea, some of which certainly, and all of which probably, are for the production of sounds, were first brought to notice by Hilgendorf—in V. der Decken's "Reisen in Ost-Africa (Crustacea)"—but had been independently observed by himself in a number of species during his dredging excursion to the Andaman Islands in 1872. They were paired organs, as in scorpions, the *Mygale*, and the *Phasma* to be brought to notice that night—that is to say, organs working perfectly independently of each other were on each side of the body. In some forms (I.) they were seated partly on the body (carapace) and partly on a pair of appendages; of these some (*a*) had the *scraper* on the body and the *rasp* on the appendages—*e.g. Matuta*, in which the organs are developed in both sexes; and others (*b*) had the *rasp* on the body and the *scraper* on the appendages—*e.g. Macrophthalmus et affinia*, in which the scraper was formed by a sharp-edged lamellar projection on the meropodite of each of the chelipeds, and the rasp was the crenulated infraorbital margin; in these the apparatus could only be developed in the males, the females having short and small and quite inconspicuous chelipeds, which hardly reached so far as to the margins of the orbits. In others (II.) they were seated wholly on the appendages; in the males of the species of *Ocypode* the *rasp* was on one and the *scraper* on another part of the same appendage; in those of *Platyonychus bipustulosus* the *rasps* were on one and the *scrapers* on another pair of appendages; the walking-legs of the second pair were here very long and robust, and their third joint (meropodite) had its upper margin produced upwards at apex into a sharp crest (the *scraper*); both Dana and Milne-Edwards had noticed the remarkable length and structure of this pair of legs, but the former alone had mentioned, in his description of the species, the regular transverse plication of the under surface of the propodite of the chelipeds, which constituted without doubt the *rasp*. The above did not pretend to be a complete account of stridulating apparatus in Crustacea; but separated as he at present was from notes, drawings, and specimens, he could not go into greater detail. The cases of *Macrophthalmus* and of *Platyonychus* had not, he believed, been previously recorded. In the forms alluded to by Mr. Kent, no special sound-producing apparatus seemed to be developed. Everybody who had searched for animals on coral-reefs or had dredged in tropical seas was familiar with the "clicking" sounds emitted by the *Alphei* and their allies. The sounds which here always accompanied so sudden an opening of their claws to their fullest extent that dislocation seemed imminent each time, might be caused either by the impact of the dactylopodite upon the joint to which it is articulated, or by the forcible withdrawal of the huge stopper-like tooth of the dactylopodite from its pit in the immovable arm of the claw; in which latter case the noises might be susceptible, *mutatis mutandis*, of the same physical explanation as that produced by the withdrawal of a tightly-packed piston from a cylinder closed at one end. These were the explanations that occurred to him while watching a small species that lived in force amidst the branches of the zoophytes called *Spongodes*, the masses of which crackled all over when brought to the surface. The sounds in this case resembled very closely those made when sparks were taken by the knuckles from the prime-conductor of a small electrical machine. The sounds emitted by the Sphæromid might possibly be produced by the impact of the terga of the posterior somites upon one another at the end of each movement of extension.

Mr. Wood-Mason then announced the discovery of stridulating organs in *Phasmidæ*, in a species of *Pterinoxylus*, and in illustration of his remarks exhibited an impression of Westwood's plate of Serville's species, *P. difformipes*. Here, as in Crustacea and some other Arthropods, an apparatus working perfectly independently of its fellow was developed on each side of the body. The rough prominent basal portion of the costal nervure of the wings formed the rasp, in connection with which was developed a large oval "speculum," "talc-like spot," or "mirror." The rasps were scraped by the sharp and hard front edges of the tegmina, the dome-like form of which seemed admirably adapted, and probably did, to some extent, serve to increase the sound by resonance. In Serville's species, according to Westwood's figure, the stridulating apparatus appeared to be more highly developed, the "mirror" being more distinct, and the tegminal cavities more spacious. The males of the *Pterinoxyli* were unknown. We had here another case in which functional stridulating organs are present in females. The only other insects known to him in which stridulating organs were seated partly on the wings and partly on the tegmina were the orthopterous *Œdipoda*, which, according to Scudder (*Amer. Nat.* ii. 113), stridulate during flight, in connection with which fact it was interesting to observe that the female *Pterinoxyli*, though incapable of flight, needed to expand their organs of flight in order to bring their similarly situated apparatus into play.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—At Queen's College, James Henry Hickens, Epsom College, has been elected to a Natural Science Scholarship.

CAMBRIDGE.—The Rede Lecture will be delivered by Prof. Clerk-Maxwell, in the Senate House, on Friday, May 24, at half-past 2 o'clock, on the Telephone.

OWENS COLLEGE.—Should this institution ever be transformed into the University of Manchester, it will only be after overcoming a good deal of strong opposition. The Liverpool Town Council are to petition in favour of a new corporation with power to incorporate Owens College and other institutions, and that the new University do not bear any merely local or personal appellation. Naturally, also, the Yorkshire College does not look kindly on the proposal, although until Owens College resolved to take this step the two institutions were on very friendly terms. We trust some arrangement will be come to ultimately that will satisfy all concerned.

WORKING MEN'S COLLEGE.—The Science Classes at the Working Men's College, which, during the last three years have, under Mr. Dunman's teaching, have become so popular and useful a feature of that institution, assembled on Saturday last at the Broad Street Restaurant to celebrate the termination of a very successful course by a dinner. Mr. Thomas Hughes had promised to be present, but in his compulsory absence Mr. Dunman himself occupied the chair. A pleasing feature of the evening was the presentation to Mr. Dunman, by the students in these classes, of a handsome despatch box as a token of their appreciation of the thoroughly efficient manner in which he has discharged the duties of science teacher.

STRASSBURG.—The Extraordinary Professorship of Petrography, lately occupied by Prof. Rosenbusch, is to be filled by Dr. Cohen, of Heidelberg.

SCIENTIFIC SERIALS

THE *Journal of the Russian Chemical and Physical Societies* of St. Petersburg (vol. x. No. 3) contains the following papers:—On the mono- and dioxymalonic acids (Part 2), by R. Petrieff.—Researches on the transformation of diethylcarbinol into methylpropylcarbinol, and on the synthesis and the properties of diethylacetic and methylpropylacetic acids, by A. Saytzeff.—On the synthesis of diphenylenephenylmethane and of diphenylenetolylmethane, by V. Hemilian.—On the falsification of butter, by P. Koulechoff.—On the elementary law governing the reciprocal actions between currents and magnets, by A. Socoloff.

Verhandlungen der k.k. Zoologische botanischen Gesellschaft in Wien. (1867, vol. ii.) This volume, like its predecessors, contains valuable additions to zoological and botanical literature. By far the most important papers contained in it are Dr. L. Koch's notes on Japanese *Arachnida* and *Myriapoda*, and Herr H. B. Möschler's remarks on the *Lepidoptera* fauna of Surinam, continued from a former volume. Of other interesting papers we note :—Lichenological excursions in the Tyrol, by F. Arnold. —On the spiders of Uruguay and other parts of America, by E. Keyserling.—Introduction to the monography of *Phaneropterida*, by Brunner von Wattenwyl.—Hymenopterological notes, by F. F. Kohl.—On the flora of the Ionian Islands of Corfu, Cephalonia and Ithaca, by G. C. Spreitzenhofer.—On a species of *Aphis*, *Pemphigus Zeae Maïdis*, L. Duf, which attacks Indian corn, by Dr. Franz Löw.—Notes on the *Aeolidiadae*, by Dr. Rudolph Bergh.—On the Brazilian ants collected by Prof. Trail, by Dr. Gustav Mayr.—There are also in this volume some smaller communications from the botanical laboratory of Dr. H. W. Reichardt.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, April 11.—"On Stresses in Rarefied Gases arising from Inequalities of Temperature," by J. Clerk-Maxwell, F.R.S., Professor of Experimental Physics in the University of Cambridge.

1. In this paper I have followed the method given in my paper "On the Dynamical Theory of Gases" (*Phil. Trans.* 1867, p. 49). I have shown that when inequalities of temperature exist in a gas, the pressure at a given point is not the same in all directions, and that the difference between the maximum and the minimum pressure at a point may be of considerable magnitude when the density of the gas is small enough, and when the inequalities of temperature are produced by small solid bodies at a higher or lower temperature than the vessel containing the gas.

2. The nature of this stress may be thus defined; let the distance from the given point, measured in a given direction, be denoted by h, and the absolute temperature by θ; then the space-variation of the temperature for a point moving along this line will be denoted by $\frac{d\theta}{dh}$, and the space-variation of this quantity along the same line by $\frac{d^2\theta}{dh^2}$. There is in general a particular direction of the line h, for which $\frac{d^2\theta}{dh^2}$ is a maximum, another for which it is a minimum, and a third for which it is a maximum-minimum. These three directions are at right angles to each other, and are the axes of principal stress at the given point; and the part of the stress arising from inequalities of temperature is in each of these principal axes a pressure equal to—

$$3\frac{\mu^2}{\rho\theta}\ \frac{d^2\theta}{dh^2},$$

where μ is the coefficient of viscosity, ρ the density, and θ the absolute temperature.

3. Now, for dry air at 15° C., $\mu = 1{\cdot}9 \times 10^{-4}$ in centimetre-gramme-second measure, and $\frac{3\mu^2}{\rho\theta} = \frac{1}{p}\,0{\cdot}315$, where p is the pressure, the unit of pressure being one dyne per square centimetre, or nearly one-millionth part of an atmosphere.

If a sphere of one centimetre in diameter is T degrees centigrade hotter than the air at a distance from it, then, when the flow of heat has become steady, the temperature at a distance of r centimetres will be

$$\theta = T_0 + \frac{T}{2r}, \text{ and } \frac{d^2\theta}{dr^2} = \frac{T}{r^3}.$$

Hence, at a distance of one centimetre from the centre of the sphere, the pressure in the direction of the radius arising from inequality of temperature will be—

$$\frac{T}{p}\,0{\cdot}315 \text{ dynes per square centimetre.}$$

4. In Mr. Crookes's experiments the pressure, p, was often so small that this stress would be capable, if it existed alone, of producing rapid motion in small masses.

Indeed, if we were to consider only the normal part of the stress exerted on solid bodies immersed in the gas, most of the phenomena observed by Mr. Crookes could be readily explained.

5. Let us take the case of two small bodies symmetrical with respect to the axis joining their centres of figure. If both bodies are warmer than the air at a distance from them, then in any section perpendicular to the axis joining their centres, the point where it cuts this line will have the highest temperature, and there will be a flow of heat outwards from this axis in all directions.

Hence $\frac{d^2\theta}{dh^2}$ will be positive for the axis, and it will be a line of maximum pressure, so that the bodies will repel each other.

If both bodies are colder than the air at a distance, everything will be reversed; the axis will be a line of minimum pressure, and the bodies will attract each other.

If one body is hotter, and the other colder, than the air at a distance, the effect will be smaller; and it will depend on the relative sizes of the bodies, and on their exact temperatures, whether the action is attractive or repulsive.

6. If the bodies are two parallel discs, very near to each other, the central parts will produce very little effect, because between the discs the temperature varies uniformly and $\frac{d^2\theta}{dh^2} = 0$. Only near the edges will there be any stress arising from inequality of temperature in the gas.

7. If the bodies are encircled by a ring having its axis in the line joining the bodies, then the repulsion between the two bodies, when they are warmer than the air in general, may be converted into attraction by heating the ring, so as to produce a flow of heat inwards towards the axis.

8. If a body in the form of a cup or bowl is warmer than the air, the distribution of temperature in the surrounding gas is similar to the distribution of electric potential near a body of the same form, which has been investigated by Sir W. Thomson.[1] Near the convex surface the value of $\frac{d^2\theta}{dh^2}$ is nearly the same as if the body had been a complete sphere, namely, $2T\frac{1}{a^2}$, where T is the excess of temperature, and a is the radius of the sphere. Near the concave surface the variation of temperature is exceedingly small. Hence the normal pressure on the convex surface will be greater than on the concave surface, as Mr. Crookes has shown by the motion of his radiometers.

Since the expressions for the stress are linear as regards the temperature, everything will be reversed when the cup is colder than the surrounding air.

9. In a spherical vessel, if the two polar regions are made hotter than the equatorial zone, the pressure in the direction of the axis will be greater than that parallel to the equatorial plane, and the reverse will be the case if the polar regions are made colder than the equatorial zone.

10. All such explanations of the observed phenomena must be subjected to careful criticism. They have been obtained by considering the normal stresses alone, to the exclusion of the tangential stresses; and it is much easier to give an elementary exposition of the former than of the latter.

If, however, we go on to calculate the forces acting on any portion of the gas in virtue of the stresses on its surface, we find that when the flow of heat is steady, these forces are in equilibrium. Mr. Crookes tells us that there is no molar current, or wind, in his radiometer vessels. It may not be easy to prove this by experiment, but it is satisfactory to find that the system of stresses here described as arising from inequalities of temperature will not, when the flow of heat is steady, generate currents.

11. Consider, then, the case in which there are no currents of gas, but a steady flow of heat, the condition of which is

$$\frac{d^2\theta}{dx^2} + \frac{d^2\theta}{dy^2} + \frac{d^2\theta}{dz^2}\ (= -\Delta^2\theta) = 0.$$

(In the absence of external forces, such as gravity, and if the gas in contact with solid bodies does not slide over them, this is always a solution of the equations, and it is the only permanent solution.) In this case the equations of motion show that every particle of the gas is in equilibrium under the stresses acting on it.

Hence any finite portion of the gas is also in equilibrium; also, since the stresses are linear functions of the temperature, if we superpose one system of temperatures on another, we also superpose the corresponding systems of forces. Now the sys-

[1] Reprint of Papers on Electrostatics, p. 178.

tem of temperatures due to a solid sphere of uniform temperature, immersed in the gas, cannot of itself give rise to any force tending to move the sphere in one direction rather than in another. Let the sphere be placed within the finite portion of gas which, as we have said, is already in equilibrium. The equilibrium will not be disturbed. We may introduce any number of spheres at different temperatures into the portion of gas, and when the flow of heat has become steady, the whole system will be in equilibrium.

12. How, then, are we to account for the observed fact that forces act between solid bodies immersed in rarefied gases, and this, apparently as long as inequalities of temperature are maintained?

I think we must look for an explanation in the fact discovered in the case of liquids by Helmholtz and Piotrowski,[1] and for gases by Kundt and Warburg,[2] that the fluid in contact with the surface of a solid must slide over it with a finite velocity in order to produce a finite tangential stress.

The theoretical treatment of the boundary conditions between a gas and a solid is difficult, and it becomes more difficult if we consider that the gas close to the surface is probably in an unknown state of condensation. We shall, therefore, accept the results obtained by Kundt and Warburg on their experimental evidence.

They have found that the velocity of sliding of the gas over the surface due to a given tangential stress varies inversely as the pressure.

The coefficient of sliding for air on glass was found to be $\lambda = \frac{10}{p}$ centimetres, where p is the pressure in millionths of an atmosphere. Hence at ordinary pressures λ is insensible, but in the vessels exhausted by Mr. Crookes it may be considerable.

Hence if close to the surface of a solid there is a tangential stress, S, acting on a surface parallel to that of the body, in a direction h, parallel to that surface, there will also be a sliding of the gas in contact with the solid over its surface in the direction h, with a finite velocity $= S\frac{\lambda}{\mu}$.

13. I have not attempted to enter on the calculation of the effect of this sliding motion, but it is easy to see that if we begin with the case in which there is no sliding, the effect of permission being given to the gas to slide must be in the first place to diminish the action of all tangential stresses on the surface without affecting the normal stresses; and in the second place to set up currents sweeping over the surfaces of solid bodies, thus completely destroying the simplicity of our first solution of the problem.

14. When external forces, such as gravity, act on the gas, and when the thermal phenomena produce differences of density in different parts of the vessel, then the well-known convection currents are set up. These also interfere with the simplicity of the problem and introduce very complicated effects. All that we know is that the rarer the gas and the smaller the vessel, the less is the velocity of the convection currents; so that in Mr. Crookes's experiments they play a very small part.

Mathematical Society, April 11.—C. W. Merrifield, F.R.S., vice-president, in the chair.—Mr. Artemas Martin, Pennsylvania, was elected a Member, and Messrs. W. M. Hicks and T. R. Terry were proposed for election.—The Chairman, on the recommendation of the Council, nominated Messrs. Brioschi, Darboux, Gordan, Sophus Lie, and Mannheim for the honour of Foreign Membership.—Prof. H. J. S. Smith, F.R.S., vice-president, read two papers: second notice on the characteristics of the modular curves, and a note relating to the theory of the division of the circle.—Mr. Tucker communicated a letter from Prof. Tait, and read an abstract of a paper by Prof. Minchin on the astatic conditions of a body acted on by given forces, and a portion of a paper by Mr. C. Leudesdorf on certain extensions of Frullani's theorem.

Royal Astronomical Society, April 12.—Lord Lindsay, president, in the chair.—A paper was read by Capt. Abney, R.E., F.R.S., on photography at the least refrangible end of the solar spectrum, and some photographs of spectra of great interest were exhibited to the Fellows. Some discussion ensued, and Dr. De la Rue asked a question respecting colour photography; Capt. Abney attributed such phenomena to different degrees of oxidation of the spectrum. The President brought up Dr. Draper's discoveries, but Capt. Abney declined to speak upon that subject.—The Astronomer-Royal remarked upon the proposal to set up a statue of the late M. Leverrier, showing that the gratification and pride which the neighbours of such a great luminary would naturally take in setting up his monument ought not to be snatched from them by the intervention of strangers. It was also pointed out that the charter of the Society does not admit of any subscription in its corporate capacity.—Mr. Christie read a letter from Mr. Ellery upon Mars at opposition, 1877. It appeared that the planet was very ill-defined, and not much good could be done with it.—Mr. B. G. Jenkins read a paper on the transit of Mercury, summarising the history of such phenomena, and referring to the spot of light on the disc and the ring round the limb, and their variations corresponding with perihelion and aphelion transits. Mr. Chambers suggested that if this paper were published before next transit it would be of great value, whereas, according to the practice of the present editor of the *Notices*, that would not be done. Prof. Cayley, the editor, made the proper excuses for the lateness of the publication.—The Astronomer-Royal announced his intention to assist competent observers, who wished to observe the transit, by giving them the use of the telescopes which were employed for the transit of Venus.—Mr. Green read a letter from Prof. Schiaparelli on Mars as seen recently a long time after opposition, and showed some curious drawings.

Chemical Society, April 18.—W. Crookes, F.R.S., in the chair.—The following papers were read:—On terpin and terpinol, by Dr. Tilden. The author prepared crystallised terpin, $C_{10}H_{20}O_2 . OH_2$, by Wigger's process, and obtained the same compound from American and French terpentine, but did not procure any crystalline substance from the terpenes of the orange group. By the action of dilute hydrochloric acid on terpin, an oily body, terpinol, boiling 205°–215°, was obtained, having the formula, $C_{10}H_{18}O$. By the action of dry hydrochloric acid on terpinol, a dihydrochloride was prepared. The author believes that in the preparation of terpin by the ordinary process, terpinol is formed at a certain stage of the reaction. By acting on terpin with dilute sulphuric acid, a hydrocarbon, $C_{10}H_{16}$, boiling at 176°–178°, sp. gr. 0·8526 was obtained; it is optically inactive, and gives no crystalline deposit with hydrochloric acid, and no crystalline nitroso compound; the author proposes to call it terpinylene.—The poisonous principle of *Urechites suberecta*, by J. J. Bowrey. This plant grows wild in Jamaica; it has dark green leaves and large bright yellow flowers; it is locally called "nightshade." It is known to be very poisonous. The author has extracted from the fresh leaves of the plant, by the use of alcohol, water, and a temperature not exceeding 38° C., a white crystalline body, urechitin, $C_{28}H_{42}O_8$, to the presence of which the plant owes its poisonous properties. It is very soluble in hot alcohol, chloroform, and glacial acetic acid; almost insoluble in water and dilute spirit. It is intensely bitter, and very poisonous; it gives, with strong sulphuric acid, a characteristic colour reaction. The liquid passing from yellow through red to purple, a trace of nitric acid increases the rapidity of the colour-changes. If the leaves are dried at 100°, urechitoxin is obtained, either crystalline or amorphous. This substance resembles urechitin in its chemical and toxical properties. Both substances are glucosides.—The temperature at which some of the alkaloids, &c., sublime as determined by an improved method by A. W. Blyth. The author has determined the melting and subliming points of many active vegetable principles, and classed them as regards their behaviour to heat for practical purposes. He has also devised a new method for determining subliming points: it consists essentially in placing the substance on a thin cover glass floating on a bath of mercury, and examining a second cover glass placed over the substance, from time to time with a ¼-inch objective, the mercury being gradually heated.

Entomological Society, April 3.—H. W. Bates, F.L.S., F.Z.S., president, in the chair.—Miss E. A. Ormerod was elected a Member of the Society.—Mr. McLachlan remarked that the opinion expressed by Mr. J. P. M. Weale at the last meeting as to the functional purpose of the cephalic process in *Termes triniverius*, was corroborative of an observation already recorded in Hagen's "Monographie der Termiten."—Mr. F. Grut exhibited, on behalf of the Rev. T. A. Marshall, a collection of insects which that gentleman had made in the Windward Islands.—Mr. F. Smith exhibited a series of specimens of a species of "harvesting ant" sent to Mr. Darwin from Florida by Mrs. M. Treat. Three series showed a gradation from large soldiers and small workers, all having acutely dentate

[1] Wiener Sitzb., xl. (1860), p. 607.
[2] Pogg. Ann., clv. (1875), p. 337.

mandibles, to other ants of all sizes with mandibles having rounded teeth, and other specimens in which the teeth were obsolete. It was not, however, made clear whether intermediate forms of teeth were found in nests, or whether three distinct races existed. The species appeared to be identical with *Myrmica barbata* from Texas.—Mr. A. A. Berens exhibited two examples of *Thestor mauritanicus* taken on the Atlas Mountains.—Mr. McLachlan exhibited a coleopterous larva sent from Zanzibar by Dr. Kirk. He also exhibited a portion of the stem of a coffee-tree which had been bored into by this larva, and which was especially remarkable on account of the presence of a series of conical holes which opened a communicator between the inner gallery and the atmosphere.—Mr. W. C. Boyd exhibited and made some remarks on a specimen of *Pterophorus lætus* taken at Deal.—The Secretary read a paper communicated by the Rev. T. A. Marshall, entitled "Notes on the Entomology of the Windward Islands."—The Rev. H. S. Gorham communicated descriptions of new species of Cleridæ, with notes on the genera and corrections of synonymy.—Dr. D. Sharp communicated a paper on some Nitidulidæ from the Hawaiian Islands.—The Secretary read a paper by Mr. J. P. M. Weale, entitled "Notes on South African Insects," and exhibited drawings made by the author in illustration.—Mr. Wood Mason exhibited and made remarks on the insects referred to in the foregoing paper, and was followed by Mr. Meldola on the same subject.—The following papers were also communicated:—On display and dances by insects, by Mr. A. H. Swinton; and On the secondary sexual characters of insects, by Mr. J. W. Slater.—Part V. of the *Transactions* for 1877 was on the table.

Geological Society, March 20.—Henry Clifton Sorby, F.R.S., president, in the chair.—John William Head was elected a Fellow of the Society.—The following communications were read:—On the chronological value of the triassic strata of the south-western counties, by W. A. E. Ussher, F.G.S.—Note on an *Os articulare*, presumably that of *Iguanodon mantelli*, by J. W. Hulke, F.R.S., F.G.S.—Description of a new fish from the lower chalk of Dover, by E. Tulley Newton, F.G.S.—Further remarks on adherent carboniferous productidæ, by R. Etheridge, jun., F.G.S.—The submarine forest at the Alt Mouth, by T. Mellard Reade, F.G.S.

Institution of Civil Engineers, April 20.—Mr. Bateman, president, in the chair.—The papers read were descriptive of three bridges on the Punjab Northern State Railway, viz., "The Ravi Bridge," by Mr. R. T. Mallet, M. Inst. C.E.; "The Alexandra Bridge, over the Chenab," by Mr. H. Lambert; and "The Jhelum Bridge, by Mr. F. M. Avern, M. Inst. C.E.

Victoria (Philosophical) Institute, May 6.—A paper on the physical geography of the East, by Prof. J. L. Porter, LL.D., was read. A discussion ensued, in which many Eastern explorers and others took part.

PARIS

Academy of Sciences, April 29.—M. Fizeau in the chair.—The following among other papers were read:—The theory of germs and its applications to medicine and surgery, by MM. Pasteur, Joubert, and Chamberland. It is shown to be possible to produce at will purulent affections either putrid or without any putrid element, or anthracic, or variable combinations of these kinds of disorder, according to the specific *microbes* that are caused to act on the living organism.—Experiments relating to the heat which may have been developed by mechanical actions in rocks, especially in clays; consequences for certain geological phenomena, notably for metamorphism, by M. Daubrée. He measured the rise of temperature produced in hard clay passed between rotating cylinders and between fluted cones; also the effect of pug-mills. In one case of pug-mill action for an hour the rise was more than 30°. For the same times, however, the heating effect is greater with cylindrical rollers.—Experiments with a view to determine the true origin of the chorda tympani, by M. Vulpian. These favour the conclusion that the nerve proceeds not from the facial nerve nor the intermediate nerve of Wrisberg, but from the trigeminus.—On magnetic rotation of the plane of polarisation of light under the influence of the earth, by M. Becquerel. Between a Jellet polariser and an analyser, with telescope and divided circle, is placed a tube (0·5 m. long) with parallel glass ends and containing sulphide of carbon. By means of terminal plane mirrors the luminous ray is successively reflected, the rotation being thus increased. The luminous ray comes to the eye after traversing the tube five times. Now, if the system be placed in the plane of the magnetic meridian, the plane of polarisation is not the same in looking north and in looking south; there was an angular differece of about 6′·5 between these positions. On the other hand, when the system is placed at right angles to the magnetic meridian, the same direction of the plane of polarisation is got, whether one looks east or west, and it is the bisecting position of the former two. The angular difference is considered due to the action of the earth.—Suppression of the return wire in use of the telephone, by M. Bourbouze. Connecting to earth by means of plates of gilt copper about 1 m. by 2 ctm., placed at 40 to 50 ctm. depth in garden soil, he got more distinct transmission.—On the transparency of coloured flames for their own radiations, by M. Gouy. Two layers of incandescent vapour, of the same density and temperature, but of very unequal thickness, give very different spectra. One cannot, from an examination of the lines of any spectrum, draw any conclusion as to the physical state of the vapours producing it, unless their thickness be known and taken into account.—On the solution of platinum in sulphuric acid, by M. Scheurer-Kestner. In apparatus of ordinary concentration the solution varies from 1 gramme to 8 grammes per ton of concentrated acid, according as the product obtained contains 94 or 99 per cent. of monohydrated acid; with fuming acid the quantity of metal may amount to 1,000 grammes. But by lowering of the boiling point and diminution of the platinum, as in Kesler's apparatus, the loss of metal may be greatly lessened.—On the vapour density of sulphide of ammonium, by M. Salet. He experimented by mixing at 80° known volumes of sulphuretted hydrogen and of ammonia; in no case was any contraction observed.—Experiments on the effects of lateral compression or crushing in geology, by M. Favre. In these experiments a layer of clay was made between two blocks of wood fixed on a piece of caoutchouc, which was first stretched and, after receiving the clay, was allowed to contract. Various phenomena of mountain chains were thus reproduced.—On the daily oscillation of the barometer, by M. Cousté. He considers it due to variations (1) in the quantity of atmospheric vapour of water, (2) in vertical ascending currents formed partly by the dilated air, but more by the water vapour developed by the sun in the low and middle layers, condensed anew in the upper layers.—Remarks on a letter of M. Wolf, on the period of daily variations of the inclination needle, by M. Faye. He gives a table of sun-spot minima covering 267 years, and showing 11·11 years as the period.—On the ultra-violet absorption spectra of earths of gasolinite, by M. Soret. The lines of a new base were observed.

CONTENTS PAGE

THURSDAY, MAY 16, 1878

THE MICROPHONE

WE were enabled to announce a fortnight ago, that Prof. Hughes, the inventor of the type-printing telegraphic apparatus which goes by his name, has made the wonderful discovery that certain bodies are sensitive to sound, in the same way as selenium is sensitive to light. That is to say, if we place these bodies in the circuit of a small battery, and subject them to sound-vibrations, in other words, talk at them, the electric current continually passing through it will be so continuously modified by the voice that the object may be used instead of a telephone for sending a message.

Since our note was penned an opportunity has been afforded us by the kindness of Prof. Hughes, of inquiring into the precise manner in which this and other startling results have been accomplished.

The impression left upon us by a careful following of all Mr. Hughes's experiments, is, that by them we are brought face to face with one of the most wonderful discoveries of the century. To see Prof. Huxley, who was one of those present, solemnly talking at a small glass tube about two inches long, was, in itself, a sight worth seeing; but to go into another part of the house, and, on putting a telephone to the ear to find that the talking at the glass tube there resulted in a quite perfect, very easily audible reproduction of the quality of every word which the Professor uttered, was a thing almost transcending the marvellous.

That by such experiments as these we are beginning to tap sources and modes of energy hitherto undreamt of was rendered most evident by an experiment which has suggested the name, placed at the head of this article, for the instrument by which it is accomplished. The delicate rubbing of a fine camel's hair pencil over a smooth wooden surface under certain conditions of contact, although, of course, inaudible in the ordinary way, was rendered evident in the telephone by a crackling noise, of which the intensity was almost painful to the ear. In this way Mr. Hughes has enabled Mr. Preece to hear a fly walk; we were not so fortunate as to hear this, because the only small fly available in the room, after having been carefully hunted down and inclosed in a small tumbler, obstinately declined to walk on the wood.

We have said so much by way of giving an idea in the first instance of the manner and result of the experimentation. The kind of inquiry into the molecular structure of bodies it renders possible, and the applications to which, undoubtedly, it will soon be put, will be best grasped after a somewhat detailed description of the apparatus itself. This description was given by Mr. Hughes at the meeting of the Royal Society held on Thursday last, and it may safely be said that never was a more difficult problem presented to men of science by simpler apparatus.

Although a telephone, as will be seen, is part of the apparatus utilised, the total problem presented by Mr. Hughes is a very much more complex one than that presented by that most marvellous of modern instruments.

Mr. Hughes has employed the telephone as a phonoscope of the greatest delicacy, to detect variations in currents, and the consequent reproduction of sound. The materials experimented upon by him were arranged as in the following sketch, in which B represents a battery, S the source of sound or material examined, and T the telephone or phonoscope.

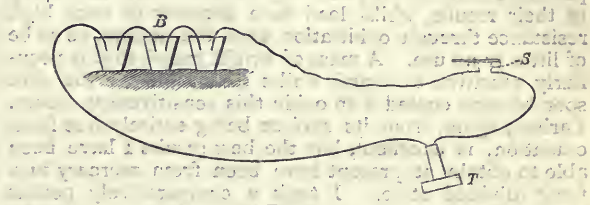


FIG. 1.

The battery was a simple Daniell's cell, of Minotto's form, made by using three common tumblers, a spiral piece of copper wire being placed at the bottom of each glass and covered with sulphate of copper, and the glass being then filled with well-moistened clay and water. A piece of zinc as the positive element was placed upon the clay. Insulated wires were attached to each plate, and three of these cells were joined in series.

All experiments were made on a closed circuit.

Prof. Hughes's work was begun by studying the effect of strains, Sir W. Thomson and others having shown that the resistance offered by wires to currents is affected by them, it followed that as the conveyance of sound vibrations must induce variations in strains, the wire resistance should vary when it was used to convey sound.

A stretched wire was therefore, in the first instance, introduced at S. The wire was talked at, but no effect was marked until a breaking strain was applied; at the moment of breaking, a sound was heard. Next, the broken ends were pressed together. The next stage in the experiments we quote from the paper itself:—

"It was soon found that it was not at all necessary to join two wires endwise together to reproduce sound, but that any portion of an electric conductor would do so even when fastened to a board or to a table, and no matter how complicated the structure upon this board, or the materials used as a conductor, provided one or more portions of the electrical conductor were separated and only brought into contact by a slight but constant pressure. Thus, if the ends of the wire terminate in two common nails laid side by side, and are separated from each other by a slight space, were electrically connected by laying a similar nail between them, sound could be reproduced. The effect was improved by building up the nails log-hut fashion, into a square configuration, using ten or twenty nails. A piece of steel watch-chain acted well. Up to this point the sound or grosser vibrations were alone produced, the finer inflections were missing, or, in other words, the *timbre* of the voice was wanting, but in the following experiments the *timbre* became more and more perfect until it reached a perfection leaving nothing to be desired. I found that a metallic powder such as the white powder—a mixture of zinc and tin—sold in commerce as "white bronze," and fine metallic filings, introduced at the points of contact, greatly added to the perfection of the result."

Here, then, was articulate speech clearly reproduced.

Prof. Hughes's next efforts were to discover the best material and form to give to his apparatus.

"Although I tried all forms of pressure and modes of contact, a lever, a spring, pressure in a glass tube sealed

up while under the influence of strain, so as to maintain the pressure constant, all gave similar and invariable results, but the results varied with the materials used. All metals, however, could be made to produce identical results provided the division of the metal was small enough, and that the material used does not oxidise by contact with the air filtering through the mass. Thus platinum and mercury are very excellent and unvarying in their results, whilst lead soon becomes of such high resistance through oxidisation upon the surface, as to be of little or no use. A mass of bright round shot is peculiarly sensitive to sound whilst clean, but as the shot soon become coated with oxide this sensitiveness ceases. Carbon, again, from its surface being entirely free from oxidation, is excellent, but the best results I have been able to obtain at present have been from mercury in a finely divided state. I took a comparatively porous non-conductor, such as the willow charcoal used by artists for sketching, heating it gradually to a white heat and then suddenly plunging it in mercury. The vacua in the pores, caused by the sudden cooling, become filled with innumerable minute globules of mercury, thus, as it were, holding the mercury in a fine state of division. I have also tried carbon treated in a similar manner with and without platinum deposited upon it from the chloride of platinum. I have also found similar effects from the willow charcoal heated in an iron vessel to a white heat, and containing a free portion of tin, zinc, or other easily vaporised metal. Under such conditions the willow carbon will be found to be metallised, having the metal distributed throughout its pores in a fine state of division. Iron also seems to enter the pores if heated to a white heat without being chemically combined with the carbon as in graphite, and, indeed, some of the best results have been obtained from willow charcoal containing iron in a fine state of division.

"Pine charcoal treated in this manner (although a non-conductor as a simple charcoal) has high conductive powers, due to the iron; and from the minute division of the iron in the pores, is a most excellent material for the purpose."

The substances above referred to are in practice confined in a glass tube or box, and provided with wires to enable them to be easily inserted into a circuit. This is called a transmitter.

The resistance of the conductors employed is affected by sounds absolutely inaudible, and it is this quality which Prof. Hughes utilises in what he calls *par excellence* his *microphone*. This marvellous instrument, of which we shall hear so much in the future, consists of a lozenge-shaped piece of gas-carbon one inch long, $\frac{1}{4}$ inch wide at its centre, and $\frac{1}{8}$ of an inch thick; the lower pointed end pivots upon a similar block, the upper rounded end plays free in another carbon block; all these pieces of carbon are tempered in mercury, and carbon is used in preference to any other material, as its surface does not oxidise. Prof. Hughes, in his paper, states:—

"The best form and materials for this instrument, however, have not yet been fully experimented on. Still, in its present shape, it is capable of detecting very faint sounds made in its presence. If a pin, for instance, be laid upon or taken off a table, a distinct sound is emitted, or, if a fly be confined under a table-glass, we can hear the fly walking, with a peculiar tramp of its own. The beating of a pulse, the tick of a watch, the tramp of a fly, can thus be heard at least a hundred miles distant from the source of sound. In fact, when further developed by study, we may fairly look for it to do for us, with regard to faint sounds, what the microscope does with matter too small for human vision."

The construction of the tube-transmitter exhibited to the Royal Society will be seen from the annexed woodcut. It consists of an exterior glass tube, G, two inches long and $\frac{1}{4}$ inch in diameter; in it are four separate pieces of willow charcoal. A is made to press on B, C, D, E, and F, until the resistance offered to the current is about one-third that of the line on which it is to be employed.

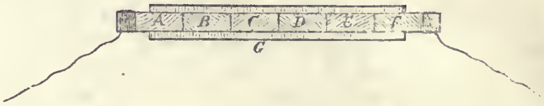


Fig. 2.

As Prof. Hughes properly remarks, it is as yet impossible to say what effect will flow from this wonderful discovery, a discovery which shows that it is possible to transmit clear and intelligent articulate speech, and to render the inaudible audible by the mere impact of sound waves upon matter along which an electric current is flowing.

It is not too early, however, to see that we have in the microphone a new method of attaching and quantifying molecular motions.

PHYSICAL SCIENCE FOR ARTISTS[1]

II.

THE examples I gave in my last paper were tested by a reference to the probable action of the aqueous vapour of our atmosphere in absorbing the various constituents of sunlight—the sun being the great source of light with which artists are specially concerned.

The reason that such a test was not applied long ago was because we are only just now beginning to understand why it is that the sun shines; why its light is white, and again why it is that this white light in passing through a great thickness of our atmosphere as it must do at sunrise and sunset—when the beams graze the surface of the earth instead of impinging upon it at a high angle—is in great measure absorbed or used up before it gets to the eye. The result of the condition to which I have just referred is familiar to all; at sunrise and sunset the sun is red and not white.

The light of the sun we know now is due to the quivering or vibration of the molecules of the matter of which the sun is composed. No molecular vibration no light; given molecular vibration, the more intense it is the more intense is the light produced. The absorption of the sunlight by our air in the manner I have stated is due to the molecules of our air already in vibration being set in still stronger vibration by the sunlight passing through them. Here again then we have molecules and vibrations. In short the vibration of molecules, so far as light is given out or reflected or quenched by them, sharply defines the physical region in which artists are chiefly interested.

In a work which recently appeared,[2] I have tried to show how the actions involved in sending a telegraphic message may help us to form a mental image of what goes on before the sensation of light is produced; we have a sending instrument, a medium, and a receiving instrument.

[1] Continued from p. 31.
[2] "Studies in Spectrum Analysis."

The first, under all circumstances, is a molecule or series of molecules in vibration, and the quality of the light depends upon the vibration either inherent in the molecule or dependent upon the quality of the energy which sets it in vibration or controls the vibration.

The second is the ether, which does for light what our atmosphere does for sound. Competent to transmit vibrations of all lengths without loss of energy, it behaves with perfect fairness, so to speak, to light of all kinds.

The third, the receiving instrument, in our case is the eye of the artist above all things, but not to the exclusion of everything else, because every object which reflects light must receive it first, and sometimes important modifications are brought about in the act of reflection. To mention two instances :—white light from the sun falls on a leaf, but leaves appear green by the light which they reflect, and this transformation is the result of molecular work. The light of the moon is yellow in comparison with sunlight for the same reason, and the difference between sunlight and moonlight effects has its origin in the lunar molecules.

We call the light of the sun white, and much of the action of light may be studied by supposing this light to be a simple thing, by which I mean non-compound. A hole in a shutter through which the sun shines will convince us that light travels in straight lines. The idea of a modern novelist that light can travel spirally through a key-hole is not based on fact.

It may happen, too, if there be a brightly illuminated object outside the hole in the shutter, that another point not to be neglected will be illustrated. Because light does travel in straight lines the various rays coming from the different parts of an object and passing through a small aperture will build up an inverted image of it on the other side of the aperture. The most obvious bearing of this in artist's work is that if the sun shines through the narrow apertures left by leaves in a thick wood, we shall have images of the sun on the ground; the shadows of the leaves will be dominated by circular intervals, and the higher the leaves the larger these circles will be. During the eclipse of 1870 the images of the delicate crescent of the sun thrown on the ground through the orange and olive trees were most perfect in form, and produced a strange effect never to be forgotten.

The laws of reflection can also be studied without any higher knowledge of the properties of light, and the difference between "specular reflection," the case in which light is reflected as in a mirror, and "scattering," in which, in consequence of the roughness of the surface on which it falls it is thrown off in all directions, will be at once recognised.

Water is the great reflector employed by the artist. Take, for instance, No. 643 in this year's Academy. If a painter will imagine a vertical plane passing through the object reflected—say a hill-top, and his eye; and plot a section with the height of his eye and the hill-top above the water and the distance between them roughly to scale; and if he will further recollect that the lines which connect the reflecting point of the water with his eye and the hill-top must make equal angles with the water level, he will find all he needs to insure correctness. In a picture taken, *e.g.*, with the eye at *a*, in the annexed woodcut, he will not include the distant hill-top at *y*, while with the eye at *b* he would do so.

In tranquil water the same consideration determines the locus of the reflection of the sun or moon. But if there be scattering, there will be a wake. The above

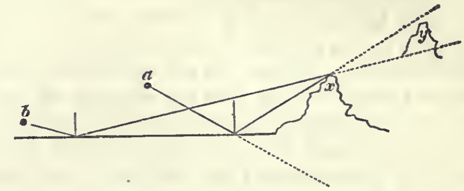


FIG. 1.

reasoning will show that it will be absolutely incorrect to throw this wake athwart the picture; nevertheless, this has been done, and by artists of the highest celebrity.

It is, perhaps, in the case of reflection of light by the poor moon that the modern artist comes to the greatest grief; and yet the only peculiarity in this case of reflection of light is that we are dealing with reflection from a spherical body which changes its place with reference to the light source and our eyes. If an artist would amuse himself any evening with his children in imitating these conditions with a lamp and some oranges he would never make another mistake. We should have moons painted with discretion instead of *à discretion*, and I fancy the "balance of the picture" would be found to be much less frequently disturbed by scientific accuracy than is generally imagined. I have known an artist to defend a crescent moon directly opposite a setting sun on the ground that if the moon had been painted full this much-prized "balance of the picture" would be entirely upset; and yet when it was suggested that the effect of two moons should be tried the idea was scouted as ridiculous—why, I could not understand from the standpoint taken by the artist.

Five minutes' reading of any elementary astronomy is all that is necessary to show that when the moon is on one side of the horizon, say east, and the sun on the other, say west, the observer must be between them, and that therefore the moon's reflecting side in its entirety must be turned towards him. It will not want even this amount of reading to convince him that if a sunset is painted as in Fig. 2, by no possibility can a globe at

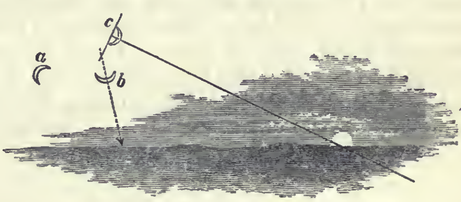


FIG 2.

a be lighted up, as shown. Nor can it be lighted up by the sun as shown at *b*, because the sun must be somewhere on the dotted line. If the moon is drawn at *c* the illumination must be symmetrical with reference to the line joining the moon and sun, and the nearer the moon is apparently to the sun, the more delicate must the crescent be.

Enough for the present on this subject. There is, however, one other point worth notice, which, although it is more astronomical than physical, I may perhaps be

allowed to refer to here. When an artist wishes to introduce a constellation as well as a moon into his picture, he should especially avoid the familiar northern ones, especially the Great Bear, as the moon has never been known to pass through that constellation. Still if my memory serves me, she has been painted in her silent majesty rendering glorious the sky over a village church, represented broadside on with the chancel to the right. She had therefore been caught due north in an unguarded moment. With an *English* village church (which is bound to be truly oriented) represented in the fashion I have indicated it is also as well to avoid the temptation to show up the stained glass windows of the aisles by introducing a sunset behind them.

I wish to be as little digressive as possible, but there is one point more which demands a word in passing. If an artist will put the moon or the sun into a picture, he should understand that in nine cases out of ten he lays down an almost perfect scale by which the accuracy of his delineation of landscape may be tested. In spite of the strange physiological effect which gives us the exaggerated sensation of the size of the sun or moon when they are near the horizon (so that we can compare them with familiar objects), the real variation is practically *nil*, and for our purpose it is enough to say that both sun and moon steadily subtend an angle of half a degree. Now it is because we can observe this angle and because we know the distance of the sun and moon as well, that we can calculate the sizes of these luminaries. Similarly, if an artist paints Peckham Rise from Camberwell or Mount Everest from the valley of Cashmere (supposing either picture possible—I don't know), and then puts a sun or moon in, *par dessus le marché*, he gives all the data necessary for the determination of the height and size of the hills in question.

By the kindness of an American astronomer I can give some statistics of considerable interest on the heights of hills in the United States as roughly surveyed in this way—that is as determined by pictures in which, by means of the moon or otherwise, the necessary data are provided. The pictures on which they are based were exhibited in 1876 and 1877. One mountain (I think it was in Missouri, but its exact name has escaped me) reached the respectable elevation of 105 miles. The average height in the United States generally, taking the pictures all round, was 43⅗ miles. There was only one artist who had got a hill into his picture less than thirteen miles high, but he only succeeded in doing this once.

So much then with regard to reflection, and the digressions which reflection has suggested.

So far there has been nothing said about colour.

It has been known from the time of Kepler that the white light of the sun, and indeed of all bodies which emit it, is not the simple thing we have so far taken it to be. It is really a sensation produced in our eye by the commingling of an innumerable series of different wave-lengths of light, each one of which, taken separately, we are bound to consider as a pure colour from the physical point of view, however we regard the physiological action which gives rise to the sensation.

Nature shows us in the rainbow, about which I shall have something to say by and by, the breaking up of this complex beam of white light into its various elements. The physicist arrives at the same result by employing a prism. A round hole in a shutter, through which a beam of light is allowed to enter into a dark room, and a common lustre inserted in the path of the beam, is all that is required in the way of apparatus to demonstrate the marvellous phenomenon of the analysis of white light into its constituent elements.

We not only get colour as the result of the analysis of white light, but we get it as a result of molecular structure, that is, some bodies like the sun and a candle give us that kind of light which we can break up into a complete series of coloured constituents; other bodies give us light which is not white, which is coloured to begin with, and so remains coloured to the end of the chapter.

Let us now pass on to those principles, the application of which to the coloured phenomena with which artists have to deal will, I am sure, prove of the greatest interest. Here we must grope our way as well as we may be able in a region where at present the senses are entirely powerless. We are in the world of the infinitely little. We approach one of those questions of molecular physics which no doubt in a few years must become one of the chief fields of investigation to men of science.

If I take a lump of iron, it is in what is generally called the solid state. If I apply heat to it it becomes molten, and we call it liquid. If the heat is still further increased, we drive the molten iron into iron vapour, as we more commonly drive water into steam.

Now we have achieved these results gradually, breaking down the molecular structure of the iron and of the water until at length we have got the molecular structure down to that of its ultimate fineness; so that when we have got to the stage of vapour at the end of our labour we have got a condition of things in which the smallest particles, or the ultimate molecules, which go to make up iron and water are there in their individuality and exist as separate points.

Now, what can this have to do with colour?

It has this to do with it: if I make a lump of iron hot, it gives out light, because its molecular constitution is disturbed or rendered more disturbed by the conditions to which we expose it when we heat it, and this state of unrest is rendered visible to us by the phenomena of light. Here we have simply the reason why there is a visible universe at all. If it were not for the condition of unrest of matter, we should never see anything; and, therefore, so far as the sensation of light is concerned, the visible universe would cease to exist altogether if this condition of unrest were abolished.

This being so then, let us take each of those molecular groupings in the solid iron as being in a state of unrest. Let us take for granted that the phenomena of sight depend first upon the state of unrest; secondly, upon the state of unrest being communicated to the surrounding ether, which, as we have already seen, does for light what air does for sound; and that the state of unrest of the molecules of iron having set up an equivalent state of unrest in the ether, or, if you like it better, having set up a chain of vibrations in the ether, this ether is competent to communicate its own vibrations thus imposed upon it to the optic nerve in our eye.

It brings before that nerve such an accurate reproduction, so to speak, of the vibrations which were communicated to it at the other end by the vibrations of the molecules of iron, that we have the impression that we see a mass of white hot iron, because the ether was agitated by precisely a mass of white hot iron in the first instance.

Now, if we observe any mass such as this, giving us light in the ordinary way, we get merely the impression of form; but if we admit such white light to our eye through the prism, we get it transformed into a band of colour called a spectrum, because, as we have already seen, white light is built up of a gamut of light notes from the lowest note to the highest; that is—to talk in the language of colour—from the extreme red to the extreme violet, through all those colours which are so gloriously brought before us in the rainbow, and more too.

But this does not happen with all substances.

Let us go now from our mass of white hot iron back to those smallest particles of iron vapour, those ultimate molecules to which I first drew attention. If we subject the light which comes from them to the same treatment, that is, if we allow it to come to our eye through a prism, we find that we don't get the whole gamut of colour represented as in the former case. We only get a light note, so to speak, here and there. Ordinarily, the phenomenon is presented to us in consequence of the construction of the spectroscope, by a series of lines, because ordinarily the spectroscope, is so arranged that the vibrations from any set of molecules are made to paint for us images of a fine aperture called a slit, through which the light is made to pass. If the light is discontinuous so far as the gamut of light is concerned, we only get a light note here and there. If it is continuous, the series of images is continuous; and we get what is termed a continuous spectrum, that is, the band of rainbow tints.

Now, mark this well, that when we treat the vapours of all metals that we know of in this way, we find that the arrangement of these bright lines, the arrangement of the images of the slit in other words, is different in the case of the vapour of every metal, so that we may say that no two vapours in Nature have the same colour.

We are now justified in saying, speaking in the language of molecules, that when we drive any chemical substance down to its ultimate fineness, and cause the ultimate molecules of each such chemical substance to vibrate, the vibrations from each chemical substance communicated to the ether and by the ether to our eye are so distinct that if we will take the trouble to record the effects thus obtained, we are for ever afterwards able to recognise the vibrations of that particular molecule, in whatever conditions we see those vibrations thus rendered spectroscopically visible to us, whether the molecules we are examining are in our laboratory, or in the most distant body in the depths of space.

The blue part of the spectrum which we obtain when we examine the light which is communicated to our eye by the vibrations of the finest particles of manganese, to take an example, contains a series of lines absolutely without any arrangement—a broad band here, a single line there, a double line elsewhere, and so on. In iron the arrangement of the lines is perfectly different. They are more numerous, and a detailed examination would convince us that it is quite as easy to make as definite a map of such a spectrum, and thus to point out the differences, as it is easy to make a map or drawing of any two things which differ in themselves.

This is sure and certain knowledge. It seems to deal with a condition of things with which the artist will never have to do. This is so, but the necessity for the statement of these facts will be abundantly seen in the sequel in which I hope to show that between the two extreme molecular stages to which I have drawn attention, that which always gives us white light, and that which gives us coloured light, the colour *in no case* being the same, we have stages with coloured light which practically is *always the same* for all bodies.

J. NORMAN LOCKYER

THE AMERICAN STORM WARNINGS [1]

STORM movements from west to east over Europe are familiar to your readers, yet I will refer briefly to them here. The chief storm routes are as follows:—From the regions immediately north of the British Islands to the Norwegian coast and over the Scandinavian Mountains to the Eastern Baltic and Central Russia, thence crossing the Ural Mountains into Siberia. From the British Islands directly to Denmark, North Germany, and Southern Russia to the regions north of the Caspian Sea, and from the British Channel, over the Netherlands, to Central Europe, passing north of the Alps into the Danube valley, and over the Black Sea to Asia Minor. The influence of position of the area of high pressure southward of the storm track on the direction of the latter is very great. Whenever the first described route is followed, the pressure is high over Great Britain, France, and Central Europe, and is falling in the Mediterranean and Northern Africa. If the second route is taken by the storm the barometer is high over Spain and the Mediterranean, giving the zone of low pressure a generally eastern direction. When the last-described route is followed, the pressure is high in the Atlantic off the coast of France and Spain, leaving a relatively low pressure over Great Britain, Southern Norway, and Sweden, the Western Baltic, and Central Europe.

The heaviest rains occur along the west coast of France and the British Islands, and on the Norway coast. The precipitation lightens eastward and south-eastward to Central Europe, but seems to increase again in the Danube valley, where the influence of moist air from the Mediterranean basin operates in increasing the energy of the storm. Over the whole field minor disturbances are frequently developed by the movements of the high pressures, which rarely become serious storms.

We must regard a knowledge of the general movement of the atmosphere as essential to success in the prediction of the movements of storms, their arrival in different regions, and their character. During the past year I have followed out and depended on the operation of what I conceive to be a law of atmospheric movement which I deduced from observations of the changes that occur over the areas of the American and European continents

[1] Continued from p. 34.

and of the Atlantic Ocean. I was early struck with the incompleteness of a theory which implied the development and movement of detached areas of high and low barometer, especially as such areas must be traced with more or less irregular but distinct outlines. I soon found that in order to justify the appreciation of the term "area" to such figures representing particular conditions, the pressures over large spaces between them should be left practically unaccounted for, and that the most singular alignments of isobars which appear on the weather charts of the United States Signal Service Bureau, and of the European observatories, would not satisfy the want. I became convinced that the lack of continuity in the recognised systems of high and low pressures was a vital fault which affected the whole fabric of weather prediction. Following the investigation from day to day, I found that these areas, so called, performed their procession across the weather chart with a very striking regularity, and that they maintained relations toward each other which gave me an idea of their general arrangement and distribution over the field of observation. By a practical application I found that the zone theory of pressures fulfilled all my expectations. By means of the facilities for cabling daily observations from Europe afforded by Mr. Bennett, and through the agency of the *Herald* ship news department in collecting meteorological data from the logs of vessels that were making West Indian and Transatlantic voyages, I have been able to establish the connection or rather the continuation of the American zones across the Atlantic Ocean and Europe.

Some two months after the *Herald* storm predictions had begun to attract attention in Europe, the late M. Leverrier, as director of the Observatory of Paris, requested that he should be informed fully on the *Herald's* system. At the same time he expressed the greatest interest in the work, and said that he regarded it as so far successful. In reply to M. Leverrier's inquiries I addressed him a letter dated July 10, 1877, on the subject of the *Herald's* weather warnings, from which I extract the following paragraphs explanatory of the zone law of atmospheric movements :—

"From a system of observations and comparisons which the *Herald* Meteorological Department has had in operation for over two years, I have been drawn to the conclusion that instead of forming a series of detached areas of erratic movement, the high pressure encircles the earth in a number of unbroken zones, the axes of which alter in direction under the influence of inconstant conditions. Also that between these zones of high pressure lie the zones of low pressure along which the storms take their courses. The normal direction of these zones is nearly parallel with the equator, but they are sometimes so displaced by a combination of influences that their axes form an angle of forty-five degrees and over with that line. I am also satisfied that between the southern extremity of Greenland and the equator there are two zones of low, and perhaps two, but certainly one, of high pressure. The approximate axis of the zone of high pressure lies between the 30th and 45th parallels of latitude, but, as I have already stated, this direction is subject to extraordinary variations.

"The zones of high pressure are defined by the isobars of 30·0 inches, or 762mm., and their margins, as well as their axes are undulating constantly under the influence of disturbances moving along the zones of low pressure. Storm areas or depressions are invariably found within the concave curvatures of these margins, and roll, as it were, along the lines which yield before them more or less readily. I therefore call these the undulating zones of high and low pressure, because of the constant wave motion observable in their axes and margins.

"The outlines of the zones of high and low pressure are governed by the movements and development of the storm centres or depressions. When disturbances of ordinary and uniform energy succeed each other at regular intervals on the northern margin, the axis of the zone of high pressure lying between the 30th and 45th parallels approaches in alignment to a regular undulation whose general direction is almost parallel with the equator. Modifications of this condition are caused by the relative infrequency of disturbances along the southern margin of the zone. When storms of unequal energy and development occur, the northern undulations become correspondingly irregular, and these reacting on those of the southern margin produce the distortions of outline which sometimes occur. The axis of the zone of high pressure assumes a compound undulatory movement which produces extraordinary variations of the weather within the range of its influence. You will readily perceive how the direction of the course of a southern storm could be changed by the influence of a northern disturbance, and how we can account in some measure for the eccentric movements of these meteors?

"Sometimes two, or even three storm centres, will force their way into the same concavity of the northern margin of the high-pressure zone. When it occurs, the combined energy of the storms enlarges the area of the general depression, absorbs or presses away the obstructing wave of dense atmosphere, or, may be, causes it to temporarily accumulate, with its apex or crest, far into the north. Then according to the resisting power of the wave in front, the storm centre commences to ascend toward the north along a gradually diminishing gradient which has the effect of throwing it on the Norwegian coast, or of giving it a curved course that will bring it from the north-westward toward the British Isles. Hence, although a storm centre may be leaving the Newfoundland coast apparently *en route* to Ireland, it is impossible to predict where it will reach Europe unless the direction and character of the undulation of high pressure in advance of it and the general trend of the axis of the zone of high pressure are known. These can only be ascertained by a series of daily observations extending over a large area such as that of the United States and Canada on this Continent, or through your international system in Europe and Western Asia. . . .

"In the centre, or nearly so, of each wave of high pressure, there is a point where the atmosphere attains its maximum density. This point is recognised ordinarily, and, indeed, properly, as the centre of the area of highest pressure, or anti-cyclonic area. The centres of highest pressure always alternate with the centres of lowest pressure when crossing a given meridian, the air movement around the former being with the hands of the clock, while that around the latter is in the contrary direction. I find that the general movement of the wind along the southern margin of the zone of high pressure is always westward, following the undulations, while that of the northern margin is eastward, also following the wave-lines. Now, taking this fact in connection with the geographical position of the zone of highest pressure between the 30th and 45th parallels, we have on the former line a general westward movement of the wind, corresponding with the trade winds, while on the 45th parallel, and north of it for some distance, the prevailing winds are from the westward. Again, a series of observations on our Atlantic and Pacific coasts, and in the interior, go to prove beyond question that some regions such as Lower California, and the south-eastern

portion of the Atlantic and Gulf States may be considered as permanently within the wave-lines of the zone of high pressure. This means that the curving of the northern and the southern margins of the zone give to these regions their prevailing winds, which, in the case of Florida and Georgia are called the 'trade-winds.' In the mid-Atlantic, north of the latitude of thirty degrees, there is another permanent area of high pressure and on the southerly side of the area are experienced the 'trade winds.' Now what are these 'trade winds' after all, but the westward flow of the air along the undulating southern margin of the zone of high pressure.

"Sometimes the zone of high pressure becomes compressed to a narrow band between two areas of low pressure, and large areas of high pressure will be formed both east and west of the narrow part of the zone. . . .

"You will naturally desire to know how we reconcile the development and movements of tropical cyclones with the foregoing statements. Although the subject is one that demands a more special treatment than it can receive from me now, I will state that, when at the period of the equinox the solar influence becomes a disturbing element in the meteorology of the equatorial zone, the alignment of the axis of the zone of high pressure becomes a compound curve, so as to present to the westerly flow of the winds and the ocean current a margin of the zone, in the hollow of whose undulations a vortex is developed. This, by cumulative energy, travels around the wave curve presented to it, and then moves away along the southern margin of the zone of high pressure toward the European coast. The cyclone that devastated Indianola and Galveston in September, 1875, passed almost in a straight line from South Carolina to Valentia, Ireland, and thence probably over Southern Sweden into Northern Russia."

To these remarks I would add that the zones of high pressure sometimes come together but do not merge, and that disturbances moving along the partly-closed zone of low pressure toward the region of contact between the two high-pressure zones, have the necessary energy to divide them again, and thus open a passage eastward between them. On the other hand, the zones close on a depression, and lift it to the higher levels of the atmosphere, where its humidity is condensed into rain that falls over a region whereon the surface-pressures are high.

With the object of utilising the zone law for the purpose of predicting the movement of storms across the oceans and continents, the meteorologist must watch the axial and marginal undulations, and be always aware of the general direction of the former. If this knowledge is possessed the prediction of storm movements becomes a simple matter of close observation and experience. To those who devote their whole attention to the work no difficulties can arise, because nature has fixed within certain degrees the limits of deviation which storm movements assume outside the normal courses for each class. The attendant conditions, such as rains, winds, variations of temperature, and humidity, and the presence of superabundant electricity are subject to greater or lesser modifications according as the storms traverse regions of land, water, mountain, or plain. I would call special attention to the appearance of storm centres over Norway and Eastern Russia, of which no serious indications of their movements were observable in the British Islands and France. Such storms occur always when the pressure is high over the countries last named, and they pass along the northern margin of the zone of high pressure north of the Hebrides or between the Faroe Islands and Iceland, and then move in a south-easterly direction with the undulation into the Baltic and the great plains of Russia, where they sometimes develop considerable energy. This is due to the operation on the eastern slopes of the Scandinavian Mountains of the same causes that combine to produce the north-western storms of Montana Territory.

It is unnecessary to refer to the local phenomena of European storms; they are so fully understood that it is impossible to add anything to the acquired information regarding them. Yet I mention them in the hope that some of the suggestions I have offered in the foregoing may be applied to the study of their development and nature.

JEROME J. COLLINS

SOLAR RADIATION

Les Radiations chimiques du Soleil. Par M. R. Radau. (Paris: Gauthier-Villars, Imprimeur-Libraire, Quai des Augustins, 1877.)

THE importance of an accurate registration of the comparative intensities of solar radiation is not to be over-estimated at a time when so much diversity of opinion exists regarding the climatic effect which the sun produces upon our earth. Such radiations have been divided into three classes, the heating, the visible, and the chemical or actinic; and though the whole of these three divisions are for the most part arbitrarily defined, yet such a mode of denoting the rays of light lying between certain limits in the spectrum is on the whole convenient, if the proper mental reservation be made. In the work before us we have an account of the various researches that have been made by different physicists for obtaining a measurement of the comparative intensities of the chemical radiations. An absolute measure of their energy has, up to the present time, been found impracticable, but by noting the amount of change produced by them in what are known as sensitive compounds, a *comparison* of the otherwise immeasurably small quantity of work that they are capable of performing can be made. The amount of chemical decomposition or combination, caused by the work performed by these rays, is in reality a measure of the work performed by some previous chemical operation together with the infinitely smaller quantity, due to the energy in these radiations. All processes, therefore, which have been employed for the purposes of actinometry, give results which are comparative measures of intensity, and not of absolute energy. Perhaps the nearest approach to an attempt at a measurement of the latter was by means of the chlorine and hydrogen actinometer of Bunsen and Roscoe, certain absorption experiments with which have been well described in the work before us.

In the introductory matter are cited the comparatively recent researches of Vogel of the Berlin Industrial College, in which he shows that by the addition of certain dyes to silver bromide he is able to alter the position of maximum sensibility of this compound to the spectrum. The results of these experiments are certain, but the explanation offered is perhaps more doubtful. It may be remarked,

however, that, as a rule, the dyes most effective are fluorescent, and capable of combining with silver, which may be a help in estimating the reason of the alteration. After referring to other researches of Chastaing and Berthelot, the author glances at the method of estimating the intensity of the chemical rays by the combination between chlorine and hydrogen. The feasibility of this plan was announced by Draper, in 1843, but it was only carried into practical effect by Bunsen and Roscoe, who published their first results in 1853. In 1857 Draper proposed an actinometer based on the decomposition by light of ferric oxalate into ferrous oxalate and carbonic anhydride. "Unfortunately," says the author, "Draper's process, if it leaves nothing to be desired in regard to precision and sensitiveness, is laborious in execution, and is hardly possible in practice."

A reference is also made to the use of a mixture of oxalic acid and uranium nitrate by Nièpce de St. Victor for the same purpose, with a statement that with the actinometer employed the readings were very precarious, though by improved apparatus they might be made more reliable.

Bunsen and Roscoe's actinometer, which, as before stated, depended on the combination between chlorine and hydrogen, is next fully discussed. The various experiments made to obtain a proper unit of intensity, to ascertain the absorption of rays due to the chemical operations performed by the light, and the relative effect of the different portions of the spectrum, are well worthy the attention of all workers in this branch of research.

In the next division of the book we have a *résumé* of the work done by, and the apparatus necessary for, an actinometer dependent on the darkening of silver chloride, the latest form of which was brought out by Roscoe in 1874, and is known as his automatic actinometer.

The substance of the various papers on the subject by Bunsen and Roscoe which have appeared in the *Philosophical Transactions* at different times have been condensed in a division entitled the "applications climatologiques," and very interesting it is. Thus we have an account of the measurement of the intensity of light proceeding from various parts of the sky with the sun at different altitudes; of a measurement of the intensity of direct sunlight; the effect of the height of the barometer and thermometer on the amount of chemical radiations; and of the variation of their intensity at different times of the year and at different latitudes.

In another division we have a discussion on the actinometer of Roussin dependent on the chemical reaction of nitro-prussiate of sodium and ferric chloride; of Phipson's proposal to employ molybdic acid dissolved in an excess of sulphuric acid; of Becquerel's ammonium oxalate with mercuric chloride actinometer, and also of his electro-chemical measurer; and finally we have a description of Marchand's researches, which at present have been but little known in England. We suppose it was impossible to close the subject without an article dedicated to "Light and Vegetation," which perhaps is long enough considering how little is really known of these relations.

We have been thus specific in giving the contents of Radau's little work, as it is in reality the only readily accessible account of the classical researches of Bunsen and Roscoe. The years which have been occupied in these investigations by these physicists number more than twenty, and it is doubtful if the results obtained have received all that attention from men of science which they deserve. No one who has not been engaged in similar experiments can be aware of the difficulties to be encountered in carrying them out; that they are great may be shown by the fact that no independent attempt has been made to check the results or at all events there are no published accounts of them.

The book is a compilation of the results obtained by various persons, and it would be out of place to criticise it in the same way we should if it contained original investigations made by the author. At the same time we may say that there are some few points in the various researches which are open to modification and even to correction, more particularly in those of Marchand.

BRAIN

Brain; a Journal of Neurology. April 1878. Part I. To be published quarterly. (London: Macmillan and Co.)

A NEW scientific quarterly has made its *début*, entitled *Brain*, a Journal of Neurology, edited by Dr. Bucknill, Crichton-Browne, Ferrier, and Hughlings-Jackson, names well known in connection with the physiology and pathology of the nervous system, and supported by an able staff of contributors.

According to the prospectus which has been issued, *Brain* will treat of the anatomy, physiology, pathology, and therapeutics of the nervous system, from the brain, downwards. The functions and diseases of the nervous system will be discussed, both in their physiological and psychological aspects, but mental phenomena will be treated only in correlation with their anatomical substrata, and mental disease will be investigated as far as possible by the methods applicable to nervous diseases in general. The want of such a journal has, say the editors, been long felt; and, considering the great advances which have been made of late years in the physiology of the brain and nervous system, and the great ignorance that prevails with respect to diseases of the nervous system, even among the majority of otherwise well-informed medical men, we believe the statement well founded, and anticipate a career of great usefulness to the newly-founded journal. In the first number the editors have fulfilled the greater part of their programme, which is stated to include original articles, critical digests, reviews, and abstracts of researches on the nervous system at home and abroad, correspondence on matters relating to neurology, &c. The strength of Part I. lies in its original articles. The first is by Mr. Jonathan Hutchinson, "On the Symptom-Significance of Different States of the Pupil;" the second by Mr. G. H. Lewes on "Motor-Feelings and the Muscular Sense;" the third by a French contributor, M. Duret, "On the *Rôle* of the Dura Mater and its Nerves in Cerebral Traumatism;" the fourth by Dr. Gowers, "On some Symptoms of Organic Brain Disease;" the fifth "On Brain Forcing," by Dr. Clifford Allbutt; the sixth by Dr. Bevan Lewis "On the Comparative Structure of the Cortex Cerebri;" and the seventh by Mr. Crochley Clapham, "On Skull Mapping." There are also clinical

cases by Drs. Hughes Bennett, Buzzard, and Urquhart. Dr. Ferrier gives an analysis of an important memoir by Dr. Duret, "On the Mechanism of Cerebral Concussion and Compression," and Dr. Bucknill reviews severely, but not beyond its deserts, a work by Dr. Bateman, intitled "Darwinism Tested by Language," in which the main point sought to be established against Darwin and evolution is the immateriality of the faculty of speech, and its being a distinctive attribute of man.

Several shorter notices are given of recent papers and lectures relating to the brain and nervous system. These might, with advantage, have been much more numerous, and we hope to see this part of the programme more completely carried out in subsequent numbers.

The original articles would require each a separate analysis to do them justice. We content ourselves in the meantime with merely mentioning their titles. They are all worthy of attentive study, and many of a high standard of excellence, as might indeed be expected from the names of the contributors.

While the majority of the articles in *Brain* are of special interest to physiologists and medical men, they will, at the same time, prove a rich field of material for those—a rapidly increasing army—who believe that psychology is to be advanced, not merely by interrogating consciousness, but by intelligent study of the relations between body and mind, as indicated by physiological research and the phenomena of disease.

While philosophical speculation has interest but for very few of the medical profession, the facts relating to diseases of the nervous system daily observed by medical men, and reported and commented on in a journal like *Brain*, ought to prove of value to all students of the problems of physio-psychology.

We heartily wish *Brain* all success and prosperity in its career.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

The New "Oil Immersion" Object-Glass Constructed by Carl Zeiss, of Jena

By the courtesy of its manufacturer, this remarkable lens was sent me, a fortnight since, that I might carefully examine it. The results may be of interest to those who have not seen the lens: and the statement of them is due to the industry and skill of the maker.

The lens has a focal length of one-eighth of an inch: it is an "immersion," but the fluid employed is the oil of cedar wood. The object of this is that the fluid placed between the lens and the covering glass of the object, may have refractive and dispersive indices as nearly as possible coincident with those of crown glass, the material of which the covers and the front lens are composed. Ol. ligni cedri is the liquid that has been found to be most capable of meeting these conditions; and by its use the covering glass, thick or thin, and the oil and lens, become practically one homogeneous whole; and the need for the "screw collar correction" for different thicknesses of cover, is done away. At the same time, and by the same means, a large and efficient "angle of aperture" is secured. Mr. J. W. Stephenson, F.R.M.S., suggested to Prof. Abbe this method, and Prof. Abbe and Carl Zeiss have together produced the glass.

As a piece of workmanship it is extremely fine; and it can be used with quite as much ease as an ordinary immersion ¼-inch objective. It works admirably with Powell and Lealand's ordinary sub-stage condenser, with Wenham's reflex illuminator, and with the small plano-convex lens which the maker sends with it to be fastened to the under-surface of the slide with the oil of cedar wood. But I have also secured admirable results with the illuminating lens of Powell and Lealand's supplementary stage, which gives entire command over the angle of the illuminating ray.

The "spherical aberration" in this lens is beautifully corrected; the "field" being perfectly flat. The colour corrections are, so far as the lens goes, equally perfect; but are somewhat conditioned by the dispersive power of the oil, which can be modified readily, and for which Carl Zeiss provides. The sharpness and brilliance of the "definition" which this lens yields is absolutely unsurpassed, in my experience; and it has a very great power of "penetration."

I tested it with a series of "tests" with which I have proved and compared the glasses of various makers in England, the Continent, and America for some years. Up to the time of receiving this lens, the ⅛-inch that had done the most in my hands, was one of the "new formula" lenses of Powell and Lealand. It is but justice to say that all my most crucial tests were equally mastered by the lens of Carl Zeiss. I have not been able to do more with it, than with the English glass, *but the same results* can be accomplished *much more readily*. The correction has to be brought into operation, and careful adjustment made, to get the finest result with the English lens; but the German glass has simply to be brought into focus, and the best result is before the observer, provided that the light has been adjusted in the most efficient manner. It is true that for sharp and perfect definition we must be careful to adjust the length of the draw-tube; in working this lens there is much need of attention to this matter; and speaking from a practical point of view, it takes the place, in securing crisp definition, of the screw-collar adjustment; although, of course, much easier of application. But it is so easy to work the lens with fine results on the more delicate tests, that I think that those who make the resolution of these their primary object in the possession of a microscope, can scarcely fail in securing their utmost desire. It is a glass pre-eminently suited for the resolution of difficult lined or beaded objects.

I have in my cabinet several frustules of *Navicula rhomboides* ("*N. crasinnervis*") which I cannot fully resolve with Powell and Lealand's new formula $\frac{1}{12}$-inch objective. But all that I *can* resolve with the English ⅛ I have resolved with the German glass. *Amphipleura pellucida* is easily resolved into delicate beads when the frustules are moderately coarse; and almost any that can be met with are resolvable into lines; and this when these diatoms are mounted in balsam. And the highest eye-pieces made may be used without any practical detriment to the image; although, of course, with a reduced sharpness of the definition.

On the whole, I think it in many senses the finest lens, of its power, that I have ever seen; and in every sense it is an admirable acquisition.

But it is a fact that even water "immersion" lenses are of very limited service in observations continuously conducted upon minute *living* organisms in fluid. We may gladly call in their aid, in the determination of a delicate change of form, or in the more perfect detection and definition, of an obscure point of structure; but for steady and constant work we are bound to avoid them; for the fluid under the delicate cover is in danger every moment of being "flooded" by coming into contact with the water on the top of the cover, and between it and the lens; because the movements of the organism have to be counteracted by the movements of the mechanical stage, in order to keep any form that may be studied in view constantly. But this opens to us the possibility of going to the edge of the cover at any moment; and thus, by the mingling of the fluids, rendering the observation void. This, of course, will apply still more fully when, as in the case of the valuable glass of Zeiss, the "immersion fluid" is an essential oil.

Happily it is only in special cases that the greater analysing power, combined with larger working distance, which is possessed by immersion lenses, is required. It is in the earlier study of an organism, and before continuous work upon it has begun. And even if it be not, in the majority of cases, a first-class dry English lens of a higher magnifying power, if efficiently

used, accomplishes all that is required. Hence the fine "new formula" lenses, *dry* (also provided with fronts to be used as immersion lenses), are as yet an unsurpassed boon for this special class of work. And certainly it is one which, in relation to biology, has a most important future. I know of course, that the optician has irresistible limitations to deal with; but the "new formula" dry lenses I have referred to, prove, in comparison with the preceding lenses, made by the same firm, that the dry lens was capable of most serviceable improvement. The same applies to a $\frac{1}{75}$th-inch lens, made recently at my request by the same skilful makers. As an analytical optical instrument, it is possessed of capacities far greater than are represented by its mere increase of *magnifying power over the* $\frac{1}{50}$th-inch objective, by the same makers; and equally so in relation to their $\frac{1}{50}$ths of six or seven years ago, when the superior magnifying power of the latter is considered. And yet the $\frac{1}{75}$th-inch and the $\frac{1}{50}$th-inch to which I refer, were admirable glasses, and have done excellent service. What is important, therefore, is that the larger demand for lenses that will "resolve" readily, difficult lined and beaded objects, which can certainly be best done, all things being equal, with "immersion" lenses; and to the improved manufacture of which Carl Zeiss' oil immersion gives apparently a new departure: should not lead the best opticians in England, the Continent, and America to abandon efforts for the still greater improvement of their dry lenses. They are of the greatest value to the practical biologist, working amidst the minutest living things in Nature, and from the study of which so much may be anticipated.

There is another feature in the use of this lens which is a drawback. The essential oil is a solvent of most of the varnishes and gums used in mounting, and "finishing," microscopical "slides;" and consequently some of our cherished "tests"—placed near the edge of the cover, and which we have been in the habit of using for years, will not serve us. And this, of course, has a wider application. But this may be overcome by coating the edge with shellac-varnish, which the oil does not dissolve; only this is extremely brittle, and is not to be depended on.

But it is further necessary, in using this lens, that the objects should be mounted in balsam, or some other fluid with an equal refractive index. The majority of "dry" mounted objects are by no means better shown by this lens than by an ordinary immersion lens. But this may be overcome if the objects, such as frustules of diatoms, be "burnt" on to the cover. This intimately unites the crown glass cover and the object, making them practically one. If this be not done the ray coming from the object has to enter air before passing into the lens, so neutralising the special properties of the glass. But here again the *special* objects—used, for example, as "tests"—and obtained as the result of years of careful selection, are of no avail.

But this glass will be of great value in the study of rock structures, &c., because the oil will render them transparent without special polishing; and its great working distance will in such work be a great boon.

It may perhaps be right to note that this lens, although not provided with the complex arrangement of "screw-collar adjustment," and although *only* "immersion," is higher in price than the most costly $\frac{1}{8}$th by any English maker, although the latter lens may have the screw collar correction, and be both "immersion" and dry. W. H. DALLINGER

St. James's Parsonage, Woolton, Liverpool, May 1

Science for Artists

IN NATURE, vol. xviii. p. 29, there is an article upon "Physical Science for Artists," in which one of my pictures is thus described: "No. 309. The Sunrise Gun, Castle Cornet, Guernsey—Tristram Ellis. Sky colour good; impossible colour of water under sky conditions given."

It is not usual for an artist to answer a criticism, but in this instance I do so purely upon scientific grounds. The water shown is slightly ruffled with a breeze blowing *towards* the spectator, and hence reflects a part of the sky which makes a greater angle above the horizon than the reflection makes below it. The central part of the sea would reflect that portion of the sky which is at the very top of the picture, and if the critic will kindly re-examine, he will find the colours of those parts almost identical. As the sky gets greener towards the zenith with the given kind of sunrise, the sea appears greener than the portion of the sky shown, and this effect is heightened by the strong *green local colour* of the water in the *shadows*. The sea was painted after careful consideration and study direct from nature, and remembering the breeze is nearly parallel with the line of vision, is, I think, correct. If the wind had been at right angles to this line the colour would have been quite different, and perhaps this is a matter which the writer of the article did not at the moment take into consideration. TRISTRAM ELLIS

Kensington, May 10

Time and Longitude

THERE is a practical answer to the problem put by Mr. Latimer Clark (NATURE, vol. xviii. p. 40). As a matter of fact the day begins, or rather the day is first named at the 180° meridian east or west from Greenwich; but this initial line, if I may call it so, diverges in the South Pacific to about 170° west from Greenwich, bringing many of the islands, as Fiji, Friendly, Sunday, Chatham, &c., into the same date with the nearest civilisation, Australia and New Zealand, Asia, &c. Without notes I cannot trace this line accurately between the Isles, but to take certain cases. Fiji counts its day east from Greenwich, Hawaii and Society west from Greenwich. At this moment I forget which division the Navigators enter, so to answer the problem, Where did last Monday begin?—At about 170° west longitude. Where did it end?—At 180° west in North Pacific. How long did it exist?—At any one place twenty-four hours, but taking adjacent places on either side of the initial line, Monday will have been a date during forty-eight hours; or if a vessel should be just on the eastern side of the 180° meridian, and keeping, as she should, Greenwich time through American route, Monday will have been a date during very nearly forty-nine hours.

The case proposed by Mr. Latimer Clark is no hypothetical one. During the war of 1855 the squadron in the Pacific was sent across to co-operate with the fleet in China. It found itself a day behind the China fleet as it had entered the Pacific round Cape Horn, whilst the China fleet had passed round the Cape of Good Hope, and for a short time the two fleets side by side kept different days. Again the steamers from San Francisco to Japan alter their dates temporarily whilst in Japan to suit the local reckoning, and enter both dates in the log. J. P. MACLEAR

May 13

Menziesia Cærulea

I AM rather surprised to see it stated by the Rev. M. J. Berkeley in NATURE (vol. xviii. p. 15) that the late "Dr. Thomas Thomson was so fortunate, after three times ascending the Sow of Atholl, as to *rediscover the long lost Menziesia cærulea*." I doubt if it was ever lost, certainly it has not been long lost. I find, on looking over my Herbarium, that my specimen was collected August 6, 1867; since then I have heard of it having been found by others. I saw several plants which I left, and I have little doubt that some of them are there still. Fortunately the preservation of the plant is due to the following circumstances:—1st. That it flowers in May; few botanists visit the Highlands till later in the year. 2nd. The plant has a considerable general resemblance to *Empetrum nigrum*. I have seen them growing in the *same tuft;* in such a case it requires a very sharp eye to distinguish one from the other even at a short distance. 3. The plants are *widely scattered* over the hill, so that it would require days to enable any one to say that it was lost; indeed no plant is likely to be lost so long as the natural conditions remain unchanged. It may be stolen but not lost. I take for granted, of course, that every true botanist will be merciful in such a case.

Edinburgh, May 6 ALEX. CRAIG CHRISTIE

"Hermetically Sealed"

WHAT is hermetic sealing? I have been under the belief that it means sealing with the material composing the object to be sealed; as in the case of sealing a glass tube in the spirit-lamp. M. Bordier's charming paper on the Greenland Eskimo (NATURE, vol. xviii. p. 16), says that an aperture in a hut is hermetically sealed with goldbeater's skin; and that a fisherman is hermetically enveloped round the loins by a leathern bag. You may, perhaps, think it worth while, in the interest of accurate scientific terminology, to settle the point. W. T.

May 10

The Structure of Coryphodon

I OBSERVE in your issue (vol. xvii. p. 340) a note by Prof. O. C. Marsh stating that I have included in the cast of the olfactory lobes of the brain of *Coryphodon* that of a part of the nasal cavity also. Prof. Marsh fails to point out the qualifying remarks to be found in my descriptions. In the explanation of Plate I. of the *Proceedings* of the American Philosophical Society, 1877, p. 620, I say, "The right bulbus of the olfactory lobe is too large above, owing to the want of preservation of the superior wall of the cavity." In my quarto report to Lieut. G. M. Wheeler in Vol. IV. p. 223 of his Report to the Chief of Engineers, I remark, "In excavating the matrix from the Olfactory chambers some difficulty was experienced in attempting to lay bare the superior and inferior walls, &c. On one side of the bulb this boundary was probably passed through, giving a larger vertical diameter than the true one."

Philadelphia, March 23 E. D. COPE

[Prof. Cope adds other remarks on this subject and on other questions in dispute between himself and Prof. Marsh, which our space will not permit us to reproduce.—ED.]

Lightning Phenomenon

ON observing the lightning on Friday evening I noticed that several of the brighter flashes were preceded by one, or sometimes two smaller flashes, the large flash following immediately after and taking the same course as the smaller ones. I should be glad to know if any of your readers observed this, and also how it is accounted for. H. J. STAPLES

Clifton College

Secondary Lunar Rainbow

AT an early hour this morning I had the good fortune to witness a phenomenon which is of somewhat rare occurrence.

Soon after 12.15 A.M., at which time I was burning the midnight oil, by a curious coincidence, over spectrum analysis, I was roused from my books by the pattering of rain outside the open windows; upon looking up I perceived that the moon was shining brightly, and naturally concluded that as it was not very far above the horizon there must be a rainbow.

I rushed over to the opposite side of the "quad." and was rewarded for my wetting by the appearance of a most magnificent bow, in which the colours were easily distinguishable on the dark background of clouds. To complete the phenomenon and to render it remarkable, there was also a perfect secondary arc.

The primary bow lasted in great brilliance for more than ten minutes, and thus I was enabled to rouse some of our men to see it, but it vanished with the punctuality of a creation of a fairy tale, immediately the clock tolled the half-hour. Soon after its disappearance the clouds were dispersed and the heavens studded with stars. W. J. NOBLE

Keble College, Oxford, May 12

OUR ASTRONOMICAL COLUMN

THE REAPPEARANCE OF TEMPEL'S COMET, 1873, II.—The *Bulletin International* of the Observatory of Paris for May 7, contains the elements of this comet for the return to perihelion in the present year, as determined by M. Leopold Schulhof from a complete discussion of the observations in 1873, and the application of the perturbations up to the next perihelion passage. Expressed in a form slightly modified from that adopted in the *Bulletin*, the elements are as follow:—

Perihelion passage, 1878, September 1·4961 G.M.T.

Longitude of perihelion	306° 7′ 2″	Mean Eq. 1878·0.
,, ,, ascending node ...	120 59 41	
Inclination of the orbit	12 45 34	
Eccentricity	0·552895	
Log. semi-axis major	0·476478	
Mean daily sidereal motion	684″·3689	

Motion—direct.

It will be remarked that the date of perihelion passage given by M. Schulhof's investigation is considerably later than was assumed in a former note in this column, from the best calculation of elements at the last appearance then available, and we believe it is yet open to material uncertainty. The recovery of the comet at this return may therefore involve a very close search with instruments of great optical capacity, but as it may, and very probably will be within reach when the moon is again below the horizon in the evenings, it may be hoped that no time will be lost in commencing a strict examination of the track of sky on which its orbit must be projected at different dates. The following positions are extracted from M. Schulhof's ephemeris:—

12h. Paris M.T.	Right Ascension. h. m. s.	Declination. ° ′	Distance from earth.	Intensity of light.
May 18	15 59 16	+ 6 32	0·779	0·54
,, 20	15 57 34	6 39	—	—
,, 22	15 55 49	6 45	0·755	0·59
,, 24	15 54 0	6 49	—	—
,, 26	15 52 9	6 50	0·734	0·65
,, 28	15 50 16	6 49	—	—
,, 30	15 48 22	6 47	0·715	0·70
June 1	15 46 29	6 43	—	—
,, 3	15 44 38	+ 6 36	0·700	0·75

The intensity of light at the date of the last observation of the comet in 1873, which was made at Mr. Bishop's Observatory, Twickenham, on October 20, was 0·385. At this time the comet was the *extremum visibile* with a 7-inch refractor. It may appear strange that observations should not have been made at a later period with the more powerful instruments available in many of the European observatories, but it does not always occur that the largest telescopes show decided advantage over much smaller ones in following up faint comets. With the same refractor De Vico's comet of short period of 1844 was distinctly visible on December 31 in that year, which is the date of the last observation at Pulkowa, and but for the intervention of clouds, observations would have been possible. Nevertheless neither object could have been detected, in all probability, if the positions had not been pretty accurately known, and thus the recovery of a faint periodical comet when it first comes within reach, subject as the calculated places may be to material error, is a very different matter to distinguishing a faint object when we know exactly where to look for it.

On this, the first return, of Tempel's comet of 1873 since its periodicity was discovered, the date of perihelion passage upon which the geocentric path depends may reasonably be expected to be several days in error, although not necessarily so. The following would be the variations in right ascension and declination on May 18 and 30, caused by assuming the perihelion passage to occur four days earlier or four days later than the time calculated by M. Schulhof:—

	P.P. four days earlier. R.A. m.	Decl. ′	P.P. four days later. R.A. m.	Decl. ′
May 18	+ 16·7	− 50	− 15·9	+ 48
,, 30	+ 17·9	− 45	− 16·7	+ 43

This comet was discovered on the night of July 3, 1873, by M. Tempel, at the Observatory of Milan, in the constellation Cetus. Its short period of revolution was pointed out about the same time by M. Schulhof and Mr. Hind, from the observations made in July. At aphelion it approaches the orbit of Jupiter within 0·64, and in about 309° heliocentric longitude is within 0·052 of the orbit of Mars.

OCCULTATION OF MARS.—On the evening of June 3 Mars will be occulted by the moon, and no other occultation of a bright planet, visible here, will take place for several years, until 1882 or later. At the Royal Observatory, Greenwich, the immersion will occur, according to the tables, at 10h. 1m. 9s., and the emersion at 10h. 42m. 49s., mean times, the angle at emersion counted as in the *Nautical Almanac* for inverted image being 288°.

If we apply to this case the method for distributing predictions over a certain extent of country, founding our calculation upon Greenwich, Edinburgh, and Dublin, we shall find the following expressions for the determinations of the Greenwich times of immersion and emersion for any place within or not far beyond this area. M is the longitude from Greenwich, in minutes of time, + if E., − if W., and the latitude of the place is put $= 50° + L$.

		h. m.	m.	m.
G.M.T. of immersion	= June 3,	10 3·50	− 1·60 L	− 0·151 M
G.M.T. of emersion	= ,,	10 44·40	− 1·07 L	− 0·114 M
The angle at emersion is	...	288°·9	− 0·74 L	− 0·12 M

As an example, suppose the times are required for Liverpool, longitude 12m. 17s. West, latitude 53° 24′.

$L = + 3·4.$ $M = − 12·28.$

$− 1·60 \times 3·4 = − 5·44$m.

$− 0·151 \times (− 12·28) = + 1·85$m.

Therefore time of immersion = 10h. 3·50m. − 3·59m. = 9h. 59·91m. or 9h. 59m. 55s. G.M.T. at Liverpool.

Similarly, the time of emersion will be found to be 10h. 44·40m. − 2·29m. or 10h. 42m. 7s. G.M.T., and the angle at emersion is 288°.

GEOGRAPHICAL NOTES

AFTER the results of the last English Arctic Expedition, and the criticism with which it met on its unexpected return, one would have thought that Arctic exploration in the old lines had become a thing of the past, or at least that it would only be carried on in a very much modified form, after the method, say, proposed by Lieut. Weyprecht. On the contrary, the desire to reach the Pole has become, apparently, keener than ever. There is every reason to believe that Capt. Howgate's scheme, to found a colony of Polar knights at Lady Franklin Bay, will be approved of by the U.S. Congress. Mr. Gordon Bennett, whose zeal for the promotion of knowledge is happily not short-lived, is to send out the *Pandora* by the Spitzbergen route on a similar quest, and it is rumoured that Mr. Stanley is to lead the forlorn hope. Prof. Nordenskjöld takes up the task laid down as hopeless 300 years ago, of finding a north-east passage; and last, but really first in point of time, a little Dutch expedition left Ymuiden on the 6th inst. for a six months' Arctic cruise. Since the stirring times of William Barentz, the Dutch have had much to do to keep their heads above water, but now that they have got well out of difficulties, it is gratifying to find that they are turning their attention in a direction in which long ago they won much glory and did good service. The appropriate name of the little schooner of eighty tons, in which the expedition sailed is the *Willem Barentz*. The expedition is commanded by the Dutch naval officer, J. J. de Bruyne, with Lieutenants L. R. Koolemans Beynen and H. M. Speelman, second and third in command, accompanied by a small corps of scientific experts and a crew of eight men, fourteen all told. The first point which the expedition purposes visiting will be Jan Mayen Land, next steering its course for the north-west coast of Spitzbergen to Amsterdam Island, examining the edge of the west ice *en route*. Smeerenburg, in this island, was the principal seat of operations of the Dutch Northern Company, and some days will be spent here in looking out and marking the graves of Dutchmen, several of whom died on the island when in the service of the Company in the winters of 1633-34 and 1634-35. It is expected that about July 15 the expedition will proceed to Novaya Zemlya, probably calling at Bear Island, and, after restoring the Dutch landmarks at these various points, will attempt to penetrate as far as possible to the north-west from the coast of Novaya Zemlya at the latest period of the navigable season, returning home before the winter. The expedition on which the *Willem Barentz* sails is purely national, having been organised exclusively by means of the voluntary contributions of the Dutch. The *Willem Barentz* has been specially constructed at Amsterdam for the service, and fitted with all the modern appliances of an Arctic ship. An English photographer, Mr. W. J. A. Grant, accompanies the expedition. Every opportunity possible will be availed of by the expedition to make observations in magnetism, meteorology, zoology, and natural history, together with deep-sea soundings, and the ascertaining of the direction and force of the currents in the Barentz Sea and surrounding waters. None of the four expeditions we have spoken of will be watched with more kindly interest than that in the *Willem Barentz*.

THE Malay Peninsula, from Wellesley Province to Singapore, contains, according to a communication addressed by an experienced tea-planter to a Straits paper, millions of acres of low, undulating, thickly-wooded hills, which are well suited for the growth of tea, as the soil of which they are composed is similar to the best tea soils of India. The variety to be planted must, however, it would seem, be that indigenous in Assam. The land referred to is, indeed, only suited to the cultivation of tea or coffee, and with cheap land, plentiful labour, regular seasons, and easy transport, the Malay Peninsula would certainly appear to possess unequalled advantages for the production of tea. The soil of Singapore has been, until recently, much underrated, but it has been shown conclusively that pepper, tapioca, and sugar can be successfully grown upon it, and it is probable that the tea-shrub, which is a hardy plant, can be grown on the island as well as on the peninsula.

OUR sources of information respecting Corea and its inhabitants are very limited in number, but now and again we glean some news thereof through the Japanese, and from a letter which the *North China Herald* annually receives from a correspondent at Newchwang in Southern Manchuria. By these means we learn that the porcelain of the country is very fine; palm-leaf fans are ornamented with paintings, in various colours, of human figures and landscapes; cotton-stuffs are made like that which comes from Mikawa in Japan, and the silk is like *pongee*, but is produced in small quantities; the only coin in use is not round, but consists of pieces of rod-iron, some four inches long, and bent into a curve. Game both large and small, abounds in the country; the hills are covered with pheasants; fallow and other deer are met with everywhere; bears are numerous, especially in the lofty mountains in the north; and spotted and striped tigers have proved themselves very dangerous of late years. Strange to say, notwithstanding the number of tigers in the mountains, the Corean houses have very primitive doors, a framework pasted over with paper being their only protection.

THE latest intelligence from Senegal notifies the arrival of Lieut. Semellé, head of the French expedition for crossing Africa eastwards. M. Paul Soleillet, already known as a Saharan explorer, has also gone to St. Louis for the purpose of leading an expedition by way of Timbuctoo, and In Çalah, to Algeria, the purpose being to divert as much of the Saharan trade as possible to Algeria. These, with the hopeful expedition under Abbé Debaize, which is to cross the continent from Zanzibar, show that the French are taking a fair share in African exploration.

SIGNOR GESSI and Signor Matteucci, the Italian explorers in Central Africa, encountering unexpected difficulties between Fadasi and Kaffa, have been obliged to return to Khartoum.

THE Geographical Society of Marseilles is preparing an interesting exhibit in the Paris Exhibition. The principal portion is devoted to African products and contains 216 specimens of the leading objects of commerce derived from this continent. They are gathered together in a section surmounted by the portraits of Cameron, Livingstone, and Stanley. A large ethnographical collection of weapons, utensils, etc., completes the exhibit.

PHYSICAL PROPERTIES OF METALS[1]

ONE of the most characteristic properties of metals is the power possessed by them when in more or less compact masses of acquiring (by polishing, pressure, or other mechanical treatment) such a condition of surface that light incident thereon is for the most part again reflected, whereby a peculiar glistening appearance is presented, known as the *metallic lustre*.

Owing to the influence of the air, moisture, vapours arising from putrefaction, &c., metallic surfaces, even when highly polished and brilliant, become more or less rapidly tarnished, so that the power of reflecting light is to a considerable extent lost. Before the invention of glass polished metallic surfaces were employed as *mirrors;* and for reflecting telescopes such surfaces are still in use. Now, however, it is usual to employ as mirrors glass surfaces behind which a thin coating of some lustrous metallic mass is placed, so that the smooth surface of the glass at once determines the peculiar reflective power of the metal applied to it, and preserves the metal from mechanical injury and from the corrosion of the air. For this reason these household appliances are ordinarily termed "looking-glasses," although strictly speaking it is not the glass that is the essential part.

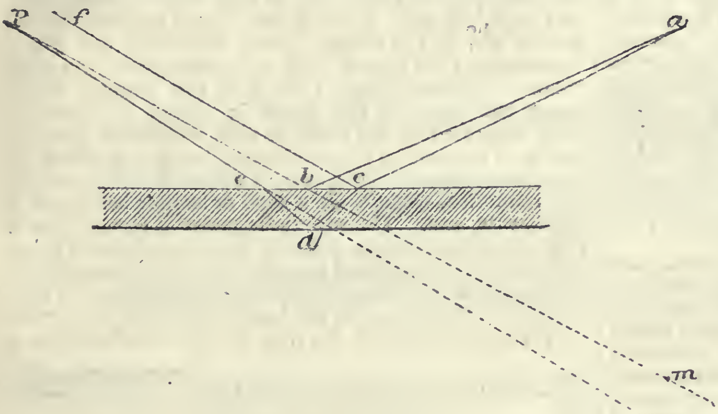


FIG. 1.

FIG. 2.

Three principal methods of applying these metallic substances to glass are in use; the best plate-glass mirrors (perfectly plain surfaces) are prepared by spreading out on a table surrounded with a deep groove or gutter, and capable of being raised on hinges so as to be placed at any angle with the horizon, a sheet of tinfoil, and smoothing it with a soft brush; mercury is then poured on and gently rubbed over the tinfoil with a hare's foot or a roll of flannel so as to penetrate and brighten the tin; more mercury is then poured on, and the surface cleansed from dross, &c.; finally, the perfectly clean sheet of glass is dexterously slid over the brilliant mercurial surface in such a way as to avoid inclosing any particles of dust or air-bubbles between the metal and glass. The table is then slightly raised at one end, so that the surplus mercury may gradually run off and be caught in the gutter; and the slope is increased daily, a piece of flannel being placed on the glass with weights on it to facilitate the draining off of the mercury. After two to four weeks, according to the size of the plate, the mirror is complete, the tin amalgam having then completely set, and being tolerably firmly adherent to the glass, although easily rubbed off and scratched on account of its slight tenacity. To preserve the back of the mirror from injury a suitable wooden frame is provided, in which the whole is fixed, when a finished mirror is the result.

For curved surfaces, such as the insides of globes, flasks, &c., for ornamental purposes, a somewhat different plan is employed: a fluid or semi-fluid amalgam capable of adhering to glass is poured into the vessel to be "silvered," and shaken about therein until the inner surface is covered with a film of the composition; the surplus amalgam is then poured out and used for other similar objects. A mixture of one part each of lead, tin, and bismuth, with two parts of mercury, answers well, the mixture being made perfectly fluid by slightly warming it before pouring into the vessel to be silvered.

A method which has of late years come largely into use for silvering mirrors of various kinds, and notably the reflectors of telescopes and lighthouses, is based on the power of certain chemical reagents to throw down silver in the metallic state from certain of its solutions, &c., the reduced silver in many cases adhering firmly to the surface of the vessel in which the action takes place, or to objects immersed in the liquid. Thus, if calcium tartrate in a moist state be placed in a glass vessel with a crystal of silver nitrate and a drop of ammonia solution, and the mixture cautiously heated, and made to flow successively over the whole inner surface of the glass, a fine mirror may be developed. Aldehyde, oil of cloves, and other essential oils, grape-sugar, and some other organic substances, may also be employed as reducing agents, especially the first substance.

If a "mirror" (*i.e.*, a glass surface with a brilliant metallic film behind) be carefully examined, it will be found that in most positions it will give a double image of any object reflected, one image being usually more brilliant than the other. Fig. 1 illustrates how this is brought about; a ray of light from an object at *a*, strikes the glass surface at *b*, and is reflected to the eye of the observer at P, so that an image is seen situated at *m*. Another ray of light incident on the glass at a point *c*, is partly reflected along *cf*, this portion of the ray consequently never reaching the eye at P at all; the rest of the ray enters the glass, being refracted along *cd;* at the junction of the glass and metallic surfaces reflection takes place along *de*, and at *e* the ray is refracted along *e* P, thus also reaching the eye of the observer, but

[1] From a forthcoming volume of the NATURE Series—"Metals and their Chief Industrial Applications. Being, with some Considerable Additions, the Substance of a Course of Lectures Delivered at the Royal Institution of Great Britain in 1877." By Charles R. Alder Wright, D.Sc., &c., Lecturer on Chemistry in St. Mary's Hospital Medical School. (London: Macmillan and Co., 1878.)

necessarily causing the image formed to be seen apparently situated at n, a point different from m. The relative quantities of light passing along e P and b P (that is, the relative brightnesses of the two images) depend on the degree of obliquity of the incident light c; the greater the angle $a\,b$ P (*i.e.*, the more obliquely the light falls on the mirror), the brighter is the image at n. The power of glass thus to reflect light to a considerable extent without any metallic film behind is utilised in the illusion known popularly as "Pepper's ghost," which consists simply of a large pane of glass sloping forwards from the stage at an angle of about 45° (Fig. 2). Objects such as A B, placed between the footlights E, and the pane of glass F in a horizontal position, and strongly illuminated, will produce to a spectator in front at P, a virtual image or "ghost," apparently situated at C D, the illusion being heightened by hiding, by means of screens, all the apparatus in front of the pane from the audience, and darkening that part of the stage behind the pane, the real objects furnishing the ghosts being placed on a dead black ground. When the lights E are extinguished, and other lights illuminating the stage behind the pane turned on, the ghosts disappear, whilst the real actors at D C on the stage behind the pane become visible *through* the transparent glass.

Most of the metals used in the arts in the free state are of considerable density, aluminium being by far the lightest, a circumstance which, together with its considerable strength and power of resisting the tarnishing effects of the air, renders it peculiarly suitable for numerous purposes: the draw-tubes of telescopes, opera-glasses, &c., and the graduated circles of surveying-instruments, &c., are often made of this metal for these reasons. According to the way in which a piece of metal has been obtained, its density will vary somewhat, being increased by hammering or any mechanical action which forces the particles together, *e.g.*, wire-drawing or sheet-rolling. The following table gives the numerical values of the average densities of most of the more important metals :—

Specific Gravity of Metals (*Water* = 1).

Platinum	21·5	Iron	7·8
Gold	19·3	Tin	7·3
Mercury	13·6	Zinc	7·1
Palladium	11·8	Antimony	6·7
Lead	11·3	Arsenic	5·6
Silver	10·6	Aluminium	2·6
Bismuth	9·8	Magnesium	1·8
Copper	8·9	Sodium	0·97
Nickel	8·8	Potassium	0·86
Cadmium	8·7	Lithium	0·59

Although the property of being drawn into wire is closely allied to that of being rolled or hammered into foil and leaves, yet the two are not necessarily possessed to equal extents by the same metal; gold, silver, and platinum are pre-eminently "ductile," whilst copper and iron are but little inferior to them in this respect. Aluminium and zinc can be obtained in tolerably thin wire, whilst lead and tin have so little cohesion that they cannot be drawn beyond a very limited degree of fineness. On the small scale, wires are readily obtained by casting the metals into thin pencils,[1] slightly pointing the ends of these and passing them into a funnel-shaped hole in a steel plate (*draw-plate*) of suitable size, gripping with pliers the protruding pointed part, and forcibly pulling the whole bar through the hole, the process being then repeated with a slightly smaller hole.

In drawing wire on a manufacturing scale, the process is just the same in principle, only, instead of drawing the wire through the draw-plate by hand by means of a wheel and axle, &c., the wire is pulled through by hand with pliers for a foot or two, and this portion then fastened to a revolving drum which then pulls the rest of the wire through, coiling up the drawn-out portion on the drum; the wire is then passed through the next smaller hole, being uncoiled from the first drum, and coiled again on a second in so doing, and so on until drawn to the required degree of fineness. In this way great lengths of wire are drawn at one operation.

By forming metals into wires of equal dimensions, and then determining the weight requisite to break these wires, the differences in tenacity exhibited by metals and alloys may be readily demonstrated. A convenient apparatus for this purpose is made of an iron tripod six or seven feet high, the legs of which are stayed together at the bottom and in the middle; from the top of the tripod is suspended by a stout hook a dynamometer or spring balance furnished with a hook at the bottom, whilst about half way up the tripod is affixed a horizontal axle, supported by the stays in such a position that the centre of the axle is perpendicularly beneath the hook of the dynamometer. This axle is provided with a winch, and round it is coiled a stout rope or leather band with a hook at the end. The wire to be tested is formed into a ring about three or four inches in diameter, the ends being intertwisted and soldered together; the hooks attached to the bottom of the dynamometer and to the rope are then inserted in this ring, and the handle turned so as to wind up the rope and stretch the ring until its form becomes a

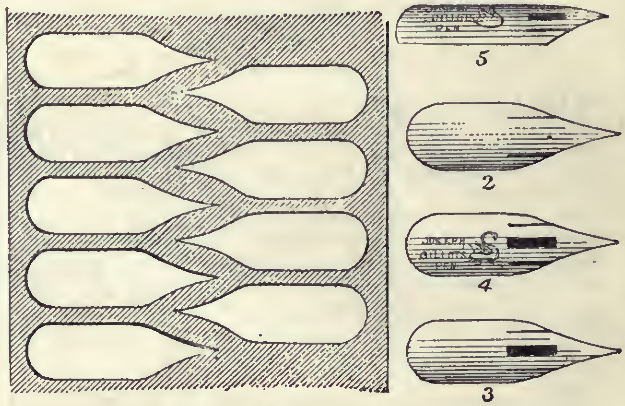


FIG. 3.

narrow oblong. The tension is then increased by winding the rope until the wire breaks; the reading of the dynamometer is noted by an assistant at the moment of rupture.

In this kind of way the order of tenacity of the metals is found to be as follows:

25 Iron.	8 Gold.
16 Copper.	7 Zinc.
14 Platinum.	1·5 Tin.
12 Aluminium.	1 Lead.
10 Silver.	

Closely connected with the physical structure which enables metals to exhibit the phenomena of crystallisation, malleability, and ductility, is the power which some possess of returning to their original shape when deflected therefrom by some external force not too great (*elasticity*); a property possessed to an extreme degree by good steel. The operations of wire-drawing, rolling, hammering, and the like generally increase the elasticity of metals, whilst annealing and fusing usually diminish it. Some metals are almost wholly devoid of elasticity; thus lead scarcely exhibits a trace of this property, being so soft that it is readily abraded by the nail. Some metals and alloys, when worked into appropriate shapes and struck, continue vibrating for some time, and hence are powerfully

[1] For metals of moderately-low melting-points the fused substance may be drawn up into a hot thin glass tube or pipe-stem by suction, and allowed to solidify therein. By fusing the metal in the bowl of a tobacco-pipe and tilting this so that the stem is inclined downwards, the molten metal can often be made to form a rough wire or thin rod in the stem readily obtainable by breaking away the pipeclay after cooling.

sonorous (*e.g.*, aluminium, bell metal, steel, standard gold, &c.).

The chief value of many metals and alloys for industrial purposes lies in their possession to a greater or less

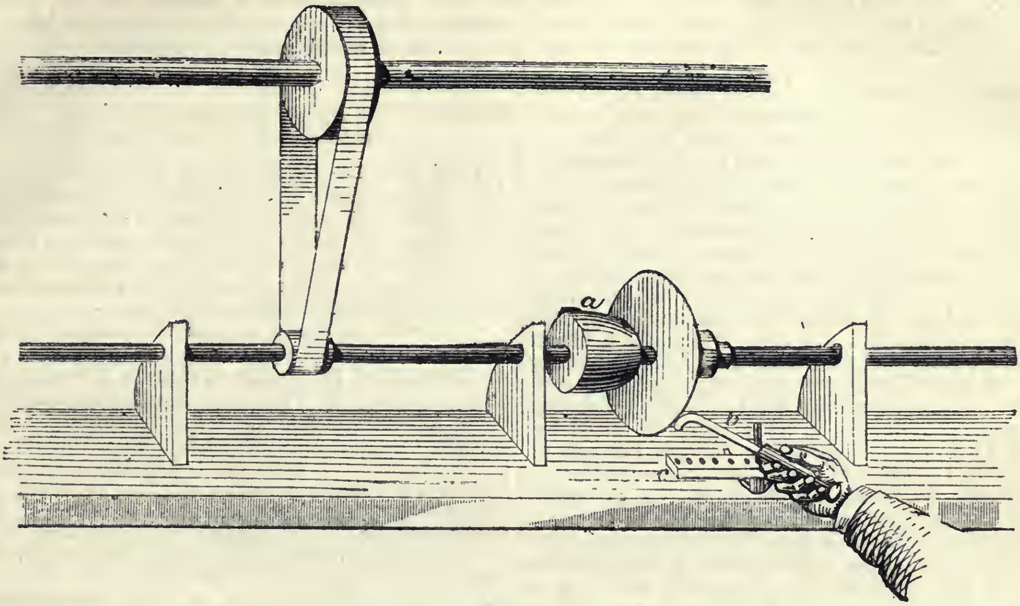


FIG. 4.

extent of a combination of properties of somewhat

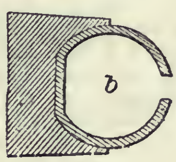


FIG. 5.

opposite kinds; whilst they possess sufficient rigidity to keep their shape even with moderately hard usage and to bear "wear and tear," when once fashioned into articles of domestic and everyday use, they have the power of yielding to pressure, &c., to a sufficient extent to enable them to be readily worked into these forms. In some cases the requisite softness for this latter purpose is hardly attained until the temperature is considerably raised; thus most articles of wrought iron are made when the metal is softened by heat so as to yield readily to percussion (*forging*) and other shaping processes. Closely connected with this softening or incipient conversion into a pliable mass by heat, is the phenomenon of *welding*, or

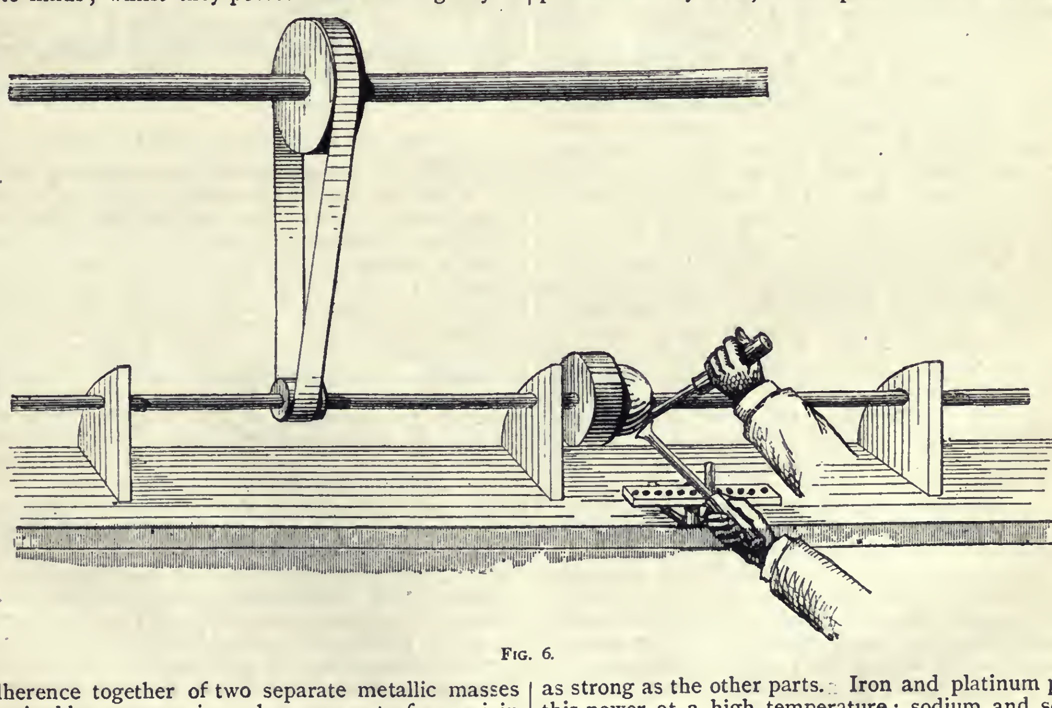


FIG. 6.

the adherence together of two separate metallic masses when united by pressure in such a way as to form a join as strong as the other parts. Iron and platinum possess this power at a high temperature; sodium and some of

the rarer metals at the ordinary temperature; gold also can be welded cold, under certain conditions, as in gold-beating. On the possession of these properties depend most of the metal-fashioning crafts, those where the metals are fused and cast being the main exceptions.

Thus in the manufacture of steel pens, as carried out by Messrs. Gillott and Sons, there are no less than eighteen stages between the conditions of bar steel and finished pen; and most of the stages are different applications of these properties of metals in reference to the shaping of the material into the required form. The bar steel is first converted into thin sheets, which are again rolled to the requisite degree of thinness; from the rolled steel "blanks" are punched out by a machine, leaving a kind of skeleton or network of "scrap steel" (Fig. 3), which is melted up or welded together and used over again. Two "side slits" are then made in the blank (No. 2), and a somewhat wider centre slit (No. 3) pierced, a portion of metal being punched out in making this orifice; the metal is then annealed and marked with the maker's name; a device or trade mark is raised by embossing (No. 4), and then the hitherto flat pen is converted into a portion of a cylinder, or curved (techically, "raised") by a suitable machine (No. 5); after which it is hardened, tempered, and cleaned by scouring with emery, &c.; the tip is then "straight-ground," *i.e.* the metal is thinned at the writing end by grinding in the direction of the length of the pen, after which it is "cross-ground," in the transverse direction. Finally the slit from the nib to the punched-out central part is cut, and the pen is coloured and varnished for sale.

Again, the manufacture of table-spoons and forks, many kinds of brass-work, cutlery, percussion-caps, copper pans and kettles, medals, and coins, and a thousand-and-one articles of every-day use, all depend upon the possibility of forcing the metal into various shapes without fracturing it, by mechanical processes, such as forging, punching, pressing, embossing, and the like. One of the prettiest illustrations of the application of pressing and shaping force is afforded by the processes in use for "teapot spinning," *i.e.* the production of a Britannia-metal teapot by a process technically termed *spinning*. The alloy being rolled into sheets of convenient thickness, a circular disc is cut out and placed in a kind of lathe as represented in Fig. 4, the metal disc being pressed against a nearly hemispherical wooden chuck *a*. The lathe being set in motion, the workman presses against the off-side of the disc with a peculiarly shaped tool, *b*, held steadily by means of the rest, *c*, so as gradually to bend the disc over the mould, *a*, and so to convert the disc into a bowl. The bowl thus formed is taken off the lathe and set with the convex part fixed into the concavity of a hollowed-out chuck (shown in section *a*, Fig. 5); by the aid of two differently shaped tools held one in each hand and applied, the one within and the other without the rim of the bowl, the metal is gradually bent inwards as it revolves, so as finally to take an almost globular shape: Fig. 6 indicates the closing stage of this operation, the nearly globular bowl thus formed being shown in section in Fig. 5 *b*. Finally the lid, spout, handle, &c., are attached, and the whole brightened and polished for the market. During the spinning the edge of the disc, some forty or fifty inches in circumference, becomes diminished to almost half that in the bowl, and to about one-quarter in the globular pot, the metal being thus as it were pressed in upon itself, as well as somewhat extended, the superficial area of the outside of the globular pot being somewhat greater than that of one side of the circular disc used in the first instance. In a similar fashion jugs and analagous vessels are "spun up," out of plates, the lips for pouring being subsequently shaped by carefully hammering or pressing out the metal on a wooden or metal mould. Silver articles, *e.g.*, bowls, teapots, &c., are frequently curved by an analogous operation; the second stage, however, cannot so well be applied to silver, so that if a closed-in vessel is required like a teapot, it is usually made in two halves, neatly soldered together.

SCIENTIFIC RESULTS OF D'ALBERTIS' LAST EXPEDITION TO NEW GUINEA

NOT long ago (NATURE, vol. xvii. p. 383) we gave a short narrative of M. D'Albertis' recent expedition to New Guinea. Through the kindness of Dr. George Bennett, of Sydney, New South Wales (at present in London) we are now able to add the following extracts from a letter just received from that distinguished explorer respecting the scientific results of his expedition:—

"I forward to you a copy of the account of my last voyage to New Guinea. I have not given any account of the results of the voyage as regards the collections of natural history, but I now inform you that the collection made is certainly less than I anticipated. Still, considering the great difficulties I had to encounter I ought to be satisfied. I have eight hundred skins of birds, including about two hundred species, of which I hope that twenty or twenty-five will prove to be new to science. Others will be interesting from the localities in which they were to be found and also from their rarity. I procured another specimen of the *Harpyopsis novæ guineæ*, the fourth obtained by me in New Guinea, and it is certainly remarkable that it has never been obtained by any other traveller in New Guinea. I also found the rare ground-pigeon, *Gymnophaps albertisi*, which I had previously obtained at Dorey in 1872, but it is so rare there that only one or two specimens were found by Beccari and Bruijn, and I have likewise two or three new parrots. Among the insects there are many very beautiful, and no doubt many of them will be new. The examination of my collections will be interesting to naturalists as showing the capricious distribution of animal life; for among my beetles from Papua, there are some found in Australia, and others indigenous to the Philippine Islands. I may also mention that I found a fine Buprestis (*Stigmodera duboulayi*), which is very rare, and known only in Western Australia. I may also have in my collection one or two new mammals, but this will be decided when I bring my collections to Europe."

THE REV. ROBERT MAIN, F.R.S.

PRACTICAL astronomy in this country has sustained a serious loss in the death of the Rev. Robert Main, which took place at the Radcliffe Observatory, Oxford, on the morning of May 7. Mr. Main entered at Queen's College, Cambridge, and graduated as sixth wrangler in 1834, and was Fellow of his college 1836-38, taking clerical orders in 1836. On the appointment of the present Astronomer-Royal he was selected to fill the office of First Assistant in the Royal Observatory, Greenwich, which position he retained, until, on the death of Mr. Johnson, he was appointed, in June, 1860, to the direction of the Radcliffe Observatory. During his connection with the Royal Observatory he was a frequent contributor to the *Memoirs* of the Royal Astronomical Society, his first paper "On the Node and Inclination of the Orbit of Venus" having been presented in June, 1837. This was followed by memoirs "On the Correction of the Mean Distance, Eccentricity, Epoch, and Longitude of the Aphelion of the Orbit of Venus," and he returned to the same subject in two subsequent communications read April 13 and December 14, 1838. In May, 1840, Mr. Main contributed a paper on "The Present State of our Knowledge of the Parallax of the Fixed Stars," which was of much value at the

time, as presenting a condensed criticism of all that had been effected in this direction; this paper was originally read to the council, in support of Bessel's claim to the gold medal for "his assumed discovery of the parallax of the remarkable star 61 Cygni." In 1845 Mr. Main procured the reduction of the numerous sextant observations of the great comet of 1843, the results of which were presented in a memoir read in January, 1846; but they did not justify, in point of precision, the time and trouble which had been expended upon them. In a paper read March, 1849, Mr. Main gave his deductions on the ellipticity and form of the planet Saturn, from measures at the Royal Observatory, showing that there is not, as was suspected by Sir William Herschel, any sensible deviation from a perfect ellipse. In April, 1856, he made a communication on "the values of the diameters of the planets having measurable discs," embodying observations with a double-image micrometer, extending from 1840 to 1852. His subsequent contributions to the same memoirs are (1) "On the Value of the Constant of Refraction" (1857), (2) "On the Proper Motions of the Stars of the Greenwich Catalogue of 1576 Stars for 1850" (1858), (3) "On the Value of the Constant of Aberration" (1860). Mr. Main successively filled the offices of Secretary and President of the Royal Astronomical Society.

As Radcliffe Observer Mr. Main has most conspicuously maintained for the Oxford establishment the high reputation with which it was left by his energetic and respected predecessor, Mr. Johnson. The successive volumes of observations have appeared with marked regularity, Mr. Main himself taking a much more active part in the routine computations than is usual for the director of an observatory, with the view of insuring with his comparatively small force this desirable result. Of the great value attaching to the Radcliffe observations it is unnecessary to speak here. We will only express the hope that the future conduct of the Institution may render as valuable services to practical astronomy as in the hands of Johnson and Main it has done in the past.

THE NATIONAL WATER SUPPLY

NEXT to a Conference of European powers deliberating on the fate of nations, it is difficult to imagine a Congress which may possibly more largely affect the welfare, health, and life of the people, than that to be held at the Society of Arts next week, on the subject of Water Supply.

The Congress is called at the instigation of His Royal Highness the Prince of Wales, to consider his proposition "how far the great natural resources of the kingdom might by some large and comprehensive scheme of a national character, adapted to the varying specialties and wants of districts, be turned to account not merely of a few large centres of population but for the advantage of the nation at large."

To inquire into these resources has been one of the objects of various Royal Commissions, which, though conducted with great ability, a lavish expenditure of time, and successful in collecting a large amount of very valuable information, have all failed to recommend how these bountiful stores of water can be made available. They found in the words of the Rivers' Pollution Commissioners "that an inquiry into the water supply of provincial towns must be one of great magnitude, involving a large amount of statistical and topographical investigation over the whole kingdom."

The Duke of Richmond's Commission, 1868-9, found it impossible, without further powers to carry out the inquiry, but they express their decided opinion "that the Legislature should jealously watch any proposal for a town taking water from a gathering ground at a distance from it, lest by so doing it may deprive other places nearer to such gathering ground of their natural source of supply;" and further, "that when any town or district is supplied by a line of conduit from a distance provision ought to be made for the supply of all places along such a line." This last suggestion has been adopted by the Select Committee of the House of Commons, lately ordered to "report upon the present sufficiency of the water supply of Manchester and its neighbourhood, and of any other source available for such supply," who recommend that the towns along the route of the proposed aqueduct from Thirlmere to Manchester be allowed a supply, after Manchester has been provided with twenty-five gallons per head. It is a source of regret that this Committee did not avail themselves of their full powers to inquire into all the means existing of supplying Manchester, as even should the Thirlmere scheme have proved the best, the information gained might have been useful to other districts. For though it is advisable that the inhabitants of Manchester, being accustomed to soft water, should continue to receive it, hard would probably be found equally wholesome, if pure, to those populations they propose to supply that are not at present using soft water.

Three points will probably be uppermost at the Congress—(1) Evidence to show the stores of water available; (2) How far the existing water legislation requires amendment, so as to give cheaper and quicker water powers to sanitary authorities than at present; and (3) How far it is advisable to have a National Water Supply Survey of the whole kingdom in connection with the Department of Health, technically known as the Local Government Board.

Looking to the fact that the labours of the various Royal Commissions and Select Committees have failed to recommend a scheme of provincial water supply, it is perhaps too much to hope that the present Congress will succeed; but when it is remembered that these failures prove the absolute necessity of personal examination of each district, and finding for each a scheme suited to its special requirements, we may look forward to this experience being utilised, and a scheme elaborated in which a scientific inspection of the country will play a principal part.

At this juncture it may not be without interest to glance at the present sources of supply of some of our great centres of population, and the means that are being taken to increase it.

The older palæozoic rocks forming the elevated tracts of the English Lake District, the Scottish and Welsh Hills, and Dartmoor, all of which are practically impermeable to water, lie west of a line ranging through the mouth of the Exe on the south coast, to the mouth of the Tyne on the north-east coast, and it is west of this line that the largest rainfall is received, ranging from 40 to 150 inches per annum. The rapid slopes and impermeable nature of the ground on which it falls cause it to be nearly all carried off in floods often of the most destructive character, and exceeding the dry weather flow 500 and even 1,000 times; the floods in the Silurian mountain districts amounting, according to Mr. Bateman, to a volume of 200 to 500 cubic feet of water per second derived from each 1,000 acres of area, while in dry weather the water given off by peat-mosses, and the unimportant springs found in such districts, will only amount to from one-fourth to three-fourths of a cubic foot per second.

The enormous volumes of pure water which run off the Westmoreland and Cumberland Mountains are stored to a certain extent in the numerous lakes which traverse that district, occupying true rock basins and often attaining a greater depth than the English Channel between Folkestone and Boulogne. Engineers have from time to time proposed them as sources of water supply for the crowded population of Lancashire, and even for the metropolis, and at length the House of Commons has assented to Mr.

Bateman's project of taking Thirlmere for the additional requirements of Manchester and the towns on the line of aqueduct. The rainfall is stated to be 98 inches per annum, which it is calculated will afford a supply of 64 million gallons a day, of which more than 13½ will be given back to the streams as compensation. The route to Manchester is 102 miles in length, the Lune will be crossed five miles above Lancaster, the Ribble five miles above Preston by pipes capable of conveying in the first instance, 10 million gallons a day; but the covered aqueduct from Thirlmere to the service reservoir at Bolton, will be large enough to convey 50 million gallons per day. The estimated cost of the first instalment of water will be 1¾ millions.

At present Manchester is supplied by a series of reservoirs collecting the rainfall of 18,000 acres of lower carboniferous rocks, consisting of permeable sandstones and impermeable beds of shale, which latter support the water absorbed by the pervious overlying strata. This water is not conveyed away down the dip planes of the strata, into other water-sheds, but is returned to the surface of the basin in which it falls; so that out of 45¾ inches of rainfall no less than 33 have been collected in the reservoirs. The supply from these Longdendale reservoirs will very nearly reach 25 million gallons daily, or 25 gallons per head of the existing population of Manchester and district, without any additional supply from Thirlmere.

In the Liverpool Gravitation Waterworks area, in the Rivington district, the geological conditions are similar; the drainage area is 10,000 acres, the average rainfall is 46 inches, and the mean from 1861 to 1865, was 44 inches, of which 33½ were collected, leaving 10½ inches for evaporation and absorption. Part of the Liverpool supply is derived from wells in the New Red Sandstone, the Green Lane Well having yielded no less than 3¼ million gallons per day.

The New Red series occupies an area in England of 7,431 square miles, absorbing on an average 10 inches of rain annually, or 400,000 gallons per day, for each square mile of surface, and affords over nearly the whole of the district an abundant supply of water which is described by the Rivers' Pollution Commissioners as "almost invariably clear, sparkling, and palatable, and amongst the best and most wholesome waters for domestic supply in Great Britain."

The great importance of the New Red Sandstone as a source of pure water makes its southern and eastern range under the newer formations a matter of some interest. At Scarle, near Lincoln, in a boring that attained a depth of 2,030 feet and reached carboniferous strata, it was passed through and was 786 feet thick, affording a very plentiful spring of water, which rose six feet above the surface.

Southwards the New Red series rapidly thins, but the thin end of the wedge is present at Burford, west of Oxford, where true coal measures have been reached beneath them; and still further south, at Crossness, the new boring of the Metropolitan Board of Works, after penetrating the Gault with its phosphatic basement bed, with the characteristic *Ammonites interruptus*, traversed certain red beds which are possibly referable to the Trias, and which would probably, if bored through, be found to be not less than 178 feet thick.

The Commissioners comment very favourably on the character of the waters derived from the Oolites, the volume of which is immense, the "Seven Wells," forming the head waters of the Churn, yielding two million gallons daily; the Syreford spring four millions, issuing at the base of the Inferior Oolite; other powerful springs in the Fuller's earth yield twelve and even twenty million gallons per day, which might be made available for the supply of Oxford, and other towns on the Thames, as Prof. Prestwich has ably pointed out in his "Oxford Water Supply." The Thames, above this city at Wolvercot, has an average daily summer flow of seventy-three million gallons, increasing to 742 million gallons in winter floods, which proves the quickness with which the Oolitic waters are given off, the amount absorbed by the rock being probably nearly all returned to the Thames basin, its onward progress down the dip planes of the strata towards London being stopped by lines of fault. Three million gallons of water are pumped from these oolites by the Thames and Severn Canal Company, and most of this water finds its way into the watershed of the latter river.

The Lower Greensand and Upper Greensand both yield large supplies of pure water to a large district in the south-east of England; they were proved in the deep boring at Meux's brewery, to overlie the true Devonian,[1] which rises there and form the old palæozoic ridge under the London basin. At the new boring at Crossness the lower greensand was absent, but the gault is so constantly unconformable to the lower cretaceous strata, and rests in the south-west of England on various members of the oolitic and liassic strata, that its occurrence on the red rocks of probably triassic age at Crossness is not specially remarkable, but would merely indicate more extensive denudation of the older secondary rocks there, than at Meux's brewery. This, should Mr. Godwin Austen's view of the possible continuation in that area of the Belgium and South Wales coal-field be correct, would have the effect of shortening the vertical distance to the coal-measures under the Thames basin. Be this as it may, both from the scientific and economic questions involved, it is a matter of considerable importance that the Crossness boring should be continued, and the nature of the underlying rock cleared up.

In the metropolis, Col. Bolton reports that in the month of June, 1877, the average daily quantity of water supplied by eight companies was 132½ million gallons to 3,796,000 people, living in over half a million houses, or a little less than thirty-five gallons per head during that summer month. The average daily quantity required is 125 million gallons, according to Mr. Bramwell, who proposes that London should receive an additional supply derived from wells drawing their supplies from the deep springs of the chalk, which he calculates would easily yield sixteen to thirty million gallons per day, which he would store in four reservoirs, north and south of London, at an elevation of 400 feet above ordnance datum, from which he proposes to give a separate supply for potable and fire-extinguishing purposes, at an estimated cost of 5½ millions.

Prof. Prestwich has pointed out that the first settlement of the London area was on the water-bearing gravel beds, the suburbs extending in the direction of these gravels, and that all extension stopped short at the outcrop of the London clay; but so soon as water companies introduced a supply of water derived from other areas, the northern side of London was built over. In Lancashire the middle glacial sands and gravels have taken the place of the gravels of the London area, and every exposure of sand rising through the boulder clay, marks the site of an ancient town or village, as Preston, Lancaster, Kirkham, Euxton, Leyland, Wigan, and Chorley, and numerous others.

London received its first systematic water supply in 1581 direct from the Thames, pumped by a water wheel placed in one of the arches of London Bridge by Peter Morrys, an ingenious Dutchman. This continued to afford a supply for two hundred years, and with the New River, brought by Sir Hugh Myddelton in 1613, from the chalk springs of Herts, satisfied the requirements of the metropolis up to 1723, when the Chelsea Water Works were established, followed by the Lambeth in 1785, the West Middlesex in 1806, and the Grand Junction in 1820, all taking their supplies from the Thames. Iron

[1] Determined from the fossils by Mr. Etheridge, F.R.S.

street pipes in lieu of wooden ones were introduced at the end of the last century, and the plan of filtration in 1829 by the Chelsea Company.

The tidal portion of the Thames at Hammersmith and Kew afforded a supply only thirty years ago, until the companies were at length compelled to take their supplies from above Teddington Weir, so as to be out of tidal influence. Five London Water Companies have the power to draw about 110,000,000 gallons per day from the river; its minimum flow during the minimum month of dry years is 350,000,000 gallons per diem. Mr. Beardmore estimates the amount of rainfall run off the Thames basin above Kingston from 1850 to 1868, to give a mean annual rate of 7·83 inches, while the mean rainfall at Oxford was 26·08, the rest of the rainfall being evaporated, or absorbed by vegetation. The head waters of the Thames are maintained by springs of the oolites, but these lose their volume after drought, the dry-weather flow of the river being maintained by the deep-seated springs of the chalk, which occupies an area in the Thames Basin, above Kingston, of 1,047 square miles, and has, according to Mr. Beardmore, a storing capacity of sixteen months.

Large numbers of wells have been sunk in the metropolitan area during the present century, into the Thanet Sands, underlying the London Clay; as these by pumping became exhausted, the chalk was penetrated, and the rainfall which is absorbed in the Hertfordshire and Surrey Hills, and flows down the dip planes of the strata under the London clay of the Thames Basin, was pumped up by the brewers and other large consumers. The constant tax, however, on this supply has caused a steady and increasing depression in the level to which the water will rise, and has necessitated in many instances the lowering of the pumping machinery in the metropolis.

Eighteen million gallons[1] daily of water of the River Lea that would have naturally gravitated towards London is intercepted by the New River Company, who pump their chalk wells most, when the River Lea is driest, and thus draw upon the deeper springs, which would not in ordinary course have reached the surface in that area.

Following the example of some 100 or more provincial towns that have acquired the control of their own waterworks, the Metropolitan Board of Works have laid before Parliament a bill to acquire the rights of the whole of the London water companies, which, at twenty years' purchase, are valued at fifteen million pounds, an amount steadily increasing, and have coupled it with another bill, to give effect to Messrs. Bramwell and Easton's proposal to sink chalk wells for a separate supply for drinking purposes. Influenced probably by the enormous cost of the one scheme, and the inconvenience attendant on the laying of 2,600 miles of new pipes in the other, the proposals are not supported by the ratepayers, who appear to consider "living organisms" in the water now supplied to them a minor evil. The "purchase bill" is now, however, abandoned for this session, and the "well scheme" will probably rest for the present.

An extension of Mr. Bateman's project for bringing the Vyrnwy and other head waters of the Severn to the metropolis, has recently been suggested for the future requirements of Liverpool, sixty miles distant; the watersheds between the Mersey, Dee, and Severn basins are very low, so that little tunnelling would be required. The scheme is stated by Mr. Hugh Williams, who suggested it, to be capable of yielding, if required, no less than 193,000,000 gallons of water daily, after allowing for compensation, a quantity which would suffice for the wants of six millions of our population, and could not fail to have a salutary influence on the floods of the Severn.

CHARLES E. DE RANCE

[1] The New River also receives 3½ million gallons daily from the Chadwell Spring, and the water obtained from various wells in the chalk.

NOTES

SINCE our last number appeared American science has sustained a loss which will be universally deplored. Prof. Joseph Henry, the Director of the Smithsonian Institution, whose labours for the progress of science, all the world over, have been increasing, is no more. We shall take an early opportunity of referring to his long-continued labours for the furtherance of natural knowledge among men.

WE understand that Mr. E. Roberts, of the *Nautical Almanac* office, has been requested by the India office to construct for use in India a self-acting tide-calculating machine. It will be designed not only to predict the tides at open-coast stations, but also river and shallow-water tides. It will be a great improvement on the tide-calculating machine at South Kensington (now temporarily at the Paris Exhibition), inasmuch as the tides caused by the smaller lunar perturbations will be included. Each component will be fitted with a slide, so that no error will be caused from the excentricity of the pullies. The ordinates of the curves traced by the machine being as much as eighteen inches, the use of the slides is imperative. Mr. Roberts has calculated new numbers to represent the periods of the many components, and with such success, that the actual error of any one component, after a run representing a year's predictions, will not exceed the limit of error of setting the component at the commencement. The machine will be fitted with self-regulating driving-gear, so that it can be set at the close of the day and the whole year's curves be ready for reading off by the next morning. The machine is expected to be finished towards the end of the year. Now that the immense labour (the only objection raised against the employment of tidal predictions by harmonic analysis) is superseded, it is to be hoped that the Admiralty will avail themselves of an instrument, the results of which are so vastly superior to those now obtained with considerable labour by actual computation.

PROF. HUXLEY has been elected a corresponding Fellow of the Royal Academy of Rome, in the Department of Natural History.

WE learn that the following gentlemen, all highly distinguished for their numerous original researches and published memoirs on physiological and systematic botany, have recently been elected foreign members of the Linnean Society of London:—viz., Prof. Teodoro Caruel, of Pisa, Dr. Ernest Cosson, of Paris; Dr. George Engelman, of St. Louis, Missouri, U.S.; Prof. Eduard Fenzl, of Vienna; and Prof. Julius Sachs, of Würzburg.

ON April 29 a monument, in memory of the great physicist, Alessandro Volta, was unveiled at Pavia. Most of the Italian Universities, and several foreign scientific societies had sent deputies to Pavia University for this event. The monument is a masterpiece of the sculptor Tantardini of Milan. The ceremony of unveiling was followed by a dignified celebration at the University, and upon that occasion the following gentlemen were elected honorary doctors of the scientific faculty: Professors Clerk Maxwell (Cambridge) and Sir W. Thomson (Glasgow); M. Dumas (Paris), Dr. W. E. Weber (Leipzig); Professors Bunsen (Heidelberg) and Helmholtz (Berlin), Dr. F. H. Neumann (Koenigsberg), and Dr. P. Riess (Berlin).

THE death is announced of Roberto de Visiani, "the Nestor of Italian botanists," Professor of Botany at Padua, aged seventy-eight.

WE notice the death in Berlin on April 22 of the well-known astronomer Prof. Wolfers. For many years he was connected with the Berlin Observatory, and, as editor of the *Jahrbuch* issued from this institution, he has for the past forty years rendered services of the greatest value to astronomy. His re-

searches in meteorology have likewise made him known to a wide circle. The most important of these were his comparative statistics on the winter weather of Berlin, compiled from a long series of careful and minute observations. Prof. Wolfers was aged seventy-five at the time of his death.

M. FERDINAND HOEFFER died at Sannois, a small country-place in Seine-et-Oise, on May 8, at the age of sixty-eight years. He was the editor of the *Biographie générale*, published by Firmin Didot in sixty 8vo volumes. Most of the scientific memoirs in that immense work were written by him, and he was the author of a large number of historical works on astronomy and chemistry.

MARIETTE BEY, the Egyptologist, who has rendered such valued services to archæology, has been elected a member of the French Institute.

THE statue of the philosopher, Giordano Bruno, will be dedicated at Rome on February 17, 1879. On the same day, in the year 1600, he was burned at the stake, in Rome, by the Inquisition.

THE festive celebration of the fiftieth anniversary of the foundation of the Royal Polytechnic Institution of Dresden took place on May 1, and was attended by many eminent men of science and other persons of note.

A PROMINENT feature at the late banquet of the Berlin Geographical Society was the speech of the representative from the Paris Society. His concluding wish that the festival might serve towards the scientific fraternisation of the two nations, was greeted with loud applause, which was redoubled as the Crown Prince of Germany rose to publicly shake hands with the French Ambassador.

ONLY last week we noticed Capt. Burton's narrative of his visit in the spring of last year to the Land of Midian. The results of this visit were so full of promise for the empty exchequer of the Khediv (as Capt. Burton scolds us into spelling the title), that the almost veteran explorer returned again with a more formidable expedition in December last, and spent four months examining the region, during which the expedition travelled and voyaged upwards of 2,500 miles. They brought home some twenty-five tons of geological specimens to illustrate the general geological formation of the land; six cases of Colorado and Negro ore; five cases of ethnological and anthropological collections—such as Midianite coins, inscriptions in Nabathean and Cufic, remains of worked stones, fragments of smelted metals, glass, and pottery; upwards of 200 sketches in oil and water colours, photographs of the chief ruins, including the catacombs, and of a classical temple, apparently of Greek art; and, finally, maps and plans of the whole country, including thirty-two ruined cities, some of whose names can be restored by consulting Strabo and Ptolemy, besides sketches of many *ateliers* where perambulating bands like the gipsies of ancient and modern times seem to have carried on simple mining operations. Among the specimens are argentiferous and cupriferous ores from Northern Midian, and auriferous rocks from Southern. There are collections from three turquoise mines, the northern, near Aynuneh, already worked; the southern, near Ziba, still scratched by the Arabs; and the central, until now unknown save to the Bedouins. There are, moreover, three great sulphur beds, the northern and the southern, belonging to the secondary formation (now invaded by the trap granite), and the central, near the port of Mowilah, of pyretic origin. Rock salt accompanies the brimstone, and there are two large natural salt lakes. The whole of the secondary formation supplies fine gypsum, and in parts of it are quarries of alabaster, which served to build the ruins of Maghair, Sheéayb, Madiama (of Ptolemy), and el-Haurá (Leuke Kome), the southernmost part of western Nabathea. Specimens of the ores will be sent to Paris and London; the rest will be analysed in Cairo by a local commission, while the curiosities of all kinds, after being exhibited in Cairo, are to be sent to the Paris Exhibition.

WHAT may be called excursional education is finding very great favour with the lively French, and threatens to be applied in a variety of directions. The largest private school in Paris, that of Sainte-Barbe, near the Panthéon, which was established in the fifteenth century, and is now the property of a company, has just inaugurated a system of tours for teaching foreign languages. Forty of the pupils have been sent to Carlsruhe, in Germany, where they will be boarded in a number of families for several months, and receive their regular instruction from a staff of German teachers. Next year there will be an English tour, very likely to London. The pupils have been carefully selected from amongst those who are most likely to benefit from this practical system of international education.

SOME important improvements upon Prof. Bell's telephone have recently been made and patented by Mr. E. Cox Walker, electrician (of the firm of Messrs. T. Cooke and Sons) of York. The improvements consist in doubling or quadrupling the diaphragm and its accessories, and dividing the mouthpiece so that to each diaphragm there is a corresponding sub-mouthpiece. Instead of using two or four single magnets, Mr. Walker adopts one or two magnets somewhat of a horse-shoe shape, the coils being connected as for ordinary horse-shoe electro-magnets, and the connections made with the transmitting wire in the ordinary way. Taking the quadruplex mouthpiece as an example it might be explained thus: the ordinary single telephone mouthpiece is elongated, and instead of the orifice leading direct to the diaphragm it is divided into four smaller channels, each of which collects and directs the sound on to the diaphragms covering the magnets. Mr Walker does not have a separate diaphragm for each pole or coil, but has so constructed the under side of the mouthpiece that it nips the diaphragms tightly across the middle and around the opening containing the coils, and virtually divides them. Mr. Walker has also recently made an octoplex instrument, which, by adding to the number of coils, diaphragms, and corresponding divisions in the mouthpiece gives eight times the intensity of a single one. Besides the mouthpieces, Mr. Walker has patented improved earpieces. The instrument was exhibited at the Royal Society's *soirée* on May 1, and at the Royal Institution on May 3.

THE Montsouris Observatory has established at the Champ de Mars a pavilion where all the observations will be conducted on the same principles as at the establishment, and the principal registering apparatus in use will be put in operation before the public. The Meteorological Society has also established another pavilion where the telegraphic warnings of the international service will be posted daily. A collection of all the meteorological journals published by the several offices will be exhibited every day for comparison. From May 1 the French service publishes daily two maps giving the state of the weather at 7 A.M. in summer time. The first of these maps gives isobaric lines with the variations in the last twenty-four hours expressed in tenths of millimetres. The second gives the isothermal lines with thermometric variation in the past twenty-four hours expressed in tenths of centigrade degrees. The isobaric map shows, by conventional signs, the state of the sky, force and direction of winds, and state of the sea. The isothermal map shows, by other signs, the extent of rains, their importance, and the limits of congelations. The Pic du Midi and Puy de Dome observatories will also send the results of their daily observations, which will be posted in a conspicuous part of the establishment.

LIEUT. G. R. R. SAVAGE, R.E., writing from Roorkee, North-West Provinces, India, sends us an account of some interesting experiments he has been making on long-distance telephones. He constructed telephones expressly for long-distance work, and succeeded in getting a bugle-call heard distinctly over 400 miles of Government telegraph line, the wire being one of the four or five main up-country telegraph wires which are carried on one set of posts. The telephones used, Lieut. Savage constructed with about 400 ohms of No. 38 gauge wire, vibrating disc about $2\frac{1}{2}$ inches diameter, the sending vibrating disc thicker a *little* than the receiving one. It seems to him right to oppose the work done at the receiving end as little as possible by having a very thin vibrating disc; while he had noticed that, *cæteris paribus*, a thicker disc approached to a telephone magnet gives a greater deflection on a distant very sensitive galvanometer, so long, of course, as it is not too thick. Lieut. Savage asks the reason for the following circumstance:—Taking off the vibrating disc of a telephone, and tapping the magnet with any diamagnetic substance, brass, glass, &c., the tapping sound is heard distinctly at a distant telephone. This cannot be caused in the same way as the current in Prof. Bell's telephone; it must be caused, he supposes, by the particles of magnet being caused to vibrate longitudinally, and as the coil does not vibrate in unison with the particles of the magnet, the permanent lines of magnetic force must be cut by the coil, and hence a current. Hence, he asks, if this is the case, might not there be two causes combined producing the effect in Prof. Bell's telephone, both approach of disc and also longitudinal vibrations? Lieut. Savage constructed a small induction coil with soft iron core, the outer and inner coil the same. He heard and sent messages easily seventy or eighty miles by joining the two coils separately in circuit with the sending and receiving telephone. Of course there was no *increase* in any way, as no energy was expended on the current by the simple induction coil; there was a slight decrease in the sound. He thinks about 350 ohms of No. 38 wire makes the best coil for a telephone magnet $\frac{1}{4}$-inch diameter.

MR. C. F. CREHORE, of Boston, U.S., sends us an amusing incident, *à propos* of the subject of fetichism in animals referred to by Mr. Romanes recently in NATURE. A brave, active, intelligent terrier, belonging to a lady friend, one day discovered a monkey belonging to an itinerant organ-grinder, seated upon a bank within the grounds, and at once made a dash for him. The monkey, who was attired in jacket and hat, awaited the onset with such undisturbed tranquillity that the dog halted within a few feet of him to reconnoitre. Both animals took a long steady stare at each other, but the dog evidently was recovering from his surprise, and about to make a spring for the intruder. At this critical juncture, the monkey, who had remained perfectly quiet hitherto, raised his paw and gracefully saluted by lifting his hat. The effect was magical; the dog's head and tail dropped, and he sneaked off and entered the house, refusing to leave it till he was satisfied that his polite but mysterious guest had departed. His whole demeanour showed plainly that he felt the monkey was something "uncanny," and not to be meddled with.

THE earthquake of April 26 in the vicinity of Constantinople was felt even more severely farther inland. At Nicomedia damage to the value of £300,000 was caused, and an entire village was destroyed with a loss of forty lives. Three smart shocks of earthquake were felt at 7 A.M. on the 14th inst., at Hennebout (Morbihan, France), the motion being from west to east.

THE Society of Telegraph Engineers have appointed a special meeting on Thursday, the 23rd inst., at the Institution of Civil Engineers, 25, Great George Street, Westminster, when a paper will be read by Mr. W. H. Preece, on the connection between Sound and Electricity, illustrated by Prof. Hughes' recent discoveries.

JANSEN, MCCLURG AND CO., of Chicago, will shortly publish a new and greatly enlarged and improved edition of Prof. Jordan's "Manual of the Vertebrates of the United States." The section on fishes, the *Nation* states, will be entirely rewritten.

A TELEGRAM from Adelaide, dated the 8th instant, intimates that copious rains have fallen in South Australia. As heavy rains have been previously reported from other parts of Australasia and Cape Colony, the great drought which has been so disastrous over the greater part of the Southern hemisphere may now be regarded as at an end.

THE Rev. James M. Crombie, F.L.S., author of "Lichenes Britannica," has been appointed to the Lectureship on Botany at St. Mary's Hospital, recently held by Dr. H. Trimen.

AMONG the scientific productions of Germany during the past month, we notice a new edition of Liebig's classic "Chemische Briefe," "Beiträge zur fossilen Flora Schwedens," by A. G. Nathorst (Stuttgart); "Astronomisch-geodätische Arbeiten in 1876," by the Prussian Geodetic Institute (Berlin); "Die Familien-diagramme der Rhocadinen," by F. Schmitz (Halle); "Geologie der Insel Luzon," by R. von Drasche (Vienna); "Systematisches Verzeichniss der Macro-lepidopteren von Nordamerika," by B. Gerhard (Berlin); "Das Klima Ostasiens" by H. Fritsche (Leipzig); "Die Käfer von Nassau und Frankfurt," by L. von Heyden (Wiesbaden); "Die Figur der Erde," by H. Bruns (Berlin); "Bericht über die 1 u. 2 Jahresversammlung der deutschen ornithologischen Gesellschaft," by F. Cabanis (Leipzig); the fifth edition of Prof. Lenz's "Die Reptilien, Amphibien, Fische und Wirbellosen Thiere;" "Der Mensch und das Thierreich," by Professors Krass and Landois, of Münster; "Was da kriecht und fliegt!" by Prof. Taschenberg; and "Europas Kriechthiere und Lurche," by Dr. Knauer, of Vienna.

SEVERAL correspondents write concerning a bright meteor seen on Sunday night. Mr. Whitley Williams saw it when in the open fields, about three-quarters of a mile west of Lillie Bridge. The meteor bore 1° or 2° to the west of north, and elevated approximately 20° above the horizon. It was very brilliant, even dazzling in appearance, its light having a bluish shade like that of burning magnesium. It descended from right to left at an apparent angle of 60° or more with the horizon, and described an arc of 5° or 10°, at the latter end of which it was hidden behind a cloud. The sky was overcast with a few very black clouds, and only one or two stars visible. The meteor gave the impression of being very near. A correspondent at Higher Walton, near Preston, May 14, saw it about 8.51 P.M. in a direction almost due north. It was travelling from east to west; altitude when first seen, about 60°, when last seen, about 15°. Its velocity was comparatively slow, being visible for five or six seconds. During the motion a tail, the length of which subtended an angle of about 3°, followed the nucleus. When near the end of its motion the tail disappeared, and the nucleus broke up into several pieces, and shortly disappeared. No sound was heard. "D. R. S." saw it from Kirknewton, Edinburghshire. The meteor seemed to rise from a point about 30° east of the moon at the time, and travelled in rather a leisurely manner through an arc of about 73°, in a direction of about north-west by north, leaving a trail of stars behind it, then burst into pieces and disappeared, without any report. It was of a fine violet colour. It seemed oblong in form and irregular in outline from giving off sparks so rapidly.

THE additions to the Zoological Society's Gardens during the past week include a Jaguar (*Felis onça*) from South America,

presented by Major Wood; a Two-spotted Paradoxures (*Nandinia binotata*) from West Africa, presented by Capt. E. J. Hawes; a Black-backed Jackal (*Canis mesomelas*) from South Africa, presented by Mr. Richard Seyd, F.Z.S.; a Toque Monkey (*Macacus pileatus*) from Ceylon, presented by Master R. C. Heyworth; a Bonelli's Eagle (*Nisaetus fasciatus*), European, presented by Lord Lilford, F.Z.S.; a Leadbeater's Cockatoo (*Cacatua leadbeateri*) from Australia, presented by Mrs. Tennent; a Goffin's Cockatoo (*Cacatua goffini*) from Queensland, presented by Mrs. Pitt; a White-backed Piping Crow (*Gymnorhina leuconota*) from Australia, presented by Mr. John Ritchie; three Water Ouzels (*Cinclus aquaticus*), European, presented by Mr. R. J. L. Price, F.Z.S.; a Slow Loris (*Nycticebus tardigradus*) from Malacca, received in exchange; a Darwin's Pucras Pheasant (*Pucrasia darwini*), a Wonga-wonga Pigeon (*Leucosarcia picata*), nine Chilian Pintails (*Dafila spinicauda*), bred, a Reindeer (*Ranifer arandus*), born, in the Gardens.

ACADEMIC LIBERTY IN GERMAN UNIVERSITIES[1]

THIS liberty without control is a subject of astonishment to most strangers who know it only by certain very apparent eccentricities. They cannot understand how we are able, without great inconveniences, to leave young people to themselves in this way. The German remembers the student-period as the golden age of his life; our literature and our poetry are filled with the expression of this sentiment. We do not encounter anything similar to it among other European peoples. The German student is the only one who tastes an unmingled joy at the time when, in the first delight of his young independence, yet free from the anxieties of mercenary work, he may consecrate his hours exclusively to all that is noblest and best in science and in the conceptions of humanity. United by a friendly rivalry with numerous comrades devoted to the same efforts, he finds himself daily in intellectual communication with masters from whom he learns what is the movement of thought among independent spirits. I appreciate at its full value this last advantage, when, looking back, I recall my student days and the impression made upon us by a man like Johannes Müller, the physiologist. When one finds himself in contact with a man of the first order, the entire scale of his intellectual conceptions is modified for life; contact with such a man is perhaps the most interesting thing which life may have to offer.

You possess, my young friends, in this liberty of German students, a precious and glorious legacy of past generations. Preserve it in order that you may leave it in your turn to those who will come after you, and strive to ennoble and purify it still more. To guard it intact you have, each for himself, to see that the studious youth of Germany continue worthy of the confidence which has secured for them so high a degree of liberty. There is here for feeble characters a gift as calamitous as it is precious for the strong. Do not be astonished that statesmen and fathers of families think sometimes of instituting among us a system of surveillance and control analogous to that which exists in England. There is no doubt that such a system would save many whom liberty allows to run to ruin. But the State and the nation have more to expect from those who are capable of supporting liberty and whose efforts and work are the results of their own individual energy, of their dominion over themselves, and their love of science.

I have spoken above of the influence which may be exercised by intellectual contact with remarkable men; this leads me to point out another characteristic feature which distinguishes German universities from those of England and France. With us the student goes, as soon as possible, to seek instruction from masters who have proved their merit by doing something for the progress of science, which, in our eyes, is the best mark of their fitness to educate. Yet this is a thing which excites great astonishment among the English and French. They attach more importance than the Germans to a pretended talent for instruction, which consists in the faculty of expounding the subject of instruction in a clear and well-ordered form, and, if possible, in an eloquent and interesting manner, calculated to captivate the attention. At the Collége de France, the Jardin des Plantes, as also at Oxford and Cambridge, the lectures of renowned speakers are the rendezvous of the fashionable and cultivated world. In Germany we are not only indifferent to the oratorical apparatus, we are hostile to it; we undoubtedly neglect too much the external form. There is no doubt that a good exposition demands from the listener much less sustained efforts than a bad one; it enables the subject to be comprehended much more surely and much more completely, and with a well-ordered arrangement, bringing into strong relief the principal points and the divisions, much more can be overtaken in the same space of time. I do not pretend, then, to justify the contempt of form, which we often push too far, both in speaking and in writing. But it cannot be denied that many men of great intellectual originality and of remarkable scientific value, have a dull, painful, and embarrassed elocution. Yet I have often seen such professors attract numerous and faithful hearers, while orators void of thought astonished at their first lecture, fatigued at the second, and were deserted at the third. He who wishes to inspire his audience with a complete conviction of the truth of what he advances, ought, above all, to know from personal experience what produces conviction. It is necessary then that he has known how to advance alone into a region where no one has ever broken ground; in other words he must have worked upon the frontiers of human science and conquered for himself new domains. A master who presents only results acquired by others suffices for scholars to whom authority is given as the source of their science, but not for those who desire to deepen their convictions to their final foundations.

There is here, you see, gentlemen, a new sign of confidence given you by the nation. There are neither fixed courses nor fixed professions imposed upon you. You are treated as men whose free adhesion must be gained, who know how to distinguish between being and seeming, whom it is no longer sought to persuade by appealing to any authority whatever, and who, moreover, would not allow themselves to be persuaded in that fashion. It is sought, more and more, to provide you with the means of drinking science at the very fountain, either in books and historical collections, or by observation of objects and natural pheomena and by experiments. The smallest German universities have their own libraries, their mineralogical collections, &c. As regards the organisation of laboratories of chemistry, micrography, physiology, physics, Germany is ahead of all other European countries, who are only just beginning to seek to rival us. In our own university, we shall assist in a few weeks at the opening of two important establishments devoted to education in the natural sciences.

To obtain the free conviction of the pupils, it is necessary that the conviction of the masters be freely expressed; *liberty of education* is demanded. That has not always been protected from all encroachment in Germany more than in neighbouring countries. In times of political and religious strife the dominant parties have often interfered in the domain of science; but the German nation has always regarded such interferences as encroachments upon sacred ground. Here, again, the progress of political liberty in the new German Empire has been salutary. To-day, in the German universities, the most extreme results of materialistic metaphysics, the most daring speculations in the direction of the Darwinian theory of evolution, may be published without hindrance, as well as the most complete deification of the infallible Pope. No more than on the floor of European parliaments is it permitted to calumniate the intentions and outrage the person of an opponent—these are proceedings which have nothing to do with the discussion of a scientific proposition. It is also forbidden to excite to the commission of acts interdicted by the laws; but we may, without the least hindrance, discuss *scientifically* any controverted *scientific* point whatever. Liberty of education, in this sense, does not exist in the English and French universities. At the Collége de France, even a man of so high a scientific reputation as Ernest Renan has been placed under interdict. The tutors of the English universities may not diverge by a hair's breadth from the dogmatic system of the Anglican Church without exposing themselves to the censure of the archbishops and losing their pupils.

It remains for me to consider our liberty of education in another aspect. I wish to speak of the liberality with which our universities award the authority of professor.

In the etymological sense of the term, a *doctor* is a man *who teaches*, or at least, a man recognised as capable of teaching. In the universities of the middle ages, every *doctor* who found

[1] Rectorial Address of Prof. Helmholtz, F.R.S., at the University of Berlin. Continued from p. 53.

pupils, might be constituted *master*. In the course of time the practical signification of this title has been altered. Most of those who obtain it do not propose to teach; this title is only useful to them as a public mark of their scientific instruction. It is only in Germany that there still subsist some vestiges of the rights formerly attached to the doctorate. It is true that in consequence of the change which has taken place in the signification of the title, and in consequence of the increasing specialisation of the various branches of education, we require of doctors, who wish to instruct, a most searching proof of their profound knowledge of the subjects which they desire to be authorised to profess. For the rest, in most of the German universities, the legal rights of these doctors authorised to instruct, are exactly the same as those of ordinary professors; in some there are certain restrictions which are of little practical importance. The oldest masters, especially the ordinary professors, have no other real advantage than that of having more completely at their disposal the material means of study furnished by the State, and necessary for instruction in certain departments of science; besides, they are legally intrusted with the conduct of the university examinations, and, in fact, the State examinations are oftenest intrusted to them, which naturally exercises a certain influence on the most timorous students. But the influence of examinations is much exaggerated. In consequence of the come-and-go movement of the students, candidates are often tested by examiners whose courses they have never attended.

Of all our university institutions, that of *privat-docent* is what most confounds strangers. They are astonished, not without envying us, at finding among us so many young men disposed to devote themselves to hard scientific work, for the most insignificant fees, without fixed salary and without any assured prospect for the future. And, always from the practical and material point of view, they wonder that the faculties admit so easily, and with such complaisance, these young people who may, at any moment, be transformed from assistants into competitors, and that in a situation so delicate, the employment of annoying methods of competition is so rare and exceptional a thing.

The right of filling vacant chairs, as well as that of giving authorisation to the *privat docenten* belong to the faculty, *i.e.*, to the assembly of the ordinary professors, although the former of these rights is not absolute, and the final appeal is not to the faculty. These assemblies are in the midst of the German universities a relic of the ancient colleges of doctors who inherited the privileges of the primitive corporations. They also represent the union of the graduates of old, but much reduced, and organised with the concurrence of the governments. The custom is that for the nomination of the ordinary professors the faculty presents three candidates for the choice of the Government. The latter, it is true, is not rigorously obliged to abide by the candidates presented, but it is extremely rare that the presentations of the faculty have not been respected except at epochs when party strifes were very hot. Unless for very potent considerations, it is always an extremely weighty responsibility for the representatives of the executive power to institute against the wish of a competent body, a professor who will have to give publicly the proofs of his merit before a numerous auditory.

The members of the faculty have the strongest motives for strengthening as much as possible the teaching body. To be able to give one's self with joy to the labours of the professoriate, the most essential condition is that of being assured that you will not have to speak before too small a number of intelligent auditors. Moreover, the income of many of the professors depends to a large extent on the number of students. Each professor ought, then, to desire that the faculty to which he belongs draws as many and as intelligent students as possible. This end can only be attained if all the masters—professors and *privat docenten*—are chosen from among the most distinguished men. On the other hand, the efforts made by a professor to accustom his students to work energetically and with intelligence, can only be successful if he is seconded by the other members of the faculty. Finally, the concourse of distinguished colleagues contributes to maintain, in university circles, a more interesting, more instructive, and more active life. To make these reasons yield to other considerations a faculty must already have fallen very low, have lost not only the feeling of its dignity, but also that of the commonest worldly prudence; such a faculty would soon come to ruin.

As to the phantom of a rivalry among the professors, with which it is sought sometimes to frighten public opinion, nothing of the kind can be produced when the teachers and the students are what they ought to be. In the first place, it is only in large universities that there exist two chairs for the same branch of science; and in this case, if there is no difference between the official definition of the two chairs, there is certainly one between the scientific tendencies of the two professors, and these may divide the work in such a way that each reserves the subjects in which he is most competent. Two distinguished professors, who are thus complementary to each other, are so powerful a centre of attraction to students that neither of the two need fear to see the number of his auditory diminish, when even a certain number of the less zealous may divide and follow only one of the two courses.

The injurious effects of rivalry are especially to be feared when one of the professors does not feel himself established in his scientific position. Even this may not exercise any influence upon the decisions of the faculty so long as the case concerns only one or a small number of the voters.

The exclusive domination of one scientific school may be more unfortunate for a faculty than the personal interests of which I have spoken. It may in fact be foreseen that when the school will have had its day, the students will gradually resort to other universities. Years may thus pass, and the faculty be paralysed for a long time.

It is easy to see, under the sway of this system, how many efforts the universities have made to attract to them all the scientific leaders of Germany; for this it is enough to inquire how many men of original genius are found outside the universities. We may have some idea of the result of such an inquiry, for we are rallied on the fact that all German science is a science of professors. If we consider England, we find immediately men like Humphrey Davy, Faraday, Darwin, Grote, who have no connection with the English universities. In Germany, on the contrary, except the *savants* whom the Government have excluded for religious or political motives, like David Strauss, and except those who, in the quality of members of German academies, have the right of giving lessons in the universities, like Alexander and Wilhelm von Humboldt, Leopold von Buch, &c., the number of those who are found outside the universities is very small relatively to the number of those who have been professors in them. If we make the same calculation for England, we arrive at an inverse proportion. I have always considered it a very striking thing that the Royal Institution of London, a private society wishing to have for its members and for a distinguished public short courses of lectures on the progress of the natural sciences, has been able permanently to attach to itself for this purpose men having so great a scientific authority as Faraday and Humphrey Davy. There is not here a question of salary; evidently these men were attracted by an auditory composed of men and women of independent spirit. In Germany the universities are incontestably the places of education which always exercise the most powerful attraction for those who wish to instruct. It is clear that this power of attraction arises from the fact that we cannot hope to find elsewhere an audience, not only well prepared, used to work, and capable of enthusiasm, but also disposed to form personal convictions; without a disposition of this kind the science of the master will not bear fruit in the pupil.

Thus is manifest in all the organisation of our universities that respect for the liberty of personal convictions, a respect more profoundly rooted among the Germans than among their Aryan brothers of the Celtic or of the Latin branch. Among these political and practical motives have greater sway. They are always disposed, and this, it appears, in all sincerity, to withdraw from the spirit of research the examination of things which appear to them indisputable as being the foundations of their political, social, and religious organisation; they consider it perfectly legitimate to tell the young people not to cast their eyes beyond the limits which they themselves have agreed not to pass.

When we hold as indisputable a certain order of questions, even when the domain of these questions is out of the way and narrow, even when we have excellent intentions, it becomes necessary to maintain in a determined path those who study, and the master must avoid everything which would disturb authority. Then independent convictions can only be spoken of in a very restricted sense.

You have seen that our predecessors have judged otherwise. If sometimes they have energetically combatted certain results of scientific research, at least they have never attacked the root of the tree. An idea which did not rest on a personal conviction

appeared to them to have at bottom no value. In their inmost heart they always guarded the firm persuasion that liberty alone is able to remedy the abuses of liberty, and that a more mature science would rectify the errors of an incomplete science. The sentiment which urged them to shake off the yoke of the Roman Church is also that under the sway of which the German universities have been organised.

But every institution founded on liberty is obliged to count on the intelligence and judgment of those who use the liberty. Independently of what has been said above on the subject of the decisions which the students have to take for themselves in what concerns the direction of their studies and the choice of their masters, the reflections which precede show also the influence which they may exercise on the masters themselves. It is a difficult thing to have to continue the course commenced by a colleague, and that difficulty is presented at the outset of every semester. At every moment the progress of the course obliges us to fall back upon what has been previously said, to consider the same questions from other points of view and in another order. The master would soon tire of this ungrateful task, if he did not find support in the zeal of his audience. In order to be at the height of his mission, he must feel himself sustained and understood by a sufficient number of intelligent pupils. The flocking of an audience to the lessons of a master has no little influence on his nomination or his advancement; and it has an influence also upon the whole of the teaching body. All this system rests upon the idea that the general current of the opinion of the students cannot long be at fault. The majority among them come to us with a reason sufficiently formed by logic, with a sufficient habit of intellectual effort, with a judgment so considerably developed by a knowledge of the best models, to be able to discern the truth from a phraseology which has only the appearance of truth. Among students we may already distinguish the *élite* who will be the intellectual guides of the new generation, and who, in a few years, may perhaps attract the attention of the world. They are those who, especially, in scientific matters, determine the opinion of their comrades; the others involuntarily allow themselves to be guided by them. Naturally, young spirits, inexperienced and impressionable, are liable to fall momentarily into error; but, in short, we may be sure that they will always return soon to just ideas.

Such are, at least, those whom the lyceums have sent to us hitherto. It would be dangerous for the universities to see arriving in great numbers, students less cultivated. It is necessary that the general spirit of the students should not decline. If that happened, the dangers of academic liberty would surpass its advantages. We ought not then to accuse the universities of pride or pedantry, when they admit only with circumspection students educated outside the lyceums. It would be more dangerous still for a foreign pressure to introduce into the faculties masters who would not be fully qualified for having the scientific independence of an academic professor.

Do not forget, then, dear comrades, that you have a great responsibility. This glorious legacy of the past, of which I have already spoken, you have to preserve, not only for our own nation, but also to serve for example to a great portion of humanity. You are bound also to prove that youth is capable of enthusiasm for the independence of convictions and of working for it. I say working. In fact, the independence of convictions does not consist in lightly accepting hypotheses without proofs; it can only be the fruit of experiments and of persevering labours. It is your duty to show that the convictions, founded on personal researches, are germs most fruitful of new ideas, and furnish better rules of conduct than the direction of the best-intentioned authority. Germany, who, in the sixteenth century was the first to strive for the liberty of convictions, who suffered and was martyred for them, is yet in the van of the fight. A noble mission is allotted to her in the history of the world, and you are called to contribute to its triumph.

REMARKABLE CHANGES IN THE EARTH'S MAGNETISM[1]

ONE of the most important, scientifically, of the special lectures at the Geographical Society, was that by Capt. Evans, in March last, on the subject of terrestrial magnetism. The concluding portion, especially, is of high scientific importance. Capt. Evans gave a historical sketch of the subject of terrestrial magnetism from the time of the discovery of the dip of the magnetic needle. After speaking further on various departments of his subject, Capt. Evans went on to say:—

"We have now passed in review the successive stages of development of our branch of knowledge, from the pregnant epoch when its principles were enunciated by Gilbert, till the period when the well-directed munificence of his own and other Governments dotted the earth's surface with observatories, and despatched land and sea expeditions, specially equipped, for the determination of the magnetic elements. We have seen how a few earnest and gifted men have, by long and patient analysis, laid the foundations for future generations to build upon as regards theory, and unravelled the apparently inextricable web surrounding the needle's daily and yearly movements; tracing these movements to their primary source, the sun: and how by the perseverance of states and of individuals, we are now in possession of accurate knowledge as to the distribution of magnetism over the surface of our globe, as represented by the variation and dip of the needle, and by the measure of the force connected with those component elements. But the task, from a scientific point of view, is far from completed while we remain in ignorance of the causes of greater changes in the earth's magnetism going on from year to year, and so on, possibly through æons of time. From a practical point of view, so far as the interests of men are concerned, the collection of records will be a never-ending task, for every generation must observe and chart the magnetic elements of its time.

"The subject of secular change is thus one of such great interest that the remaining portion of my lecture must be chiefly devoted to it. The active mind of Halley was drawn, as one of the first, to the probable nature of the causes: collecting such observations of the variation of the compass as had then been made, and projecting them on polar maps, he found that the convergence of the several directions of the needle led to two points in each hemisphere. On this he enunciated the proposition 'that the whole globe of the earth is one great magnet, having four magnetical poles or points of attraction; near each pole of the equator two; and that in those parts of the world which lie near adjacent to any of these magnetic poles the needle is governed thereby, the nearest pole always being predominant over the more remote.' Halley saw, as he confessed with despair, the difficulties attending the proposition, 'as never having heard of a magnet having four poles,' but there were the facts manifested by the earth, and he was too sagacious and sound a philosopher to pass them by. He accordingly propounded a theory which, however fantastic it may now appear, and perhaps did at the time he wrote, has nevertheless within it the fire of genius, and may probably be found yet to contain some sparks of truth. To account for the four poles, and at the same time for the secular change of the variation, he conceived that the earth itself might be a shell, containing within a solid globe, or terella, which rotated independently of the external shell: each globe having its own magnetic axis passing through the common centre; but the two axes inclined to each other and to that of the earth's diurnal rotation. It is not difficult to follow the movements of the consequent four imaginary poles in solution of the problem.

"Hansteen working at the same problem a century after Halley [1811-19], and much on the same lines, came nearly to the same conclusion with regard to the four poles of attraction: and he rendered justice to Halley by recognising him as the first who had discovered the true magnetic attraction of the globe. Hansteen, with the material at his command, went however a step further, and computed both the geographical positions and the probable period of the revolution of this dual system of poles or points of attraction round the terrestrial pole. From these computations he found that the North American point or pole required 1,740 years to complete its grand circle round the terrestrial pole, the Siberian 860 years; the pole in the Antarctic regions south of Australia, 4,609 years; and a secondary pole near Cape Horn, 1,304 years.[1] The influence of these laborious investigations on the minds of subsequent inquirers may easily be imagined.

"The matured views of Sir Edward Sabine on the secular changes—enunciated in the clearest manner in 1864-72—are deserving of the highest consideration. An ardent admirer of

[1] From Lecture at the Royal Geographical Society, March 11, by Captain F. J. Evans, C.B. F.R.S, Hydrographer to the Admiralty.

[1] *Untersuchungen über den Magnetismus der Erde.* Christiania, 1819.

the genius and no less of the sagacity of Halley, he in part follows Halley's views, and considers that two magnetic systems are directly recognisable in the phenomena of the magnetism of the globe; the one having a terrestrial, the other a cosmical origin. The magnetism *proper* of the globe, with its point of greatest attraction (*i.e.* in the northern hemisphere) in the north of the American continent is the stronger; the weaker system, or that which results from the magnetism induced in the earth by *cosmical action*, with its point of greatest attraction is, at present, in the north of the Asiatic continent. Sir Edward Sabine also expresses his belief that 'it is the latter of these two systems which by its progressive translation, gives rise to the phenomena of secular change, and to those magnetical cycles which owe their origin to the operation of the secular change.'[1]

"Reviewing these several hypotheses by the light of observations made in recent years, it is difficult, and indeed in some directions, impossible to recognise their accordance with changes now going on: there can be no doubt, notwithstanding, that Halley and Hansteen analysed their facts with skill, and that their deductions were borne out by those facts. In explanation of this anomaly it is necessary to glance retrospectively on the changes in progress at the times in which these philosophers gave utterance to their views [1700–1819]. During this long interval, and, so far as relates to parts of the northern hemisphere, for a century before, there was in the higher latitudes a general movement of the north end of the needle in the following directions:—

"Over all that area (embracing the Atlantic and Indian Oceans) from Hudson's Bay to about the meridian of the North Cape o Europe, and from Cape Horn to about the western part of Australia, the north end of the needle was successively drawn to the west at a maximum rate of 8′ or 10′ a year. From the meridian of the North Cape of Europe to that of 130° east, it was successively drawn to the east, while from thence to Hudson's Bay it was nearly stationary, or perhaps oscillated a little: in the southern hemisphere, from about the western part of Australia to Cape Horn, the movement was throughout to the east at the maximum rate of 7′ a year. There was thus a general uniformity of movement: in that hemisphere [dividing the globe into *eastern* and *western* hemispheres] which includes the Atlantic and Indian Oceans, the needle was constantly drawn more and more to the west; in the hemisphere embracing the Pacific Ocean, more and more to the east.

"So far then to the early part of the present century we can trace a harmonious movement of the needle over the whole globe, justifying the conclusions of our old philosophers; but in the year 1818 at London, and generally contemporaneous with that epoch throughout Europe and North Africa, the westerly progress of the north end of the needle ceased, and an easterly movement commenced; this continues to the present time, and with a yearly increasing rate. But in the South Atlantic during this period the westerly movement has never ceased; it is still going on, and in some parts with rapidity. Here, then, is a marked dislocation of the harmonious regularity embodied in Halley's and Hansteen's calculations and conceptions.

"The matured views of Sir Edward Sabine, to which I have drawn attention, seem to anticipate the difficulties attendant on this new and complex movement; for, if I apprehend his meaning correctly, they imply that the poles of attraction which have a terrestrial source, *i.e.* the *magnetic poles*, are not subject to translation.[2]

"The hypothesis, if further followed, is nevertheless beset with difficulties; for we can scarcely conceive changes due to *cosmical* action to be otherwise than general in character, and to affect the whole globe. Thus, if the progressive translation of the induced or weaker system in Northern Asia—and presumably of that in the southern hemisphere—were the direct causes of the secular charges, we should anticipate uniformity in the general movements of the needle as manifested by its variation and dip over the earth's surface. But this is contrary to modern experience; for in some regions great activity of movement, both in the direction of pointing and in the inclination of the needle, is going on; in others there is comparative repose in both elements; while in another region the needle remains nearly constant in its direction, while its inclination sensibly varies from year to year. For example:—

"A region of remarkable activity presents itself in the South Atlantic Ocean: a great part of the seaboard of South America extending to Cape Horn, and including St. Paul's Rocks, Ascension, St. Helena, and the Falkland Islands, with their adjacent seas, are embraced therein. In some parts of this area the westerly movement of the needle exceeds 7′ or 8′ a year, and has so progressed for nearly three centuries. On the American coast the dip of the south end of the needle *decreases* from 7·5′ to 4′ yearly, while from the Cape of Good Hope to Ascension it *increases* from 5′ to 10′ yearly. We have here, within narrow limits, a noteworthy dislocation of the observed phenomena.

"Another region of activity, so far as is denoted by the changes of variation, extends over Europe, Western Asia, and North Africa. Here the needle, in opposition to the protracted westerly movement going on in the South Atlantic, commenced moving to the eastward in the early part of this century; it has a progressive rate which in some parts now amounts to 10′ a year. The dip diminishes in this region seldom more than 3′ a year.

"A region of activity, so far as the dip is concerned, but with little change in the variation, is to be found on the west coast of South America; at Valparaiso, as at the Falkland Islands, the south dip decreases at the rate of 7′ yearly, but in sailing northward and reaching the 10th degree of south latitude, this active movement appears to cease.

"But little activity in either element now exists over the habitable part of the North American continent or in the West Indies. Throughout China there is little change in the variation, but an *increasing* dip of 3′ or 4′, and thus a reverse movement to that going on in Europe.

"Over a great part of the Western Pacific Ocean, as also in Australia and New Zealand, there is so little change in the two elements that this may be termed a region of comparative repose.

"These are a few facts relating to secular changes going on in two magnetic elements within our own time; and what are the inferences to be drawn therefrom? They appear to me to lead to the conclusion that movements, certainly beyond our present conception, are going on in the interior of the earth; and that so far as the evidence presents itself, secular changes are due to these movements and not to external causes: we are thus led back to Halley's conception of an internal nucleus or inner globe, itself a magnet, rotating within the outer magnetised shell of the earth.

"We need not here pause to discuss the probability of this fanciful conception of the old philosopher, but proceed to examine how far the behaviour of another element, the intensity of the earth's magnetism, confirms the view that movements are going on in the interior of our globe. In common I believe with all those who have pursued the study of this element from the time when Sabine's original memoir to the British Association [1837] threw so much light on this special division of the subject, I had conceived that stability, within very limited conditions, was a distinctive condition of the earth's force; and that it was alone by watchful attention to the instruments of precision devised for its determination that changes in short intervals of time, such as a generation, could be detected.[1] If we turn to the results obtained in this country through nearly half a century, it is possible that an *increase* of two or three hundredths of the total force may be found. In Italy at the present time the annual *decrease* has been given by that active observer, the Rev. Father Perry, as ·004; so also on the North American continent, where, as we are told by the zealous magnetician, Schott, there is evidence of the force slightly increasing at Washington, of being stationary at Toronto, in Canada, and slightly decreasing at Key West, in the Gulf of Mexico. So far stability, within very small limits, obtains over a very large part of the northern hemisphere. If, however, we turn to the continent of South America and its adjacent seas (parts of which are regions of marked activity as denoted by changes in the variation and dip of the needle), we shall find a diminution of the intensity of the earth's force now going on in a remarkable degree; an examination of the recent observations made by the

[1] *Phil. Trans.*, 1864, Art. vi.; 1863, Art. xii; 1872, Art. xv.

[2] So far as modern observations bear on the position of the magnetic poles, they indicate permanency rather than change of place.

[1] The investigations of that able magnetician, Mr. Broun, lead him to consider that the earth's magnetic force increases and diminishes from day to day by nearly the same amount over the whole globe. These increases and diminutions have been traced to the action of the sun in such a way that the greatest of them recur frequently at intervals of twenty-six days, or multiples of twenty-six days—a period attributable to the sun's rotation.

Challenger's officers [1] at Valparaiso and Monte Video, compared with those made by preceding observers, show that within half a century the whole force had respectively diminished one-sixth and one-seventh—at the Falkland Islands one-ninth. Farther north we find at Bahia and Ascension Island, in the same period of time, an equally marked diminution of one-ninth of the force. This area of *diminishing* force has wide limits; it would appear to reach the equator and to aproach Tahiti on the west and St. Helena on the east; at the Cape of Good Hope there is evidence of the force *increasing*.

"Such are the facts, and how are we to interpret them? Whichever way we look at the subject of the earth's magnetism and its secular changes, we find marvellous complexity and mystery; lapse of time and increase of knowledge appear to have thrown us farther and farther back in the solution. The terella of Halley, the revolving poles of Hansteen, and the more récent hypotheses of the ablest men of the day, all fail to solve the mystery. We must not, however, be discouraged at these repulses in the great conflict for the advancement of human knowledge. The present century has been productive of keen explorers in the field of terrestrial magnetism; others emulous of fame are pressing rapidly from the rear, and knowing as we do that knowledge shall be increased, we may confidently anticipate the day when this, one of Nature's most formidable secrets, shall be revealed."

[1] This extended and carefully made series are prepared for publication; we cannot too highly estimate this valuable contribution to magnetical science.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

At the annual meeting of the Convocation of the University of London on May 14, a report from the annual committee was presented which recommended closer union and co-operation of the colleges and medical schools affiliated to the University with each other and with the Senate, and also more direct participation by the University in the work of higher education and in the encouragement of mature original work. The following resolutions were carried by large majorities after an animated debate, in which Drs. Odling, Payne, Baxter, Pye-Smith, and Weymouth, and Messrs. Hutton, Carey Foster, R. N. Fowler, and Fitch, took part:—1. That while Convocation recognises the advantages of examinations conducted by a body independent of the teachers of the candidates for degrees, it is expedient that the course of study pursued in those institutions should be brought into closer connection with the Senate. 2. That with this object it is desirable for the Senate to exercise its power under the present Charter of revising the list of affiliated Colleges, and from time to time of admitting to or excluding from this list according to the position taken by these Colleges at the University examinations for degrees, and on such other grounds as the Senate may in each case determine. 3. That it would be desirable that the educating bodies included in the revised list should be invited to communicate, by delegates or in writing, with the Senate, and that facilities should be afforded to such delegates of deliberating together and of communicating with the Senate, especially on the subject of examinations. 4. That it is desirable for the examiners of the University, either in faculties or collectively, to form a Board, one of whose functions would be to consider and report upon any subject connected with the examinations which they might deem of importance to the University. 5. That it is desirable that the University should take advantage of such opportunities as may present themselves of promoting, by the institution of University chairs, or otherwise, the cultivation of such higher or less usual branches of study as can be more conveniently or more efficiently taught by a central body. 6. That it is desirable for the Senate to consider the importance of recognising independent research in the examinations for the higher degrees in such way as the senate may approve.

M. Bardoux has sent to the French Chamber of Deputies a project for establishing in Algiers *écoles préparatoires* of science, letters, and law, in order to organise in the colony superior teaching. The expenses are estimated at one-and-a-half million of francs. An *école préparatoire* of medicine was established eighteen years ago.

The late Prof. Wilhelm Pütz, well known through his excellent geographical and historical hand-books, has bequeathed the sum of 115,000 reichsmark (5,750*l.*) to the University of Bonn, with the stipulation that it is to be employed for furthering the eaching of geography and history.

SCIENTIFIC SERIALS

American Journal of Science and Arts, April.—In this number Prof. Hastings records observations which prove that the variation in dispersive power of glass, attending variation in temperature, is relatively enormously greater than that in the refractive power. This could hardly have before escaped notice, but for a singular relation in the co-efficients, in virtue of which, probably, an achromatic combination for one temperature is good for all others within moderate limits.—Prof. Rowland has made a new determination of the absolute unit of electrical resistance, his method being to induce a current in a closed circuit by reversal of the main current. He finds the B.A. unit too great by about ·88 per cent. A difference of nearly 3 per cent. between his result and that of Kohlrausch he endeavours to explain from a criticism of the latter's method, pointing out what he thinks its defects.—Prof. Langley differs from M. Janssen as to the ultimate form of the "grains" in the solar photosphere, regarding them as the ends of filaments (a simile he employs is that of a bird's-eye view of a field of grain acted on by wind), whilst M. Janssen thinks them literal spheres.—In the projection of microscope photographs, Prof. Draper increases the brilliancy of the result by removing the supporting stage of the slide further from the condenser so that a convergent beam of light may fall on the object.—Several papers in this number deal with points in American geology and physiography; the surface geology of South-West Pennsylvania, the driftless interior of North America, the ancient outlet of the Great Salt Lake, Lower Silurian fossils in Pennsylvanian limestone, intrusive nature of the triassic trap-sheets of New Jersey, &c. A tree-like fossil plant, *Glyptodendron*, lately found in the Upper Silurian rocks of Ohio is described by Prof. Claypole as (from its position) possessing a peculiar interest.

Annalen der Physik und Chemie, No. 3, 1878.—M. Röntgen here describes experiments which seem to invalidate results obtained by Wilhelmy in 1863 and 1864 regarding the condensation of fluids on the surface of solid bodies. He finds the difference of the two surface tensions, caoutchouc-air and caoutchouc-water, to be about 8·0 mg. per millimetre, when both have attained their normal value (which does not occur immediately after contact).—It is shown by M. Claes that for extremely dilute solutions of a substance with absorption-bands, the position of these bands may considerably vary, and a band is absolutely characterised by that wave-length which belongs to it in solution in solvents that are without dispersion.—In a paper on quantitative spectrum analysis, M. Vierordt investigates the influence of narrowing of the entrance-slit on colour-tone and brightness; by adapting four movable plates to the slit he has been able accurately to fix the amount of error in his determinations of intensity of light with his spectral photometer, and show that throughout the spectrum they are very small, and may mostly be neglected. In every case, however, they can be fully corrected by arrangements he describes.—M. Lommel advances a theory of normal and anomalous dispersion, and M. Fröhlich applies the principle of conservation of energy to the phenomena of diffraction.—The temperature-surface of water vapour is treated by M. Ritter.

SOCIETIES AND ACADEMIES

London

Royal Society, April 4.—"On the Determination of the Constants of the Cup Anemometer by Experiments with a Whirling Machine," by T. R. Robinson, D.D., F.R.S.

In his description of the cup anemometer (*Trans.* R. I. Academy, vol. xxii.), Dr. Robinson had inferred from experiments on a very limited scale with Robins whirling machine that the limiting ratio of the wind's velocity to that of the centres of the cups = 3. Recent experiments by M. Dohrandt have shown that this number is too great. As some of the details of M. Dohrandt's experiments appeared objectionable, and as all the data necessary for determining the constants were not given, it seemed desirable to repeat them.

After describing the apparatus used, and the locality where it was erected, he explains the forces which act on an anemometer. When these balance each other through a revolution, the condition of permanent motion is expressed by the equation $\alpha V^2 - 2\beta V v - \gamma v^2 - F = 0$. Where V and v are the velocities of the wind and anemometer in miles per hour, F the momentum of the friction, at the centres of the cups, in grains, the coefficients α, β, γ, cannot be found *à priori* in the present state of hydrodynamics; but if they be constant, or vary but little as v changes, they can be found, at least approximately, by combining several equations in which $V v F$ are known. Unfortunately this method has serious difficulties. We cannot produce wind of a known V, and must substitute for it the transport of the anemometer through the air at a known speed. But the rotation of the whirling machine produces an air vortex of considerable power, whose motion must be subtracted from that of the machine. If this motion were uniform it would do no harm, but it is found to be so very irregular that the V which must be used is uncertain.

The determination of F was also uncertain in these experiments, chiefly because the locality where the apparatus was erected (though the best which he could obtain) was affected by tremors by the action of adjacent machinery which made the frictions variable. Five anemometers were tried. No. 1 with 9-inch cups and 24-inch arms, the Kew type; No. 2, the same arms, but 4-inch cups; No. 3, with 9-inch cups and 12-inch arms; No. 4, the same 12-inch arms and 4-inch cups; and No. 5, semi-cylinder cups 9 inches square and 24-inch arms. Of these the small cups gave unsatisfactory results, the cylinders (to his surprise) the best; the 9-inch were sufficiently good to authorise the following conclusions, observing that α was measured directly. It is as the area of the cups, and is independent of the length of the arms, unless they are so short that the wake of one cup interferes with the followers.

1. The equation represents the observations well enough for all practical purposes, while V ranges from 5 to 40, and F from 113 to 3683.

2. It is equally effective if γ be omitted.

3. β and γ are probably proportional to α, and the three are as the density of the air.

4. Admitting this, the specialty of any anemometer depends on $\frac{F}{\alpha}$ only.

5. The ratio of the wind's velocity to that of cups changes with v and F. The highest value in these experiments = 21·58; its least value = 2·32.

6. With the constants which he found for the 9-inch cups, the limit of this ratio, that for V infinite = 2·30 instead of 3·0.

He proposes to verify these conclusions with real wind, and has established No. 1 near one of the Kew type similar to it. By comparing their simultaneous v under different frictions, he will obtain equations which, assuming α as known, will, he hopes, give β and γ far more certainly.

Mathematical Society, May 9.—Lord Rayleigh, F.R.S., president, in the chair.—Messrs. Wm. Hicks and T. R. Terry were elected members, and Prof. Minchin was admitted into the Society. Messrs. Brioschi, Darboux, Gordan, Sophus Lie, and Mannheim, were elected honorary foreign members.—Prof. Henrici, F.R.S., communicated a paper by Dr. Klein, of Munich, "Ueber die Transformation der elliptischen Functionen."—Prof. Cayley, F.R.S., spoke on the theory of groups.—Prof. Kennedy read his notes on the solution of statical problems connected with linkworks and other plane mechanisms. The special object of this last paper was to give an elementary solution of the problem: given a linkwork or plane mechanism of any number of links, with any force acting on any one of them, find the magnitude of the force necessary to balance the mechanism of acting in any direction on any other link. The method employed was the replacement of the two links on which the forces acted by two others which had the same instantaneous centres and the same angular-velocity ratio, but which were so chosen that they could be directly connected together by a third link. In this way a simple combination of the links was used as a "virtual mechanism" to replace the original complex linkwork, and the solution became extremely simple. Incidentally the author took occasion to insist on the advantages of the consistent use of the notion of the instantaneous centre even in the most elementary treatment of mechanical problems.—Mr. Glaisher, F.R.S., communicated a generalised form of certain series.—Mr. Kempe read a portion of his paper on conjugate four-piece linkages.

Linnean Society, April 18.—Dr. J. Gwyn Jeffreys, F.R.S., vice-president, in the chair.—The Rev. H. H. Higgins exhibited photographs of a large beetle, the *Dynastes neptunus*, Shönherr, and of an undetermined species of locust from Borneo, the latter resembling the genus Pseudophyllus, but measuring 9½ inches in expanse of wings.—A paper on the geographical distribution of the gulls and terns (Laridæ) was read by Mr. Howard Saunders. Notwithstanding the wide marine dispersion of the group, it possesses several remarkable isolated forms. In numbers there are about fifty-three species of terns and skinners, fifty of gulls, and six of Skua gulls. The majority of the typical Laridæ are found in the North Pacific, where alone the Arctic and white primaried forms are connected through *Larus glaucescens* with the group which have distinctly barred primary wing feathers. In the same area can be traced the typical hooded gulls, of which *L. ridibundus* is the Palæarctic representative, and which in *L. glaucoides* reaches unbroken to the Magellan Straits, while in the eastern hemisphere it is not found beyond 10° N. lat. There also obtains the peculiar coloured tern, *Sterna aleutica*, which connects the typical Sternæ with the intertropical sooty-terns, *S. lunata*, *S. anæstheta*, and *S. fuliginosa*. Of isolated groups which have no apparent connection with the Pacific may be mentioned the New Zealand *Larus bulleri* and *L. scopulinus*, the Australian *L. novæ-hollandiæ* and the South African *L. hartlaubi*. In the Arctic region there are two isolated specialised genera of gulls, Pagophila and Rhodostethia, which are not known on the Pacific side, while amongst the terns the intertropical genera, Næmia, Anous, and Gygis, although somewhat related among themselves, offer no particular points of union with the typical Sterninæ. It results that the bulk of the evidence favours the idea of the North Pacific probably being the centre of dispersion of these chiefly oceanic or shore-frequenting birds, the Laridæ.—Mr. R. Irwyn Lynch next made a communication on the mechanism for the fertilisation of *Meyenia erecta*, Benth. This West African acanthaceous shrub has a funnel-shaped corolla with hairy anthers midway in the tube. The longer slender flexible style has its double-lipped stigma so formed and placed that insects alighting and entering towards the nectar at the bottom of the flower on their return, so move the lever-lip of the stigma as to produce pollenisation.—Mr. J. Clark Hawkshaw brought forward some notes on the action of limpets (*Patella*) in sinking pits in, and abrading the surface of the chalk at Dover. The limpet tracks are finely-grooved hollows generally of a zigzag pattern varying from eight to fourteen inches square, and about a line deep; and according to the author produced by the lingual teeth of the animal while grazing on the fine coating of sea-weed which covers the face of the chalk. The grooving deepens as the creatures repeat the process over the same ground; they moreover sink deeper stationary basin-shaped pits, resting-places to which they return after feeding. These latter, he holds, are also formed by mechanical, and not chemical agency, as some contend. Though taken singly, the denudation of the chalk by the limpets is very insignificant, yet taken in the aggregate, the amount annually abraded must be very considerable.—The following gentlemen were balloted for, and duly elected Fellows of the Society:—The Rev. A. A. Harland, the Rev. J. J. Muir, Mr. W. S. Piper, and Mr. Fred. Townsend.

Meteorological Society, April 17.—Mr. C. Greaves, president, in the chair.—Mons. Marié Davy, Capt. N. Hoffmeyer, Prof. D. Ragona, and Dr. A. Wojeikoff, were elected honorary members.—The discussion on waterspouts and globular lightning, which was adjourned from the last meeting, was resumed and concluded.—The following papers were then read:—On the application of harmonic analysis to the reduction of meteorological observations, and on the general methods of meteorology, by the Hon. R. Abercromby, F.M.S. The meaning of the harmonic analysis is first shown, in reference to average barometric pressure, by tracing the geometrical and physical significance of every step from the barogram till the tabulated results are combined in a harmonic series. It is then shown that, whether we regard this series simply as an algebraic embodiment of a fact, or as a series of harmonic components, as suggested by Sir W. Thomson, it is simply a method of averages, and our estimate of its value must depend upon an estimate of the use of averages at all in meteorology. It is then pointed out where averages are useful, and their failure to make meteorology an exact science is traced to three causes: (1) That the process of

averaging eliminates the variable effects of cyclones and anti-cyclones, on which all weather from day to day depends; and on this are based some general remarks on the use of synoptic charts, not only in explaining and forecasting weather, but in attacking such problems as the influence of changes of the distribution of land and water on climates, and the cyclic recurrence of rain or cold. (2) That deductions from averages only give the facts, and not the causes, of any periodic phenomena. The position of diurnal and other periodic variations in the general scheme of meteorology is then pointed out, and it is shown that their causes can only be discovered by careful study of meteorograms from day to day. (3) That in taking averages, phenomena are often classed as identical, which have really not one common property. For instance, rain in this country is associated with at least three different conditions of atmospheric disturbance, and it is necessary to discriminate between these kinds before meteorology can be an exact science.—On some peculiarities in the migration of birds in the autumn and winter of 1877-78, by J. Cordeaux.—Mr. Symons gave a verbal description of the recent heavy fall of rain, on April 10 and 11, the greatest amount known to have been registered being 4·6 inches at Haverstock Hill.

Royal Microscopical Society, April 3.—Mr. H. J. Slack, president, in the chair.—A paper was read by Mr. J. W. Stephenson on a new immersion object-glass which had been designed by him to obviate the difficulty often experienced in the accurate arrangement of the adjusting collars of high-angled objectives. This new glass had a focal distance of $\frac{1}{8}$ and a balsam angle of 113°; it was stated to bear very deep eye-pieces and to have a very flat field. The great difficulty of obtaining an "immersion" fluid having the same refractive index as crown glass had at length been overcome by the adoption of oil of cedar wood diluted with $\frac{1}{4}$ part of oil of fennel seeds. The objective was exhibited in the room at the close of the meeting.—A paper was read by Mr. Frank Crisp on the present condition of microscopy in England, in which, as regarded a knowledge of the optical and mathematical principles of the instrument, unfavourable comparisons were drawn between the workers at home and abroad, and a greater degree of attention to the construction of the various portions of the instrument was urged upon English microscopists.—After the meeting Dr. Millar exhibited a small piece of a very beautiful sponge, *Acarnus innominatus*, Gray, Mr. Curties some stained vegetable tissues, and Prof. Cleve some diatoms mounted to illustrate his pamphlet.

Geological Society, April 3.—Henry Clifton Sorby, F.R.S., president, in the chair.—Rev. Albert Augustus Harland, M.A., F.S.A., and Thomas William Shore, were elected Fellows of the Society.—The following communications were read:—On an unconformable break at the base of the Cambrian Rocks near Llanberis, by George Maw, F.L.S., F.G.S.—On the so-called greenstones of Central and Eastern Cornwall, by J. Arthur Phillips, F.G.S.—The recession of the falls of St. Anthony, by N. H. Winchell, communicated by J. Geikie, F.R.S., F.G.S.

PARIS

Academy of Sciences, May 6.—M. Fizeau in the chair.—The death of M. Malaguti, correspondent in chemistry, was commented on by M. Dumas.—The following among other papers were read:—Experiments on the heat which may have been developed by mechanical actions in rocks, especially clays; and deductions regarding metamorphism, &c. (continued), by M. Daubrée. He rotated rapidly a circular marble plate on a vertical axis, and applied to a small part of its surface, near the circumference, a small weighted and fixed marble plate, measuring the rise of temperature of the latter with an alcohol thermometer. In one minute, with 445 revolutions, there was an increase of 4·5°. Dry clay was also heated by friction on limestone, &c. Apart from all eruptive rocks, the transformation of rocks and appearance of new mineral species may be caused by mechanical actions in the rocks transformed.—On a new memoir by M. Bertin. Observations on rolling and pitching with the double oscillograph, on board various ships, by M. Dupuy de Lome. *Inter alia*, it is shown that the duration of rolls varies a little (for the same vessel) with the intensity of the wind, and (contrary to what one might at first think) the rolls executed against the wind are always considerably shorter than those in the opposite direction.—M. Chauveau was elected correspondent in medicine and surgery, in room of the late M. Gintrac.—Researches proving the non-necessity of intercrossing of the conductors serving for voluntary movements at the base of the brain, or elsewhere, by M. Brown-Sequard. Two series of arguments are adduced to prove that these conductors do not intercross in the rachidian bulb nor in the protuberance; hence the inference stated.—On the mechanism and use of a differential counter, by M. Valessie. This instrument gives precise indications for regulating the average velocity and working of a machine; it is used in several French ironclads. Its principal part is a second watch, the case of which turns, by the action of the engine, in the opposite direction to that of the needle.—On the impossibility of propagation of persistent longitudinal waves in ether, free or engaged, in a transparent body, by M. Pellat. This demonstration is based (1) on the fact that the reflection at the surface of an isotropous transparent body, under Brewsterian incidence, extinguishes almost completely a ray polarised in the perpendicular plane; (2) on the principle of conservation of energy applied to such reflection, supposing it takes place according to Cauchy's formulæ.—On the telegraphic employment of the telephone, by M. Gressier. He distinguishes two kinds of disturbing sounds—that arising from induction by currents in neighbouring wires, and a very confused noise (heard most at night and accompanied with deflection of the galvanometer), which he thinks due to the wire passing in different places through air layers which undergo rapid and considerable variations in potential, producing various currents. This suggested a means of studying variations in atmospheric electricity. M. Du Moncel pointed out that these latter currents had been studied with the galvanometer.—On the crystallisation of silica by the dry process, by M. Hautefeuille. Alkaline tungstates may with advantage be used instead of phosphates (as used by M. Rose), for they render obtainable at will, crystallised silica, either in the trydimite or quartz form.—On the gold method and the termination of the nerves in unstriated muscle, by M. Ranvier. As in striated muscle, these nerves end with an arborised expansion of the cylinder axis on the surface of the muscle-elements. In contraction an organ co-operates which is not under the direct action of the nerve-centres.—Action of morphine on dogs, by M. Picard. The vascular dilatation and contraction of the pupil result from paresis of the sympathetic nerve.—On Wartelia, a new genus of annelids considered wrongly as embryos of Terebella, by M. Giard.—On the malacological fauna of New Guinea, by M. Tapparone-Canefoi. This belongs quite to the great fauna of the Indo-Pacific region.—Soda in plants, by M. Contejean. More than three-fourths of terrestrial plants (not maritime) belonging to regions not apparently saline contain soda; it is mostly in the subterranean portion. Aquatic plants have it in all their submerged parts. The aptitude for soda varies according to family, species, &c.

CONTENTS PAGE

THURSDAY, MAY 23, 1878

UNIVERSITY EXTENSION

LAST week two remarkable steps were taken affecting the prospects of universities in this country, each being a new departure from which it will be difficult to recede. On Tuesday the University of London revised its constitution so far as the convocation of graduates could do it, and the changes which were adopted unanimously, or by considerable majorities, would go some way to remove many of the evils connected with that great centre of examination. On Wednesday, the Yorkshire College, Leeds, supported by numerous representatives from the other large towns of the north, waited on the Lord President of the Council to entreat Her Majesty "*if* she is pleased to create a new university, not to give a charter to the Governors of Owens College, Manchester, but to a new Corporation, with powers to incorporate Owens College and such other institutions as may now or hereafter be able to fulfil the conditions of incorporation laid down in the charter"—and also "not to connect the new university by name with any locality." Both of these movements are due to the initiative of Owens College, and if the former should result, as there seems reason to hope, in real improvements in the University of London, the people of Manchester will have done something for sounder views on education and the reform of that system of examination apart from teaching, in which Mr. Lowe recognises the essential excellence of a university.

The Manchester proposal was that Government should grant a university charter to Manchester, which has now 500 regular day students and 800 evening students, and that that charter should entitle it to confer degrees. It was objected, on the one hand, that such a precedent might lead to an indefinite multiplication of competing universities, and a consequent degradation of degrees, and on the other, that the interests of teaching institutions in the neighbourhood, such as the Yorkshire College at Leeds, might be prejudiced by the prestige conferred upon Owens College. With the view of meeting these objections, the authorities of Manchester made two suggestions. They proposed that the examiners for their degrees should be half of them professors in the university, intimately acquainted with the teaching actually given, and half of them outsiders, practically commissioned to guarantee that the degree should not be conferred on any lower qualification than that usually required in other universities. They proposed also that the new university should be empowered to confederate with it other institutions as they arose, where adequate qualifying instruction could be permanently given, and that these institutions should then be admitted to a full proportionate representation. To guard against the danger which might have been real enough, that Owens College would decline to use this power, except upon really unpractical conditions, they proposed that the Privy Council, or other educational authority of the Government, should have the right to revise their acceptance or rejection of new institutions claiming federation. Their scheme started in this way from the basis of an actual and realised success—it proposed a charter to a body which seems generally admitted to have established for itself a real university position and character; it provided guarantees against the degradation of degrees, which would certainly have been repeated in the case of any new claimants to a similar position, and it arranged that institutions like Leeds, with no reasonable ground for expecting, for many years at least, to obtain an independent university position, might fairly anticipate confederation with Manchester, if they so desired, at an early date. The confederation part of the scheme was thus directed to guard the interests of Leeds and of other towns which might soon be in a similar position, but the Manchester people did not offer at once and without further inquiry to admit Leeds or any other college.

The Leeds memorial, on the other hand, reads as if it had been carefully drawn to gather together all possible objections to the Manchester scheme. The object of its promoters may not be, but the effect of its promotion is likely to be, mere delay. The words employed, "If Her Majesty is pleased to create a new university," are calculated to unite in support not only those who think Her Majesty ought to be advised to do so, but any number of persons who think just the opposite. It was supported by Nottingham, which expressly stated that she saw no need for anything further than an affiliation, such as she supposes she has, with the University of Cambridge, and by Dr. Acland, whose speech was entirely directed to prove that no new northern university was wanted or was likely to be useful. The deputation in fact, was for opposition to Manchester, not for the promotion of a rival scheme of a new northern university in which Manchester should only be "*prima inter pares.*" The memorialists asked for nothing. They asked that "*if*" the Government were disposed to do anything, they should *not* give a charter to Manchester either in fact or by name. The positive suggestions they put forward alternatively were of the vaguest and most shadowy kind. In place of Manchester, which exists, and has submitted its claims to the consideration of the government and of the country, they suggested that the government should give a charter to "a new corporation, with powers to incorporate Manchester and such other institutions as may now or hereafter be able to fulfil the conditions of incorporation laid down in the charter." No doubt the Government could create a university by giving a charter to a certain number of eminent persons representing localities in the north of England. What, then, would be the basis of the new university? It would consist—in the conditions of incorporation laid down in the charter! "The new university," says Lord Frederick Cavendish, "is to be closely connected with the colleges of the north of England, and adapted to the circumstances of the great industrial community there." What are the colleges of the north of England? Liverpool may possibly have a college some day. Birmingham certainly will. Newcastle, being connected with Durham, makes no claim. Sheffield and Nottingham have buildings in which the work of the Cambridge extension scheme will be regularly carried on. Outside Manchester Leeds alone exists so as to have even the shadow of a claim to a more independent recognition than every one of them possesses through the University of London at the present moment.

If Leeds had wanted to be incorporated on an equal footing with Manchester from the first; if she had been conscious that her present position made that claim unreasonable; if she had utterly disbelieved the Manchester people and their promises to listen to her as soon as she liked, and been convinced that her appeal against them to the Privy Council would be in vain, she would have acted much as she has done in getting up this deputation. The promoters of a real rival scheme would have named the colleges which should at once have been confederated and constituted the new university. It was to use a weapon of war to entreat the Government "*if* they think of chartering a new university," to sit down and evolve a scheme of a "new corporation," and to sketch its fundamental principles in "the conditions of incorporation laid down in the charter."

It seems tolerably plain that the Owens College people in their anxious attempts to be reasonable, to safeguard every interest, and to take account of every susceptibility have gone too far. They have offered terms to Leeds and all similar institutions of the present and future, which Lord Frederick Cavendish, the President of the Yorkshire College, declares "fair and equitable," but which seem only to have aggravated Leeds and to have weakened their own case as against the University of London. Now that that body appears at least to think of returning to the principle of affiliated colleges, she may perhaps soon offer a better centre of affiliation than Manchester or any new college in the north. It has been her deliberate policy for the last twenty years to exercise no control over what used to be called affiliated colleges, which she does not exercise over private tutors and private students. That policy was logical, and it will be more than curious to see how the senate, if they proceed to move in the lines indicated to them in Convocation, will attempt to retrace their steps. Convocation proposes that "the list of such colleges should be revised from time to time," and that the right of "excluding from or admitting to such list" should again be exercised. But it does not say how an affiliated college in the list is to hold any better position in the University than a private "crammer" who stands upon his own capacity for successfully preparing candidates for degrees. It suggests that the authorities of such colleges be entitled to communicate with the Senate from time to time about the examinations. Such communications could not fail to be interesting, but it is difficult to see how they can be given particular effect to, so long as it is the fundamental principle of the University that the non-collegiate should be examined on the same footing as the collegiate student. It proposes that the examiners should form a board for mutual consultation with some authority, and nothing could be better. It suggests that independent research should be recognised in connection with the higher degrees, and the suggestion appears eminently reasonable and practicable. It recommends the foundation of University Chairs for the cultivation of such branches of study as can be more conveniently or more efficiently taught by a central body. It is not very easy to realise the kind of University Chairs suited to a central university with no students directly connected with it and no class rooms. Possibly the occupants of the Chair would have to give isolated courses of lectures, open, perhaps, to the general public, or to students of affiliated colleges, such as are now given at the Royal Institution. Certainly, eminent men could be found to do as much in the way of teaching in connection with the University of London as some of the distinguished occupants of the professorial Chairs of Oxford and Cambridge.

Convocation desires to move towards the rehabilitation of the affiliated colleges which were practically cast adrift twenty years ago, and it will be most interesting to see whether they can be rehabilitated consistently with the fundamental principle that the examinations of the university are to be perfectly open to all students who pay the examination fee. We venture to hazard the prediction that they cannot, and that though the affiliated colleges may be flattered with exceptional courtesies, affiliation will never mean anything very serious so long as the unattached student is not given up. Neither Manchester nor probably the airy confederation sketched out in the supposed interest of Leeds, find any room for the non-collegiate student. The promoters of both schemes will watch with the keenest interest the action of the London Senate in view of the resolutions of Convocation. But there is no such prospect of radical changes in the attitude of the University that either need be postponed.

Owens College would probably have been in a better position to-day if she had never gone out of her way to conciliate Leeds. She has only been wasting an energy that might have been better spent in combating the "Dutch auction" theory of degrees which Lord Ripon brought out with great emphasis at the deputation. The degradation of degrees would be a serious evil, for ordinary degrees are as low at present at Oxford and Cambridge and elsewhere, as any reasonable being can wish. But the multiplication of universities, so far from being an evil, is an unmixed good, and degradation and multiplication are by no means inseparable, though they have frequently been combined. German degrees were at one time in a disgraceful state; American degrees are in a bad way now; Scotch degrees lost caste when St. Andrews, which had no real medical school, sold licences to treat Her Majesty's subjects medically after an easy examination. There is one simple remedy. Let the universities give their degrees without fee or reward, and let these degrees cease to admit directly to any profession. The temptation has always been a pecuniary one. A Dutch auction is only possible in a world where people with brains and no money want to get money out of the pockets of people with no brains. Even without so radical a remedy, the Dutch auctions have been stopped in Germany and in Scotland, and to a large extent in America. They need never begin in Manchester. Let it be arranged that no new degree-granting university shall derive one halfpenny of profit from its degrees, and the whole difficulty vanishes. If there were no fear of the degradation of degrees, it would be as much for the benefit of the people of England that Manchester and Leeds and Birmingham and Liverpool should become University seats in due season, after they have fairly won their title by their own exertions, as Manchester has already done, as that everybody should learn to read and write and count. We cannot understand the

real friends of education, even in Yorkshire, spending as much energy to provide the Government with a reason for doing nothing as might itself have built and endowed a college.

PHYSICAL SCIENCE FOR ARTISTS[1]

III.

LET me begin my third paper by an attempt to graphically illustrate the conclusions to which I drew attention at the end of my second. These conclusions were as follows:—(1) Very complex molecules when in vibration give us light that we call white, which light, when split up by a prism, gives us a spectrum consisting of

Red
Orange
Yellow
Green
Blue
Indigo
Violet,

going from one end to the other. We will represent this by open letters (the initials of the various colours) to show that this is a case of the giving out of light.

V I B G Y O R

We next come to the second conclusion. (2) Very simple molecules give us coloured light. When this coloured light is analysed by the prism we find associated with the sensation of colour the fact that in this case the light is not continuous; we do not get a complete spectrum represented by

V I B G Y O R

but when we deal with the molecules of one chemical element we may get only

Y

in the case of another chemical element only

R

in another

V R

and so on, the letters representing that light is only given out in those parts of the spectrum represented by them, and not generally.

This we may also indicate by using black letters for the regions in which light is not given out, and white letters for those where light is emitted, thus

V I B G Y **O R**
V I B G Y O R
V **I B G Y O** R

We get bars of light here and there (the various mixtures of which produce different colours), instead of a *complete series* of bright bars (the mixture of which produces what we call white light).

The decomposition and recomposition of white light to which I have referred is really one of the most beautiful and at the same time most simple experiments in the whole range of optical science. The recomposition has lately been demonstrated by an elegant toy in the shape of a top, on which, while rotating with considerable rapidity, a circular disc of cardboard containing the different colours in their proper proportions painted in sectors

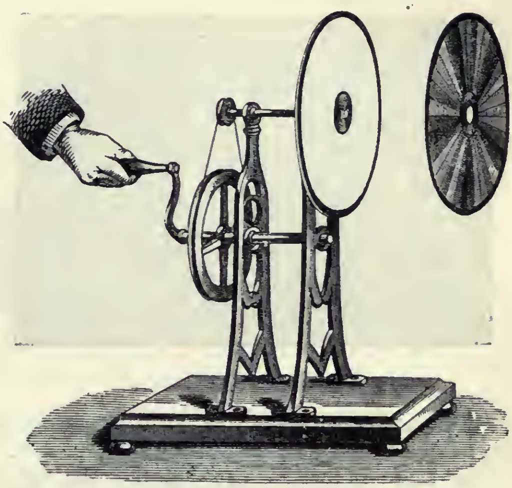


FIG. 1.—Rotating coloured disc experiment.

is placed. A lecture-room experiment of the same form is represented in the accompanying woodcut. The various colours shown on the disc at rest to the right form white light when the disc is rapidly rotated by the handle shown in the figure.

Two common lustres, or still better, two prisms (Fig. 2), enable the recomposition to be well seen. First arrange one prism as on the right in the accompanying diagram (Fig. 3). If the eye be placed where the second prism is to the left, to receive the light passing through the first prism, all the colours will be seen, but if the eye

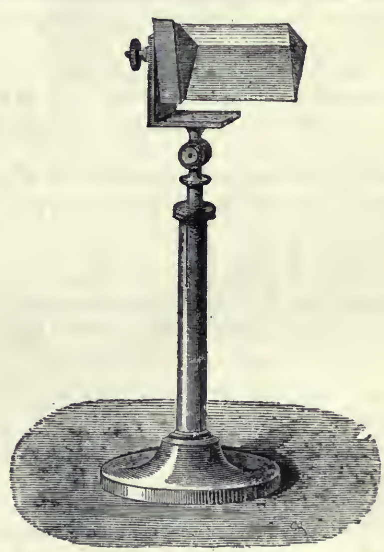


FIG. 2.—Prism mounted on stand.

is replaced by a second prism as shown, the light on emerging from the second prism will be found to be reconstituted, the colours will have again commingled, to form white light. The prism, with its refracting edge

[1] Continued from p. 61.

turned upwards, will have exactly undone the work done by the prism with its refracting edge turned downwards.

For the last fifteen years students have been occupied in mapping the spectra of coloured light sources, and the result is now that every line in the spectrum of every substance has been thus recorded with the utmost diligence and care; so that the statement that the spectrum we get from the ultimate molecules of each different chemical substance is different, is substantiated by a most overwhelming mass of evidence.

FIG. 3.—Recomposition of light.

It must be understood that in what I have stated I have represented the phenomena as being much more simple than they really are. It is quite true we can, by toning down the molecular motion, make the spectrum of each chemical element only occupy one of the spectral regions as a rule; but it is equally true that when the motion is great all the regions are filled with lines as the two following examples (Figs. 4 and 5) will show.

Here, then, is the sure and certain knowledge that we possess regarding the motions of molecules so far as one cause of the coloured phenomena observed in Nature is concerned. There is another result which has been gathered in the region of the infinitely little which helps us to another cause of these phenomena.

So far we have considered these ultimate molecules in a state of extreme vibration. As a matter of fact, so long as we are dealing with these ultimate finenesses of matter, we can still assure ourselves of the motion of the molecules when their vibrations are far less vivid. How is this accomplished?

In this way. The molecules are so apt to vibrate each in its own period that they will even take up vibrations

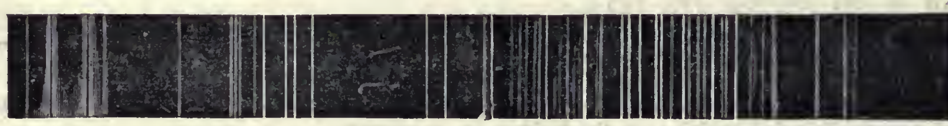


FIG. 4.—Part of the blue region in the spectrum of a mixture of incandescent aluminium, calcium, and iron vapours, showing how the images of the slit produce the appearance of bright lines.

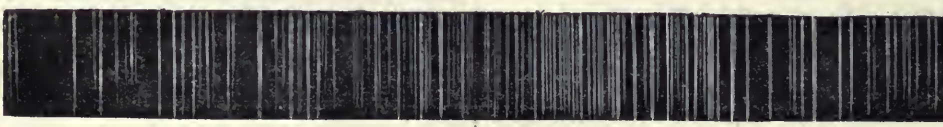


FIG. 5.—Part of the blue region in the spectrum of incandescent iron vapour.

from light which is passing among them, provided always that the particular wave-lengths of light in which they themselves vibrate are contained in the light which they receive.

Let us imagine a case. Suppose I have at one end of a room a vivid light source giving us all the possible waves of light from red to violet.

V I B G Y O R

Also suppose that I have in the middle of the room a screen of molecules feebly emitting yellow light.

Y

What will happen? Will the light come to my eye exactly as if the molecules were not there? No; it will not. There will be a difference. What then will be the difference?

Experimentally we find that the molecules which give out yellow light have kept for their own purposes—filched so to speak, from the light passing through them—the particular vibrations which they want to carry on their own motions, and we shall have

V I B G O R

as a result; the light comes to us minus the vibrations which have thus been utilised, as we may put it, by the screen of vapour.

We have in fact an apparently dark space which may be represented in this way

V I G B **Y** O R

When we use a spectroscope we get the continuous spectrum with a dark band across the yellow absolutely identical in position with the bright band observed when the molecules of the vapour of which the screen is composed radiated light in the first instance.

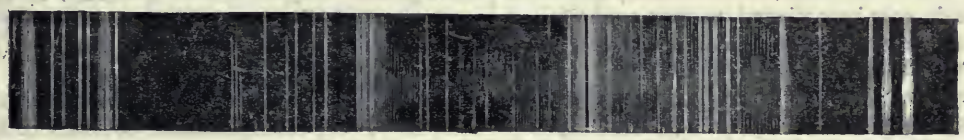
There is an experiment in the world of sound which, perhaps, may render the physics of this action clearer. If we go into a quiet room, where there is a piano, and sing a note, and stop suddenly, we find that note echoed back from the piano. If we sing another note we find that it also is re-echoed from the piano. How is this? When we have sung a particular note we have thrown the air into a particular state of vibration. One wire in the piano was competent to vibrate in harmony with it. It did so, and vibrating after we had finished, kept on the note.

One example of the phenomena observed when we use a series of molecules as a screen, I can bring

out by means of the accompanying copy of a photograph. A small mass of metallic calcium has been placed between the two poles of an electric lamp. By means of the passage of the current this mass of calcium has been raised to a very high temperature, and it has been driven to its ultimate fineness,—it has been driven, in fact, to a state of vapour competent to give out very thick bright lines in the regions marked *a*, *b*, *c* (Fig. 6.) As the vapour is gradually given off from the mass of metallic calcium, it has surrounded the interior part, and has gradually cooled as it got away from the action of the electricity. So that here we have an intensely heated mass of calcium vapour in the centre surrounded by a mass of calcium vapour which is cooling.

We have, therefore, between us and the central mass a screen of calcium molecules under exactly the same conditions as those we suggested in the screen of molecules giving out yellow light lying between us and a distant light source.

What then are the facts? On the photograph three



b *a*

FIG. 6.—Reversal of the light of hot calcium vapour by cool calcium vapour.

bright broad bands of light are seen in different parts of the spectrum, which represent three of the characteristic bands of the metal calcium. I want especially to call attention to the fact that in the middle of these bands, especially in the one lettered *a*, there is a fine line of intense blackness.

That is to say, in that particular region of the spectrum there has been no light to paint an image of the slit. What has become of that light? The light has been at work, not on the photographic plate, but among the cooler exterior molecules of calcium which have used it up.

I shall now take it for granted that the great principle, that molecules absorb the light passing through them in the same wave lengths as those which they give out when vibrating on their own account, has been rendered familiar.

We must not forget that this statement is only true so long as the molecular combinations are the same, and that we only get this result in the shape of bright lines when we are dealing with the ultimate finenesses of each chemical substance, that is, when we are dealing with each chemical substance in a state of vapour.

Without going further, then, it is clear that we are now in presence of two causes of coloured light as opposed to white light. There is, so to speak, a cause of *ef*fect and a cause of *de*fect. We now know of one reason why light may be red: the luminous vibrating substance may only be competent to vibrate at that particular rate which gives us the sensation of red light. But this is not the only reason why light may be red. If we assume a screen so constituted that all the light proceeding from a white light source, *except the red*, should be absorbed by the screen, we have there a condition in which the sensation of red would again be produced. In this case it will not be the *ef*fect of red vibrations alone in the light source, but by virtue of the *de*fect of all the others which we have assumed to be absorbed by the screen.

In both cases we arrive at

VIBGYO R

in the first case because the only light given us is

R

in the second because the screen vibrates only in

VIBGYO

and therefore only absorbs these colours.

Red fire in a pyrotechnic display is an example of the first case. The setting or rising sun is an example of the second.

The expenditure of a very small amount of money and time will enable any one to become acquainted with many of these phenomena. The best spectroscope after all, perhaps, ease of manipulation being taken into account, is a prism held close to the eye, and a fine slit, say one-twentieth of an inch broad and two inches long, carefully cut out of a piece of tin-foil, gummed on a plate of glass. When this slit, say a foot off, is observed with the refracting angle of the prism parallel to its length, a very brilliant spectrum of a candle just in front of the slit is obtained, even though it be wanting in definition. This latter can of course be improved if a narrower slit be employed: for in spectra all impurity comes from overlapping of images, and the operations of Nature are so fine that it seems as if a pure colour, such as a pure blue or a pure red, will for ever remain an abstraction; for, however great the dispersion, the adjacent wave-lengths will remain commingled, and commingled wave-lengths define a compound colour.

Instead of reducing the width of the slit, if it is not connected with the prism by means of a tube, as it may conveniently be, the slit can be removed further from the prism. In this way we get apparently a narrower slit without any reduction in the quantity of light which passes through it to the eye. A gas flame or a candle placed in front of this slit is all that is necessary to produce a continuous spectrum.

J. NORMAN LOCKYER

CLIFFORD'S DYNAMIC

Elements of Dynamic. Part I. Kinematic. By W. K. Clifford. (London: Macmillan and Co., 1878.)

THOUGH this preliminary volume contains only a small instalment of the subject, the mode of treatment to be adopted by Prof. Clifford is made quite obvious. It is a sign of these times of real advance, and

will cause not only much fear and trembling among the crammers but also perhaps very legitimate trepidation among the august body of Mathematical Moderators and Examiners. For, although (so far as we have seen) the word quaternion is not once mentioned in the book, the analysis is in great part purely quaternionic. And it is not easy to see what arguments could now be brought forward to justify the rejection of examination-answers given in the language of quaternions—especially since in Cambridge (which may claim to lay down the law on such matters) Trilinear Cöordinates, Determinants, and other similar methods were long allowed to pass unchallenged before they obtained formal recognition from the Board of Mathematical Studies.

Every one who has even a slight knowledge of quaternions must allow their wonderful special fitness for application to Mathematical Physics (unfortunately we cannot yet say Mathematical *Physic!*): but there is a long step from such semi-tacit admissions to the full triumph of public recognition in Text-Books. Perhaps the first attempt to attain this step (in a book not ostensibly quaternionic) was made by Clerk-Maxwell. In his great work on *Electricity* all the more important Electrodynamic expressions are given in their simple quaternion form—though the quaternion *analysis* itself is not employed:—and in his little tract on *Matter and Motion* (NATURE, vol. xvi. p. 119) the laws of composition of Vectors are employed throughout. Prof. Clifford carries the good work a great deal farther, and [if for this reason alone] we hope his book will be widely welcomed.

To show the general reader how much is gained by employing the calculus of Hamilton we may take a couple of very simple instances, selecting them not because they are specially favorable to quaternions but because they are familiar in their Cartesian form to most students. Every one who has read *Dynamics of a Particle* knows the equations of non-acceleration of moment of momentum of a particle, under the action of a single centre of force, in the form

$$\left.\begin{aligned} x\ddot{y} - y\ddot{x} &= 0 \\ y\ddot{z} - z\ddot{y} &= 0 \\ z\ddot{x} - x\ddot{z} &= 0 \end{aligned}\right\}$$

with their first integrals, which express the facts that the orbit is in a plane passing through the centre, and that the radius-vector describes equal areas in equal times. But how vastly simpler as well as more intelligible is it not to have these *three* equations written as *one* in the form

$$V\rho\ddot{\rho} = 0$$

and the three first integrals above referred to as the immediate deduction from this in the form

$$V\rho\dot{\rho} = \alpha.$$

Take again Gauss's expression for the work done in carrying a unit magnetic pole round any closed curve under the action of a unit current in any other closed circuit. As originally given, it was

$$\iint \frac{(x^1-x)(dydz^1-dzdy^1)+(y^1-y)(dzdx^1-dxdz^1)+(z^1-z)(dxdy^1-dydx^1)}{\left((x^1-x)^2+(y^1-y)^2+(z^1-z)^2\right)^{\frac{3}{2}}}$$

With the aid of the quaternion symbols this unwieldy expression takes the compact form

$$\iint \frac{S.\rho d\rho d^1\rho}{T\rho^3}.$$

The meanings of the two expressions are identical, and the comparative simplicity of the second is due solely to the fact that it takes space of three dimensions as it finds it; and does not introduce the cumbrous artificiality of the Cartesian cöordinates in questions such as this where we can do much better without them.

In most cases at all analogous to those we have just brought forward, Prof. Clifford avails himself fully of the simplification afforded by quaternions. It is to be regretted, therefore, that in somewhat higher cases, where even greater simplification is attainable by the help of quaternions, he has reproduced the old and cumbrous notations. Having gone so far, why not adopt the whole?

Perhaps the most valuable (so far at least as physics is concerned) of all the quaternion novelties of notation is the symbol

$$\nabla = i\frac{d}{dx} + j\frac{d}{dy} + k\frac{d}{dz},$$

whose square is the negative of Laplace's operator: *i.e.*

$$\nabla^2 = -\left[\left(\frac{d}{dx}\right)^2 + \left(\frac{d}{dy}\right)^2 + \left(\frac{d}{dz}\right)^2\right].$$

A glance at it is sufficient to show of what extraordinary value it cannot fail to be in the theories of Heat, Electricity, and Fluid Motion. Yet, though Prof. Clifford discusses Vortex-Motion, the Equation of Continuity, &c. we have not observed in his book a single ∇. There seems to be a strange want of consistency here, in coming back to such "beggarly elements" as

$$\delta_x u + \delta_y v + \delta_z w$$

instead of

$$- S\nabla\sigma,$$

especially when, throughout the investigation, we have σ used for

$$ui + vj + wk;$$

and when, in dealing with strains, the Linear and Vector Function is quite freely used. Again, for the vector axis of instantaneous rotation of the element at x, y, z (ρ), when the displacement at that point is $\sigma = iu + jv + kw$, we have the cumbersome form

$$\tfrac{1}{2}\left\{(\delta_y w - \delta_z v)\, i + (\delta_z u - \delta_x w)\, j + (\delta_x v - \delta_y u)\, k\right\}$$

instead of the vastly simpler and more expressive

$$\tfrac{1}{2} V\nabla\sigma.$$

It may be, however, that this apparent inconsistency is in reality dictated by skill and prudence. The suspicious reader, already put on his guard by Clerk-Maxwell's first cautious introduction of the evil thing, has to be treated with anxious care and nicety of handling:—lest he should refuse altogether to bite again. If he rise to the present cast we shall probably find that Prof. Clifford has ∇, in the form as it were of a gaff, ready to fix him irrevocably. That he will profit by the process, in the long run, admits of no doubt:—so the sooner he is operated on successfully the better. What is now urgently wanted, for the progress of some of the most important branches of mathematical physics, is a "coming" race of intelligent students brought up, as it were, at the feet of Hamilton; and with as little as may be of their freshness wasted on the artificialities of x, y, z. Till this is procured, quaternions cannot have fair play. Nut-cracking,

though occasionally successful for a moment, is the most wasteful and destructive of all methods of sharpening the teeth.

What we have at some length discussed is the most prominent feature of the present work, but by no means its only distinctive one. No writer, who has any claim upon his readers at all, can treat even the most hackneyed subject without giving a new and useful turn to many a long-known truth. Many of Prof. Clifford's proofs are exceedingly neat, and several useful novelties (*e.g.* Three-bar Motion) are introduced. We have to complain, however, of a great deal of unnecessary new and very strange nomenclature:—for a large part of which the author is not responsible, his error (for such we cannot help considering it) consisting in giving this stuff a place of honour in his book. One does not require to be very violently conservative to feel dismayed at an apparently endless array of such new-fangled terms as Pedals, Rotors, Cylindroids, Centrodes, Kites, Whirls, and Squirts! Yet these are but a few gleaned at random from the book. Something, it seems, *must* be hard in a text-book—simplify the Mathematic, and the Anglic (*i.e.* the English) immediately becomes perplexing. P. G. TAIT

PHYSICS OF VOLCANOES

Beitrag zur Fysik der Eruptionen und der Eruptiv-Gesteine. Von Dr. Ed. Reyer, Docent an der Universität in Wien. (Vienna: Alfred Hölder, 1877.)

DR. Reyer, of Vienna, has already made his mark in geological literature by the admirable work entitled "Die Euganeen: Bau und Geschichte eines Vulcans," in which he has given a very clear and instructive interpretation of the phenomena presented by that grand tertiary volcano of Northern Italy, of which the internal structure has been so well displayed to the geologist through the agency of denuding forces. Those who are acquainted with the merits of the first published work of Dr. Reyer will eagerly take up the volume which has now made its appearance, the title of which stands at the head of the present notice; nor will their anticipations that a difficult question will meet with masterly and original treatment at the hands of its author be disappointed.

A starting-point for a series of discussions of the phenomena of volcanic action and the causes to which these are due is found by Dr. Reyer in the demonstrated capacity of various substances in a state of igneous fusion for absorbing certain gases. If the suggestion that in this peculiar property of bodies in a state of fluidity under the action of heat we find a key to many of the most remarkable phenomena of volcanic eruptions be not altogether new, it must at least be admitted that it has never before received such ample discussion and illustration as it now undergoes in the hands of Dr. Reyer, and even still less has hitherto been done in applying the explanation in question to these numerous minor and concomitant phenomena which precede, accompany, and follow volcanic outbursts.

In the first part of the work before us, the author, after citing the interesting observations of Gay Lussac, Fournet, Thenard, Réaumur, and other chemists, in proof of the property of absorption as displayed by substances in a state of igneous fusion, goes on to show that many of the striking appearances exhibited during volcanic eruptions clearly point to the conclusion that a highly-heated magma within the earth's crust has, through infiltration, become charged with liquid and gaseous materials. He then proceeds in the second part of the volume to show how many of the phenomena of volcanoes—such as the succession of events in the history of their formation and in that of each individual eruption, the peculiarities of the internal structure of volcanoes and of the masses of lava extruded from their vents, and the nature of the gaseous exhalations which accompany the outbursts during their several stages—receive a simple explanation from this remarkable property exhibited by substances in a state of fusion.

Apart, however, from the value of its more speculative portions, Dr. Reyer's work will be welcomed by geologists as bringing together in a connected form all the most important of the recent observations which have been made upon the nature and products of volcanic activity. It is in this respect that the third part of the work before us, that which deals with the peculiarities of volcanic rocks, appears to us to be especially worthy of attention. The author not only admits that the principle which has been so long followed by German petrographers, of basing the classification of igneous rocks on their geological age, is altogether untenable, but he goes farther and strongly denounces the mischievous tendencies of this method in obscuring some of the most striking inferences to be derived from the exact study of such rocks. Strongly insisting on the fact that portions of the same magma may, under different physical conditions, assume a granitic, a porphyritic, or a vitreous structure, Dr. Reyer shows clearly how the various igneous intrusions found associated with sedimentary deposits were in all probability originally connected with centres of volcanic activity; and he also shows the grounds for the inference that masses of granitic structure are being formed at the present day by the slow consolidation under pressure of portions of the magma below the existing volcanic vents.

Of the urgent necessity for reforms in our petrographical nomenclature, the author of this work, holding the views he does, clearly perceives the necessity; and his suggestions upon the subject deserve, as they doubtless will receive, the careful attention of geologists. Some of the interesting relations between the structure and composition of rocks are, we may remark in passing, very well illustrated by the series of ingenious diagrams which accompany this volume. J. W. J.

OUR BOOK SHELF

Travels in the Footsteps of Bruce in Algeria and Tunis; Illustrated by Facsimiles of his Original Drawings. By Lieut.-Col. R. L. Playfair, H.B.M. Consul-General in Algeria. (London: C. Kegan Paul and Co., 1877.)

THE northern regions of Africa that border on the Mediterranean Sea would form a deeply interesting study for the historian. Perhaps no other portion of the world's surface has passed through more marked phases of civilisation, yet all of these have passed away and left but small trace behind them. Placed between a wondrously teeming offshoot of the Broad Atlantic

and a markedly sterile desert, this strip of territory seemingly wanting in none of Nature's riches save flowing rivers, has been conquered successively by the Romans, the Vandals, the Byzantines, the Greeks, and the Arabs. All these several possessors came and conquered and settled on these lands; but the first four civilisations died away, and the last is disappearing, in at least the large central portion of this district known as Algeria, and now under French rule. Who can tell whether this new phase will have any more vitality than the rest?—for the native tribes seem to be as unreclaimable as their own Sahara.

The many architectural ruins scattered over this district still attest the greatness of her conquerors, and the visitor to any of the provinces of Algeria, more especially to Algiers, will be astonished at the size and grandeur of these remains. In 1765 the traveller Bruce, as he tells us in an autobiography, was told by my Lord Halifax, "that the way to rise in the king's favour was by enterprise and discovery; that all Africa, though just at our door, was yet unexplored; that every page of Dr. Shaw, a writer of undoubted merit, spoke of some magnificent ruins which he had seen in the kingdoms of Tunis and Algiers, and that now was the age to recover these remains and add them to the king's collection." With this suggestion Bruce was offered the post of consul at Algiers, with a good salary. Bruce at once put aside for the moment all thoughts of the fountains of the Nile, "as involving an enterprise above the powers of an untried ordinary man," and setting out for Italy, he passed through France, and was carried in H.B.M. frigate *Montreal* from Naples to Algiers. The story of Bruce's life is yet to be written; probably no traveller has ever had to contend against a greater amount of ill-deserved obloquy. His account of his travels, we know, was received with the greatest incredulity, and yet there are very few of his statements that have not, since he published them, been abundantly confirmed. It would seem but an act of justice that we should, in the light of modern discovery, have a new edition of Bruce's Travels thoroughly well annotated, and we can think of no one so well qualified for this task as the author of this volume, which gives us an account of how Col. Playfair came to travel in the footsteps of this great father of African travel.

Bruce, we have seen, was British Consul-General at Algiers in 1765, and he received this appointment to enable him to examine and describe the many fine ruins said so truly by Dr. Shaw to be scattered over Tunis and Algeria. An account of these travels, with detailed descriptions and drawings of these ruins, was prepared by Bruce, with the intention of publishing them; but it is probable that the manner in which the simple narrative of his travels was received by the public had the effect of making him abandon this idea.

We must refer the reader to this volume for information as to how Col. Playfair, who now occupies Bruce's place as H.M. Consul-General at Algiers, discovered Bruce's manuscripts and drawings in the library of Lord Thurlow, whose wife is the great-great-granddaughter of the traveller as well as heiress of Kinnaird. As a result of this discovery, Col. Playfair determined to follow Bruce's footsteps in Tunis and Algeria, to visit every ruin which he had illustrated, and so to plan his route as to include all that was most picturesque and instructive in a country that is even yet hardly at all known to the modern traveller; and well he does all this in the sumptuous quarto volume before us.

This volume will form a lasting monument to the memory of Bruce, and some five-and-twenty of the large quarto illustrations, being facsimiles from Bruce's drawings, will serve to show how accurate as an architectural draughtsman he was, and how independent he might—had it not been for the fashion of the times—have been of the "adornments" of his Italian artist.

E. P. WRIGHT

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

On the Availability of Normal-Temperature Heat-Energy

1. I WAS interested to see the letter of Prof. A. S. Herschel, in NATURE, vol. xviii. p. 39, referring to my papers on the subject of the derivation of work from normal-temperature heat (NATURE, vol. xvii. p. 31, and vol. xvii. p. 20). There is one qualification (not affecting the reasoning in my papers) I should like to make here, viz., the case there dealt with does not appear necessarily to be out of harmony with what is termed the "second law of thermodynamics," though it may be questioned whether it quite harmonises with certain modes of stating the law. As the *facts* are of course the most important, it may perhaps be well just briefly to recapitulate here under what conditions normal-temperature heat can be converted into work, as affecting the problem dealt with.

2. Firstly, we observe that the diffusion of matter (gases) enables us to derive work solely at the expense of normal-temperature heat (*i.e.*, without an artificially-produced fall of temperature or a source and a refrigerator); and through this process a mass of gas may (by unchanged total volume) acquire a capacity for work without the performance of work. I say that the work is derived *solely* at the expense of normal temperature heat, because, although the mixing or diffusion is a necessary accident to the derivation of the work, it is in no way the *source* of the work, or the heat lost is the exact mechanical equivalent of the work derived, and the diffusion has nothing to do with furnishing the work. Still the system, after diffusion, is evidently not restored to its original state in all respects, *except the transference of heat*, although the passage of the system from its original to its final state did not furnish any of the work. To violate the second law of thermodynamics, the system would require to be restored to its original state, and if a method could be discovered for doing this, clearly no work would be theoretically required merely to effect this result (since the total volume of the gases is unchanged by the mixing).

3. It would seem scarcely to be brought into sufficient prominence in connection with the statement of the "second law of thermodynamics," how far it is possible (without a source or refrigerator) to convert normal-temperature heat into work, which is really the practical point; for there are many cases where it is of no consequence to us whether the matter from which the heat is derived is mixed or not. The great point is to derive the heat or convert it into useful mechanical energy. This is what it was my main object to treat of in the two papers referred to.

4. No doubt in nature normal-temperature heat is thus largely utilised or converted into work. For in the functions of plants and animals, two gases of different molecular weights, oxygen and carbonic acid, are largely concerned, and animal and vegetable structures are notably *porous*, so that no doubt normal-temperature heat may be converted into work through diffusion in this way—*i.e.*, by the different rates of diffusion of the two gases across the porous tissues. Indeed this principle might conceivably have more to do with animal and vegetable functions than is imagined. There are also mixtures of gases used in industrial operations, such as for explosion in gas-engines, &c. No doubt in such cases before exploding the gases, normal-temperature heat might be converted into work through diffusion (by means of a porous diaphragm connected with suitable machinery, in the manner roughly sketched), and the utility of the process would depend simply on the quantity of gas at disposal.

5. In regard to the means afforded by a porous diaphragm for (as it were) manipulating molecules and sifting them according to their velocities; no doubt this (as Prof. A. S. Herschel remarks) somewhat resembles the functions performed by the ideal being or "sprite" described by Prof. Maxwell in his "Theory of Heat," but it does not quite attain that result, for the diaphragm can only effect an unequal distribution of energy *combined with* an unequal distribution of *matter*, so that the

second law of thermodynamics is not violated, as it would be if the diaphragm effected *only* an unequal distribution of *energy*. The great point, however, to notice is that the sifting power of the diaphragm enables us to derive work from the gas at the expense of its heat, or we obtain thereby a capacity for work without the performance of work, which is the *practical* result we require, and so long as we obtain this result, we may not care so much about any inquiry whether a certain statement of a law is thereby violated or not (at least this inquiry is of *secondary* importance). The main point evidently is to realise how work may be derived from normal temperature heat without a source or refrigerator. Also it cannot surely be kept too much in view that the "second law of thermodynamics" is not *theoretically* a necessary truth, but its truth only depends (as Prof. Maxwell showed) on our inability to grasp or handle molecules. For if molecules were of such a size that we could handle them separately, then there is no doubt that we could transfer their motions to masses (without the necessity for mixing the molecules of different masses together). The attempts to prove the second law of thermodynamics as an abstract truth independently of considering the *molecular* state of matter, can therefore scarcely be considered as legitimate, as it is upon the *molecular* state of matter that the impracticability of the effect expressed in the law depends. Also it would be, perhaps, difficult to give a perfectly satisfactory *à priori* proof that no process can be discovered for utilising normal temperature heat without permanently mixing or altering the distribution of the matter concerned, more especially when it is considered how much can be already done in the way of manipulating molecules (or sifting their velocities) and deriving their heat by means of porous diaphragms in the case of diffusion. The practicability of the result would admittedly not be contrary to the principle of the conservation of energy.

6. One of the most important considerations, perhaps, connected with diffusion, would appear to be that the tendency to the uniform diffusion of matter, or rather of *velocity* [since chemically different molecules of *equal* mass do not necessarily tend to become uniformly diffused], can upset the tendency to the uniform diffusion of energy, *i.e.*, energy could not be uniformly diffused until matter (capable of diffusion) was also uniformly diffused, or homogeneous. Another important consideration would appear to be (and which, if noticed, would seem to be worthy of greater attention) that the gases of the atmosphere from the fact of their being of *different* molecular weights, tend *forcibly* to become uniformly diffused, or the danger of unequal mixture is averted, which would inevitably occur sometime or somewhere, if the gases were of the *same* molecular weights (or *dynamically* alike), and so diffusion were left to the pure contingencies of chance. I may return (by permission of the Editor) to this point at a future opportunity.

S. TOLVER PRESTON

"Underground Temperature"

THE Report of the British Association Committee on Underground Temperature appearing in NATURE, vol. xvii. p. 476, gives me an opportunity of questioning the treatment of the matter and urging the rejection of any figures obtained. This opportunity I had often wished for when reading over the allusions to underground temperature which spoil text-books, but would scarcely avail myself of now were not the "report" apparently "accepted" in significant silence.

To any one familiar with the state of circumstances down in mines, who has accumulated thought on the question of the temperature of the rocks in depth, the observations noted in this report must appear, not to say absolutely inadequate to further the inquiry, but altogether missing the point of it—that is to say, if I am right as I take it, that the purpose of the Committee is to ascertain, not mere "underground temperature" readings, but the proper temperature of the rocks as due to intra-terrestrial conditions. In all the observations conducted in mines, the temperature of the mine ventilation or a temperature almost wholly inter-dependent is expressed by the figures obtained; these figures no more indicate the true state of the case, and are therefore of no more value to the geologist or the general physicist, than the temperature of a greenhouse would assist the meteorologist. The figures obtained in bores express the temperature of the waters standing therein—which temperature cannot at all be assumed to be coincident with the rock temperature; these figures are determined by a variety of factors, the true rock temperature not being necessarily the greater. In fact, we can never arrive at the temperature of the rocks in depth through the media of water or air. To state this truth is to establish it.

A few remarks, however, bearing directly on the details of the observations in the report, may aid inquirers to arrive at a true state of thought on the subject. In the first place, and as of general application, I have to demand attention to the matter of the mine area, to depths exceeding the observation stations, being depleted of the waters naturally appertaining thereto—and for this reason even a rock temperature ascertained over such an area would be abnormal. A thermometer hung up in a mine way will unquestionably register the temperature of the ventilation in that particular place; nobody expects any other result. Can the temperature of the ventilation be demonstrated to be inter-dependent in a great measure on the temperature of the surrounding rocks, which is itself abnormal as above submitted? Certainly not! and this fact is so obvious to those having a true acquaintance with mine ventilation, that it seems to me too absurd to elaborate proofs of it. Particular stress is laid on the point that currents of air and well-aired situations were avoided. It is hard to see any outcome of this other than still more abnormal readings. Surely observers do not imagine they can penetrate the rocks so far by cornering in mines as to leave the atmosphere wholly behind. Figures obtained in still air express the temperature of dying ventilation, or of gases of exudation, or partly of both. As to the employment of a few inches of water in a hole, as supposed to secure more direct contact with the rocks, or to isolate from air; the temperature of the small body of water so employed is that obtained, and it is obvious it depends on the air temperature, and (worse) the water may possibly be decomposing. These remarks apply, for the most part, alike to the ingenious method employed down Boldon Colliery as to the more simple method of Schemnitz. Then, with regard to the Boldon Colliery observations in particular, the curious in these matters will be led to speculate as to what was going on up the ten-foot hole, and whether the "stagnant" ventilation of the district was not tending towards the explosive conditions. All the circumstances previously set down, supplemented by the exudation of gases, and the subsidence of strata following on coal working, combine to render this one of the unhappiest conceivable situations for the research on rock temperature.

A thermometer down a mine is of no utility beyond qualifying barometric readings.

The bore observations are, I venture to think, a crude phase of the method which may lead to success ultimately. As hitherto conducted, they are open to many obvious objections, which, if stated at length, would be little more than a reiteration of the above in part.

I will venture to suggest that the next steps in advance be the permanent placing of instruments in deep bores, broken rock being rammed over as over powder in blasting operations, so that all water and air, except such as may be fairly considered as entering into the structure of the rocks, be entirely excluded, and the application of thermo-electric apparatus devised by specialists in electric science, all constituting a special, and, for many reasons, invaluable attachment to an observatory or kindred institution.

WILLIAM MORRIS

Earlshill Colliery, Thurles

Helmholtz's Vowel Theory and the Phonograph

THE results obtained by Messrs. Jenkin and Ewing in their experiments with the phonograph, as described in NATURE, vol. xvii. p. 384, are so different from those reached in some experiments recently performed by Dr. Clarence J. Blake, of this city, in connection with myself, that I venture to call attention to the fact.

With the design of testing the question of change of quality in vowel tones by increasing the rate of rotation of the phonograph cylinder, we performed a number of experiments, of which I mention a few as briefly as possible.

1. The vowels *ou* and *ō* were spoken into the mouth-piece of the instrument, each four times in succession, while the cylinder was rotated at the rate of one revolution per second, as timed by the beats of a clock-pendulum. On rotating the disc so as to reproduce the vowel-sounds, these were as spoken, *ou*, *o*, each repeated four times, when the rate of rotation was one revolution per second, but on increasing the velocity to two revolutions per second, the first sounds were indistinct, while the last gave the

vowel ĕ very clearly. At half revolution per second, *ou*, *au*, were distinctly heard.

2. The vowel ō was sung while the cylinder rotated at different rates of speed. On reproducing the sounds, the cylinder being revolved more slowly than at first, the vowel *au* was heard, changing to ō, ĕ, ĭ, falling to ĕ again as the velocity was slackened a little.

3. The vowel ä was spoken while the cylinder made one revolution per second. On reproducing the sound, the rate being half a revolution per second, *au* was heard, changing to ä when the rate increased to one revolution, and at three revolutions per second ĭ was heard.

4. The vowel ō was spoken several times in succession, the rate of the cylinder being gradually accelerated. On reproducing the sound by a uniform and slow rotation, *au* and *ou* were heard; on rotating faster, ĕ and ĭ.

Several other experiments were tried in the short time during which the instrument was at our service, all of which were strikingly confirmatory of Helmholtz's theory. Difficulty was experienced in reproducing the highest vowels ē, ĭ, probably on account of want of readiness of response in the disc. The bell of a reed-pipe was placed over the mouth-piece of the instrument when the sound was to be reproduced, for which a horn of pasteboard was substituted in some of the trials.

We hope to render these experiments more rigorously quantitative, as the phonograph promises to be a valuable aid to research in this field. Very probably others may have worked with the same end in view, and if so it would be interesting to learn what has been their experience. CHAS. R. CROSS

Boston, U.S., April 29

The Telephone

WITH reference to the letter of Lieut. Savage which appeared in your last impression (p. 77) respecting the telephone, this gentleman has noticed that on removing the ferrotype disc of the sending instrument and tapping the magnet with a diamagnetic body, such as a piece of copper, the taps are distinctly heard at the receiving end. I have repeated this experiment. Not only can a diamagnetic substance be used for tapping, but the magnet may be removed altogether and a bar of soft iron substituted without causing any material difference in the results, and this bar of soft iron may be placed at right-angles to the line of dip. The vibrations of a tuning-fork are transmitted very distinctly. When held in the line of dip the results obained are more marked. Taps and the tuning-fork vibrations are readily heard, and by covering with the ferrotype disc a conversation was actually carried on through this bar of soft iron. There is perhaps nothing very surprising in obtaining these phenomena with the bar in the "dip" line, but when the same bar of perfectly soft and recently annealed iron can be held in any position in a plane at right-angles to that line and used as a sender for powerful vibrations, such as those of a tuning-fork or the taps of a diamagnetic body on the naked end of the bar, we cannot but be struck by the surprising delicacy of the telephone as a test for the earth's magnetism.

The receiving instrument used in the abve experiments was an ordinary bell telephone 2½ in. disc ·007 of an inch in thickness.

9, St. John's Road, Bristol, May 18 ALFRED CHIDDEY

Hereditary Transmission

IN 1837, Capt. D'Urban of H.M.S. *Griffin*, having captured, off the south coast of Martinique, a Portuguese slaver, called the *Don Francisco*, landed in this colony the living freight of 437 human beings, who, about two months previously, had been forced from their homes on the banks of the Congo, to be sold in Cuba.

William Laidlaw, one of the liberated slaves, who is now in a position of some trust on the Goodwill sugar plantation in the island, gives to me the following interesting details of hereditary transmission in the African, which I believe will be interesting to the readers of NATURE.

"I am about sixty or sixty-five years of age, and was born with six fingers on each hand. Soon after 'my freedom' I married a woman from 'our country.' We had four children, two being boys and two girls; they were born with six fingers on each hand, and one of the girls had six toes on each foot.

"My eldest son Robert, who is married and settled in Demerara, is the father of two boys, who have six fingers on each hand. My second son, William, who is working with me on the Goodwill estate, married, and his wife had five children; they were born having the same peculiarity; but I regret to say none are living."

I yesterday sent for William Laidlaw, and have substantiated his father's statements. I measured the sixth fingers: the one on the right hand is exactly 1¼ inch in length, and has a perfectly formed nail, the one on the left showed traces of having been partially amputated. EDMUND WATT

Resident District Magistrate

Dominica, British West Indies, April 27

What is a "Water-shed"?

SOME time ago the term "water-shed" was somewhat vaguely used to imply either the dividing ridge between two river basins or the slopes down which the water poured into the rivers themselves. Latterly, if I mistake not, it has generally been used by geographers in the former sense only. Mr. George Grove, F.R.G.S., however, in his excellent little Primer on Geography, uses the term "water-parting" for the ridge, and water-shed for the whole of the ground between the water-parting and the stream;—very clearly illustrating his meaning by reference to the ridge tiles and the slope of the roof of a house respectively.

There may be some reason, especially in a work of the kind, for substituting "water-parting" for "water-shed," in the sense first quoted, but is the use of the latter, to indicate the flow of water down the slopes, justified either by etymology, or even by the correct use of the word "shed" in ordinary conversation?

The derivation from Anglo-Saxon *sced-an* or *scead-an*, indicates the primary meaning to *divide* or *sever*. It is also used metaphorically in some of the north-country dialects, as "there is no *shed* (difference) between us." No doubt, by a very natural ellipsis it often implies flowing or falling. A woman sheds tears, or a tree sheds its leaves, and the consequent flowing down the cheeks, or fluttering down to the ground need not be specially expressed. But in this case the word is used distinctively, and should surely be used, if used at all, in its stricter and primary sense, while the fall or flow of water can be appropriately distinguished.

Of course this is merely a question of terminology, but I think it is one worth noticing if only for the sake of the youthful millions who are being brought to some knowledge of elementary geography, and will hardly be helped to appreciate the exactness of science if they find the same word is used by different authorities to describe things so different as the dividing ridge and the hill slopes of the land they live in. R. H.

Savile Club, Savile Row, W.

Abnormal Coccyx

IN NATURE for September 21, 1876, I gave an account of a peculiar abnormality in a girl aged eight, in whom the coccyx was turned backwards and upwards, and a little above it there was a circular depression in the skin, about ⅛ inch in diameter, and about ⅛ inch deep. On being dragged downwards the skin in this hollow became everted and formed a covering to the point of the coccyx. Shortly afterwards I had an opportunity of examining the other children of the family, with the following results:—

Boy aged six, normal.

Girl aged four, depression in the same spot as in the eldest sister, coccyx normal.

Girl aged two, normal.

Boy aged seven months, fairly deep hole (not measured) in same position, coccyx less curved forward than usual.

The parents were said not to possess this peculiarity; I could get no information as to the other members of the family.

A few days ago I met with another case of the same kind in a boy eight months old. The coccyx was curved sharply backwards, and there was a circular depression in the skin, about 5 mm. in diameter, a little higher up than in the other cases, which was easily raised to the level of the surrounding parts, and effaced by a little traction. ANDREW DUNLOP

Jersey

Lecture Experiment

A glass flask of about a litre capacity is partially filled with water and closed with a cork, through which a tube passes

which terminates flush with the lower side of the cork. Above, the tube is bent twice at right angles, the other extremity of the tube dipping below the surface of water of ordinary temperature.

The water of the flask is now boiled, and as soon as the air has been driven from the flask remove the flame and allow the water of the vessel to recede into the flask. At the first entry of the cool water the steam will be so greatly condensed that a brisk ebullition will take place, which for a few seconds checks the inflow of the water, driving it down the tube; further cooling quickly causes more water to enter, when the same phenomenon is repeated. After two or three oscillations of this kind the water runs continuously, and with great velocity, into the flask, which should not be allowed to fill, as it is in that case usually broken by the shock, which terminates the experiment.

FRANCIS E. NIPHER

St. Louis, April 12

Sound-emitting Crustaceans

IN an article in NATURE, vol. xviii. p. 53, you say: "Everybody who had searched for animals on coral-reefs, or had dredged in tropical seas, was familiar with the 'clicking' sounds emitted by the *Alphei* and their allies."

Those who wish to hear this sound need not go to coral reefs, or tropical seas—as the shores of Guernsey, Herm, or the other Channel Islands, produce *Alpheus ruber* and other *Alphei* in abundance.

Keeping them as I do in aquaria, it is startling sometimes in the evening to hear the loud snap, produced by sharply striking together the two claws on the larger leg.

May 10 H. STUART WORTLEY

GEOGRAPHICAL NOTES

AFTER the suppression of the Mahometan rebellion in the Chinese province of Yünnan, a number of the so-called Panthays took refuge in British Burmah to avoid the indiscriminate cruelty of their conquerors; but they have recently migrated, apparently *en masse*, to another region. This, we gather from a Rangoon paper, is a tract of country on the north-east of Upper Burmah, which belongs neither to the Siamese nor the Burmese, and over which the Chinese have never pretended to exercise any authority. This district is ruled over by a number of Shan and Kachyen chieftains, some of whom were at first inclined to oppose the Panthay settlement, but have ceased to make any opposition to it. The immigrants are said to be nearly 3,000 in number, and are divided into two settlements about ten miles apart. They have intermarried with the women of the country, and in course of time will, no doubt, form a considerable community among these savage tribes. Their principal occupation is agriculture, though a few of them have taken small quantities of goods from Mandalay, and have laid the foundation of a trade with the surrounding tribes. These Panthays, it seems, prefer the rude independence of their colony in the wilds to settling in either Upper or British Burmah.

NOTWITHSTANDING the embarrassed position of Russia at the present time, there seems to be no falling off in the exemplary activity of the Russian Geographical Society; indeed, it is well known that while its researches in Asia are of high scientific value, they are also not without political utility, and perhaps significance. The April meeting, the official abstract report of which is just to hand, was Asiatic all over. It was reported that the expedition to explore the divide between the waters of the Obi and Yenessei had set out on March 12, and that M. Smirnow was to set out on April 15 for the Petchora, to spend the summer in investigating the magnetic elements. The Society has projected two other expeditions for this summer, one of an ethnographical character in European Russia, the other purely geographical to Mongolia. M. Potanin, who had just returned, gave a summary of the results of his explorations in the Altai regions, some details of which we have already given. For the first time we have something like an adequate account of the extent, the offshoots, the physical geography, and the ethnology of the Altai region. The second part of the third volume of the results of the Siberian expedition of the Society has been published, and contains a study, by Prof. Oswald Heer, of the flora of the jurassic beds of the government of Irkutsk and the region of the Amoor. The eighth volume of the *Memoirs* of the Society, also recently published, ought to interest ethnologists, containing as it does a large collection of information on "customary law" as it exists in various districts of Russia and among some of the tribes on her borders.

THE *Times* Paris correspondent states that according to the German papers Hermann Soyaux, the botanist of the German Expedition to the Loango coast, 1873-76, will set out in July or August on another expedition to equatorial Western Africa to explore the Gabun and Ogovai country in the interests of natural science, and at the same time, under the patronage of the Hamburg firm of Wörmann, to make experiments with a view to the starting of plantations. A long account of Herr Soyaux's travels in Loango and Angola is about to appear, published by Messrs. Brockhaus.

TECHNICAL EDUCATION IN UNIVERSITY COLLEGE, LONDON

IN November, 1876, a short paragraph was inserted in NATURE (vol. xv. p. 69) which contained a notice of the commencement of technical teaching at University College in connection with the classes of mathematics, physics, engineering, and drawing. It may be of interest to state what progress has been made in the workroom up to the present time under rather unfavourable conditions. This we endeavour to do after a recent visit paid at the request of M. Robin, M.Sc., the able and painstaking superintendent of the department, under the direction of the professorial staff. At present the workroom is open on each week-day from ten to five, except on Saturdays, when it is closed at two. The superintendent is present from ten to three on three of the days. Students, who make use of the room, may choose their own hours for work.

Following the order indicated in the syllabus, we first examined the models in the mathematical section. Here we were specially interested in the models illustrative of most of the propositions of modern geometry; pencils of planes and of lines (to show the simple contrivances employed, we may say these models were made of knitting-needles with small spherical ends of sealing-wax of different colours, thus enabling the student to see their different directions; in other cases joins were indicated by ties of differently coloured wool, thus allowing motion to the figures, as in a model showing that the corresponding points of two perspective triangles meet in a line). Projective rows of points made of pricked wood, the corresponding points joined by india-rubber threads; models exhibiting the generation of ruled surfaces of the second order, movable models made of silk threads stretched by weights, parallel pencils of lines making the paraboloid. The generation of curves by the intersection of pencils of lines; this was shown by two flat pencils of lines made of coloured silk in mahogany frames, one of which moved upon the other; at the intersection of certain pairs of threads were placed small indices which clearly showed to the eye various forms of ellipses and hyperbolas. This model we remember attracted considerable attention at the *conversazione* in June last, whilst Prof. Henrici was manipulating it so as to give the curves named. Curves are also produced, whose forms are shown by the aid of sawdust or of sand scattered on a glass plate; these were mostly got as envelopes. In this department, also, are several models of linkages giving approximate and true straight lines, illustrating the dis-

coveries of Watt, Peaucellier, Tchebycheff, Sylvester, Hart, and Kempe; also examples of Sylvester's and Kempe's isoclinostats. These last models are made, some in zinc, but most of them of printer's rules, the articulation being effected by brass pipes. We close our recollections of this section with the bare mention of an ingenious application of the zoetrope to illustrate certain mechanical combinations in motion.

In the engineering department was a collection of elementary plane mechanisms as described in Prof. Kennedy's translation of Reuleaux's "Kinematics": links, made as before of printers' rules; cylinder-pair of brass pipes, prism-pair of wood and brass allowing *any* joint to be fixed, and a complete motion, all the motion truly complane; also a contrivance used for fixing a link when its plane of motion is between two others. Among instances of other mechanisms given in Reuleaux, we noted a duangle moving in a triangle, triangle in a square, &c.; models of glass, centroids stuck on them, so making the conception clear; several examples of spherical quadrilaterals made of zinc. Here also we mention the model of a steam-engine excentric, showing the reversion of motion.

Among matters in preparation we examined with interest a pendulum apparatus which presented some novel features, but it would take too much space to dwell further upon what we saw under the courteous guidance of M. Robin. What we have said—and here we acknowledge our indebtedness to the superintendent's clear exposition, and to a short account of some of the models given in "Engineering" for June 15, 1877, descriptive of the scientific objects exhibited at the professor's *conversazione*—will sufficiently indicate the way that has been made in the short space of one year, under the careful supervision of the professors. Very many of the models are the work of the students. We had not time to examine the physical section, which is devoted to the construction of simple physical apparatus.

THE SETTLE CAVE EXPLORATION

THE Settle Cave Exploration Committee have again nearly exhausted the funds at their disposal, and are preparing a statement of accounts and a fresh appeal to the public.

The great thickness of beds already excavated in the Victoria Cave has taken us down so far into the past that it would be a thousand pities to close the work prematurely before getting down to the cavern floor. But unless the Committee receive help soon they will be obliged to stop and leave the rest of the cave's history in obscurity. We have appealed to our readers in behalf of this important undertaking before, and we are sure we shall not do so in vain again. There must be many readers of NATURE interested to see the final results of the exploration of these interesting caves, and who are at the same time able and willing to give substantial help to the Committee. We are sure the smallest contribution will be thankfully received, and we trust the Committee will, without delay, be encouraged and enabled to continue their researches. The Treasurer, Mr. John Birkbeck, junr., will receive donations at the Craven Bank, Settle, Yorkshire.

ORGANISATION OF FRENCH METEOROLOGY

ON May 7 the Academy of Sciences adopted in its secret meeting the draft of a letter to M. Bardoux, the Minister for Public Instruction, asking him to state whether he was to establish a separate administration for meteorology or continue the existing system. The answer to this was considered as an essential preliminary to the selection of the candidates for the directorship of the Paris Observatory. The candidates have not been nominated yet, but a definite answer has been given to the Academy of Sciences. The decree organising the meteorological division of the observatory into a distinct service was signed by the president on the 13th and gazetted on the 14th. It is prefaced by a summary of the several steps taken by Leverrier (whose name has been carefully omitted) to give the meteorological organisation its existing form.

The decree can hardly be considered as an innovation, and may be more aptly termed a resuscitation of a former stage in the evolution of official meteorology in France. In 1864 Leverrier had under him three subordinates: one was the head of the warning department; M. Rayet, the head of that which investigated general movements of the atmosphere; and the third M. Sonrel, the head of meteorological stations, their inspector, and general computer. M. Sonrel having died and M. Rayet having resigned, their offices were suppressed for the sake of economy. The whole of the work was executed on a reduced scale by subordinates. M. Bardoux has recalled into existence these two services, which are styled "Study of the General Movements of the Atmosphere" and "Climatology with Inspection of Meteorological Stations." The head of these two services will be a meteorologist, and one of the three meteorologists will be appointed director of the Central Bureau.

This appointment will not take place immediately, as the advice of a special council of the Central Bureau must be taken by the Minister. This council will be formed of members of the Institute and the large public administrative departments—Telegraphy, Admiralty, Public Instruction, War Office, &c., connected in any way with meteorology.

The greatest innovation is the authority given to the Central Bureau over the several meteorological observatories which have been established or will be established in the various districts of France, either at the expense of Government, of departments, or of townships. The more important of these observatories now in existence are Montsouris (Paris), Lyons, Puy-de-Dôme, and Pic du Midi. It is not stated whether the decree will extend to Algerian observatories, which publish a special journal and have their observations taken with a special system.

The Bureau Central will be in direct communication with the departmental commissions which M. Leverrier has established in almost every department. Each of these commissions will have the control of the agricultural stations in its own district. If the president requires it, he will receive *franco* a daily telegram to help him to issue special warnings, as the practice is daily gaining ground. In many parts of France departmental commissions have been grouped into regional organisations according to the initiative taken by Leverrier. The directors of regional meteorological observatories, delegates of regional commissions, and delegates of the Meteorological Society of France, meet once a year to deliberate on topics of interest for the progress of meteorology. The Meteorological Society is a free society supported by voluntary contributions. It is the first time that such a body has been endowed with official privileges.

The departmental commissions, although established mostly by the prefect and the local engineers of the Ponts-et-Chaussées, are supported by private exertions and contributions, as well as donations from departments and townships. M. Leverrier established an observatory in each normal school in France (there is one normal school in each department). All these observatories are to be visited by the delegates of the Bureau Central and their observations published by it. These normal school observatories will issue warnings for their localities. Some of them have already begun.

The Champ de Mars meteorological pavilion contains a number of valuable documents already sent by normal schools and departmental commissions, whose exertions will be regulated under the new system.

COMPOSITE PORTRAITS[1]

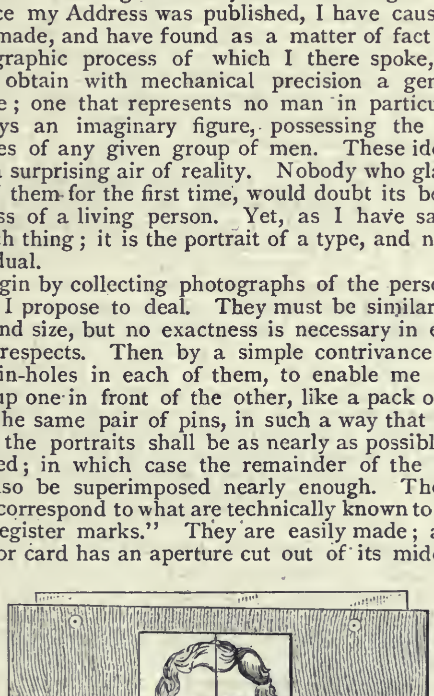
I SUBMIT to the Anthropological Institute my first results in carrying out a process that I suggested last August in my Presidential Address to the Anthropological Subsection of the British Association at Plymouth, in the following words:—

"Having obtained drawings or photographs of several persons alike in most respects, but differing in minor details, what sure method is there of extracting the typical characteristics from them? I may mention a plan which had occurred both to Mr. Herbert Spencer and myself, the principle of which is to superimpose optically the various drawings and to accept the aggregate result. Mr. Spencer suggested to me in conversation that the drawings reduced to the same scale might be traced on separate pieces of transparent paper and secured one upon another, and then held between the eye and the light. I have attempted this with some success. My own idea was to throw faint images of the several portraits, in succession, upon the same sensitised photographic plate. I may add that it is perfectly easy to superimpose optically two portraits by means of a stereoscope, and that a person who is used to handle instruments will find a common double eyeglass fitted with stereoscopic lenses to be almost as effectual and far handier than the boxes sold in shops."

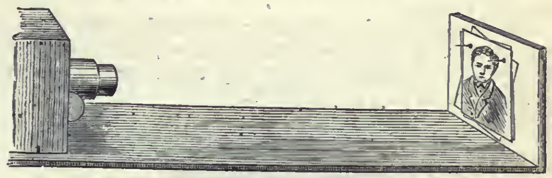
Mr. Spencer, as he informed me, had actually devised an instrument many years ago, for tracing mechanically, longitudinal, transverse, and horizontal sections of heads on transparent paper, intending to superimpose them and to obtain an average result by transmitted light.

Since my Address was published, I have caused trials to be made, and have found as a matter of fact that the photographic process of which I there spoke, enables us to obtain with mechanical precision a generalised picture; one that represents no man in particular, but portrays an imaginary figure, possessing the average features of any given group of men. These ideal faces have a surprising air of reality. Nobody who glanced at one of them for the first time, would doubt its being the likeness of a living person. Yet, as I have said, it is no such thing; it is the portrait of a type, and not of an individual.

I begin by collecting photographs of the persons with whom I propose to deal. They must be similar in attitude and size, but no exactness is necessary in either of these respects. Then by a simple contrivance I make two pin-holes in each of them, to enable me to hang them up one in front of the other, like a pack of cards, upon the same pair of pins, in such a way that the eyes of all the portraits shall be as nearly as possible superimposed; in which case the remainder of the features will also be superimposed nearly enough. These pin-holes correspond to what are technically known to printers as "register marks." They are easily made; a slip of brass or card has an aperture cut out of its middle, and



threads are stretched from opposite sides, making a cross. Two small holes are drilled in the plate, one on either side of the aperture. The slip of brass is laid on the portrait with the aperture over its face. It is turned about until one of the cross threads cuts the pupils of both the eyes, and it is further adjusted until the other thread divides the interval between the pupils in two equal parts. Then it is held firmly, and a prick is made through each of the holes. The portraits being thus arranged, a photographic camera is directed upon them. Suppose there are eight portraits in the pack, and that under existing circumstances it would require an exposure of eighty seconds to give an exact photographic copy of any one of them. The general principle of proceeding is this, subject in practice to some variation of details, depending on the different brightness of the several portraits. We throw the image of each of the eight portraits in turn upon the same part of the sensitised plate for ten seconds. Thus, portrait No. 1 is in the front of the pack; we take



the cap off the object-glass of the camera for ten seconds, and afterwards replace it. We then remove No. 1 from the pins, and No. 2 appears in the front; we take off the cap a second time for ten seconds, and again replace it. Next we remove No. 2, and No. 3 appears in the front, which we treat as its predecessors, and so we go on to the last of the pack. The sensitised plate will now have had its total exposure of eighty seconds; it is then developed, and the print taken from it is the generalised picture of which I speak. It is a composite of eight component portraits. Those of its outlines are sharpest and darkest that are common to the largest number of the components; the purely individual peculiarities leave little or no visible trace. The latter being necessarily disposed equally on both sides of the average, the outline of the composite is the average of all the components. It is a band, and not a fine line, because the outlines of the components are seldom exactly superimposed. The band will be darkest in its middle whenever the component portraits have the same general type of features, and its breadth or amount of blur will measure the tendency of the components to deviate from the common type. This is so for the very same reason that the shot-marks on a target are more thickly disposed near the bulls-eye than away from it, and in a greater degree as the marksmen are more skilful. All that has been said of the outlines is equally true as regards the shadows; the result being that the composite represents an averaged figure, whose lineaments have been softly drawn. The eyes come out with appropriate distinctness, owing to the mechanical conditions under which the components were hung.

A composite portrait represents the picture that would rise before the mind's eye of a man who had the gift of pictorial imagination in an exalted degree. But the imaginative power even of the highest artists is far from precise, and is so apt to be biased by special cases that may have struck their fancies, that no two artists agree in any of their typical forms. The merit of the photographic composite is its mechanical precision, being subject to no errors beyond those incidental to all photographic productions.

I submit several composites made for me by Mr. H. Reynolds. The first set of portraits are those of criminals convicted of murder, manslaughter, or robbery accompanied with violence. It will be observed that the features of the composites are much better looking than those of the components. The special villainous irregu-

[1] Made by combining those of many different persons into a single resultant figure. By Francis Galton, F.R.S. Paper read before the Anthropological Institute, April 30.

larities in the latter have disappeared and the common humanity that underlies them has prevailed. They represent, not the criminal, but the man who is liable to fall into crime. All composites are better looking than their components, because the averaged portrait of many persons is free from the irregularities that variously blemish the looks of each of them. I selected these for my first trials because I happened to possess a large collection of photographs of criminals through the kindness of Sir Edmund Du Cane, the Director-General of Prisons, for the purpose of investigating criminal types. They were peculiarly adapted to my present purpose, being all made of about the same size and taken in much the same attitudes. It was while endeavouring to elicit the principal criminal types by methods of optical superimposition of the portraits, such as I had frequently employed with maps and meteorological traces,[1] that the idea of composite figures first occurred to me.

The other set of composites are made from pairs of components. They are selected to show the extraordinary facility of combining almost any two faces whose proportions are in any way similar.

The accompanying woodcut is as fair a representation of one of the composites as is practicable in ordinary printing. It was photographically transferred to the wood, and the engraver has used his best endeavour to translate the shades into line engraving. This composite is made out of only three components, and its three-fold origin is to be traced in the ears, and in the buttons to the vest. To the best of my judgment the original photograph is a very exact average of its components; not one feature in it appears identical with that of any one of them, but it contains a resemblance to all, and is not more like to one of them than to another. However the judgment of the wood engraver is different. His rendering of the composite has made it exactly like one of its components, which it must be borne in mind he had never seen. It is just as though an artist drawing a child had produced a portrait closely resembling its deceased father, having overlooked an equally strong likeness to its deceased mother, which was apparent to its relatives. This is to me a most striking proof that the composite is a true combination. [I trust that the beauty of the woodcut will not be much diminished by the necessarily coarse process of newspaper printing.]

It will, I am sure, surprise most persons to see how well defined these composites are. When we deal with faces of the same type, the points of similarity far outnumber those of dissimilarity, and there is a much greater resemblance between faces generally, than we who turn our attention to individual differences are apt to appreciate. A traveller on his first arrival among people of a race very different to his own, thinks them closely alike, and a Hindu has much difficulty in distinguishing one Englishman from another.

[1] "Conference at the Loan Exhibition of Scientific Instruments," 1878. Chapman and Hall. Physical Geography Section, p. 312, "On Means of Combining Various Data in Maps and Diagrams," by Francis Galton, F.R.S.

The fairness with which photographic composites represent their components is shown by six of the specimens. I wished to learn whether the order in which the components were photographed made any material difference in the result, so I had three of the portraits arranged successively in each of their six possible combinations. It will be observed that four at least of the six composites are closely alike. I should say that in each of this set the last of the three components was always allowed a longer exposure than the second, and the second than the first, but it is found better to allow an equal time to all of them.

The stereoscope, as I stated last August in my address at Plymouth, affords a very easy method of optically superimposing two portraits, and I have much pleasure in quoting the following letter, pointing out this fact as well as some other conclusions to which I also had arrived. The letter was kindly forwarded to me by Mr. Darwin; it is dated last November and was written to him by Mr. A. L. Austin from New Zealand, thus affording another of the many curious instances of two persons being independently engaged in the same novel inquiry at nearly the same time, and coming to similar results.

"*Invercargill, New Zealand, Nov.* 6, 1877.

"To Charles Darwin, Esq.

"SIR,—Although a perfect stranger to you, and living on the reverse side of the globe, I have taken the liberty of writing to you on a small discovery I have made in binocular vision in the stereoscope. I find by taking two ordinary *carte-de-visite* photos of two different persons' faces, the portraits being about the same sizes and looking about the same direction, and placing them in a stereoscope, the faces blend into one in a most remarkable manner, producing in the case of some ladies' portraits in every instance a *decided improvement* in beauty. The pictures were not taken in a binocular camera, and therefore do not stand out well, but by moving one or both until the eyes coincide in the stereoscope, the pictures blend perfectly. If taken in a binocular camera for the purpose, each person being taken on one half of the negative, I am sure the results would be still more striking. Perhaps something might be made of this in regard to the expression of emotions in man and the lower animals, &c. I have not time or opportunities to make experiments, but it seems to me something might be made of this by photographing the faces of different animals, different races of mankind, &c. I think a stereoscopic view of one of the ape tribe and some low caste human face would make a very curious mixture; also in the matter of crossing of animals and the resulting offspring. It seems to me something also might result in photos of husband and wife and children, &c. In any case the results are curious if it leads to nothing else. Should this come to anything you will no doubt acknowledge myself as suggesting the experiment and perhaps send me some of the results. If not likely to come to anything a reply would much oblige me.

"Yours very truly,

"A. L. AUSTIN, C.E., F.R.A.S."

Dr. Carpenter informs me that the late Mr. Appold, the mechanician, used to combine two portraits of himself, under the stereoscope. The one had been taken with an assumed stern expression, the other with a smile; and this combination produced a curious and effective blending of the two.

Convenient as the stereoscope is, owing to its accessibility, for determining whether any two portraits are suitable in size and attitude to form a good composite, it is nevertheless a makeshift and imperfect way of attaining the required result. It cannot of itself combine two images; it can only place them so that the office of attempting to combine them may be undertaken by the brain. Now the two separate impressions received by the brain through the stereoscope do not seem to me to be relatively constant in their vividness, but sometimes the image seen by the left eye prevails over that seen by the right, and *vice versâ*. All the other instruments I am about to describe accomplish that which the stereoscope fails to do; they create true optical combina-

tions. As regards other points in Mr. Austin's letter, I cannot think that the use of a binocular camera for taking the two portraits intended to be combined into one by the stereoscope would be of importance. All that is wanted is that the portraits should be nearly of the same size. In every other respect I cordially agree with Mr. Austin.

The best instrument I have as yet contrived and used for optical superimposition is a "double image prism" of Iceland spar. The latest that I have had were procured for me by Mr. Tisley, optician, 172, Brompton Road. They have a clear aperture of a square, half an inch in the side, and when held at right angles to the line of sight will separate the ordinary and extraordinary images to the amount of two inches, when the object viewed is held at seventeen inches from the eye. This is quite sufficient for working with *cartes-de-visite* portraits. One image is quite achromatic, the other shows a little colour. The divergence may be varied and adjusted by inclining the prism to the line of sight. By its means the ordinary image of one component is thrown upon the extraordinary image of the other, and the composite may

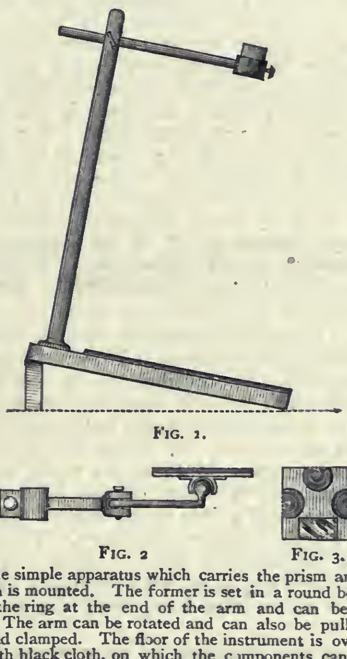


FIG. 1.

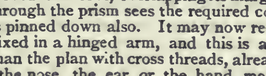


FIG. 2

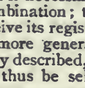


FIG. 3.

FIG. 1 shows the simple apparatus which carries the prism and on which the photograph is mounted. The former is set in a round box which can be rotated in the ring at the end of the arm and can be clamped when adjusted. The arm can be rotated and can also be pulled out or in if desired, and clamped. The floor of the instrument is overlaid with cork covered with black cloth, on which the components can easily be fixed by drawing pins. When using it one portrait is pinned down and the other is moved near to it, overlapping its margin if necessary, until the eye looking through the prism sees the required combination; then the second portrait is pinned down also. It may now receive its register-marks from needles fixed in a hinged arm, and this is a more generally applicable method than the plan with cross threads, already described, as any desired feature—the nose, the ear, or the hand, may thus be selected for composite purposes. Let A, B, C, . . . Y, Z, be the components. A is pinned down, and B, C, . . Y, Z, are successively combined with A, and registered. Then before removing Z, take away A and substitute any other of the already registered portraits, say B, by combining it with Z; lastly, remove Z and substitute A by combining it with B, and register it FIG. 2 shows one of three similarly-jointed arms, which clamp on to the vertical rod. Two of these carry a light frame covered with cork and cloth, and the other carries FIG. 3, which is a frame having lenses of different powers set into it, and on which, or on the third frame, a small mirror inclined at 45° may be laid. When a portrait requires foreshortening it can be pinned on one of these frames and be inclined to the line of sight; when it is smaller than its fellow it can be brought nearer to the eye and an appropriate lens interposed; when a right-sided profile has to be combined with a left-handed one, it must be pinned on one of the frames and viewed by reflection from the mirror in the other. The apparatus I have drawn is roughly made, and being chiefly of wood, is rather clumsy, but it acts well.

be viewed with the naked eye or through a lens of long focus, or through an opera-glass (a telescope is not so good) fitted with a sufficiently long draw-tube to see an object at that short distance with distinctness. Portraits of somewhat different sizes may be combined by placing the larger one further from the eye, and a long face may be fitted to a short one by inclining and foreshortening the former. The slight fault of focus thereby occasioned produces little or no sensible ill-effect on the appearance of the composite.

The front and profile faces of two living persons sitting side by side or one behind the other, can be easily superimposed by a double image prism. Two such prisms set one behind the other can be made to give four images of equal brightness, occupying the four corners of a rhombus, whose acute angles are 45°. Three prisms will give eight images; but this is practically not a good combination, the images fail in distinctness, and are too near together for use. Again, each lens of a stereoscope of long focus can have one or a pair of these prisms attached to it, and four or eight images may be thus combined.

Another instrument I have made, consists of a piece of glass inclined at a very acute angle to the line of sight, and of a mirror beyond it, also inclined, but in the opposite direction to the line of sight. Two rays of light will therefore reach the eye from each point of the glass; the one has been reflected from its surface, and the other has been first reflected from the mirror, and then transmitted through the glass. The glass used should be extremely thin, to avoid the blur due to double reflections; it may be a selected piece from those made to cover microscopic specimens. The principle of the instrument may be yet further developed by interposing additional pieces of glass successively less inclined to the line of sight, and each reflecting a different portrait.

I have tried many other plans; indeed, the possible methods of optically superimposing two or more images are very numerous. Thus I have used a sextant (with its telescope attached); also strips of mirrors placed at different angles and their several reflections simultaneously viewed through a telescope. I have also used a divided lens, like two stereoscopic lenses brought close together, in front of the object-glass of a telescope.

I have not yet had an opportunity of superimposing images by placing glass negatives in separate magic-lanthorns, all converging upon the same screen; but this or even a simple dioramic apparatus would be very suitable for exhibiting composite effects to an audience, and if the electric light were used for illumination the effect on the screen could be photographed at once. It would also be possible to construct a camera with a long focus, and many slightly divergent object glasses, each throwing an image of a separate glass negative upon the same sensitised plate.

The uses of composite portraits are many. They give us typical pictures of different races of men, if derived from a large number of individuals of those races taken at random. An assurance of the truth of any of our pictorial deductions is to be looked for in their substantial agreement when different batches of components have been dealt with, this being a perfect test of truth in all statistical conclusions. Again, we may select prevalent or strongly marked types from among the men of the same race, just as I have done with two of the types of criminals by which this memoir is illustrated.

Another use of this process is to obtain by photography a really good likeness of a living person. The inferiority of photographs to the best works of artists, so far as resemblance is concerned, lies in their catching no more than a single expression. If many photographs of a person were taken at different times, perhaps even years apart, their composite would possess that in which a single photograph is deficient. I have already pointed out the experience of Mr. Appold to this effect. The analytical tendency of the mind is so strong that out of any tangle of superimposed outlines it persists in dwelling preferably on some one of them, singling it out and taking little heed of the rest. On

one occasion it will select one outline, on another a different one. Looking at the patterns of the papered walls of our room we see, whenever our fancy is active, all kinds of forms and features; we often catch some strange combination which we are unable to recall on a subsequent occasion, while later still it may suddenly flash full upon us. A composite portrait would have much of this varied suggestiveness.

A further use of the process would be to produce from many independent portraits of an historical personage, the most probable likeness of him. Contemporaneous statues, medals, and gems would be very suitable for the purpose, photographs being taken of the same size, and a composite made from them. It will be borne in mind that it is perfectly easy to apportion different "weights" to the different components. Thus, if one statue be judged to be so much more worthy of reliance than another that it ought to receive double consideration in the composite, all that is necessary is to double either the time of its exposure or its illumination.

The last use of the process that I shall mention is of great interest as regards inquiries into the hereditary transmission of features, as it enables us to compare the average features of the produce with those of the parentage. A composite of all the brothers and sisters in a large family would be an approximation to what the average of the produce would probably be if the family were indefinitely increased in number, but the approximation would be closer if we also took into consideration those of the cousins who inherited the family likeness. As regards the parentage, it is by no means sufficient to take a composite of the two parents; the four grandparents and the uncles and aunts on both sides should be also included. Some statistical inquiries I published on the distribution of ability in families[1] give provisional data for determining the weight to be assigned in the composite to the several degrees of relationship. I should, however, not follow those figures in the present case, but would rather suggest for the earlier trials, first to give equal "weights" to the male and female sides; thus the father and a brother of the male parent would count equally with the father and a brother of the female parent. Secondly, I should "weight" each parent as 4, and each grandparent and each uncle and aunt as 1; again, I should weight each brother and sister as 4, and each of those cousins as 1 who inherited any part of the likeness of the family in question. The other cousins I should disregard. The weights as previously mentioned, would be bestowed by giving proportionate periods of exposure.[2]

Composites on this principle would undoubtedly aid the breeders of animals to judge of the results of any proposed union better than they are able to do at present, and in forcasting the results of marriages between men and women they would be of singular interest and instruction. Much might be learnt merely by the frequent use of the double image prism, as described above, which enables us to combine the features of living individuals when sitting side by side into a single image.

I have as yet had few opportunities of developing the uses of the composite photographic process, it being difficult without much explanation to obtain the requisite components. Indeed, the main motive of my publishing these early results is to afford that explanation, and to enable me to procure a considerable variety of materials to work upon. I especially want sets of family photographs all as nearly as possible of the same size and taken in the same attitudes. The size I would suggest for family composites is that which gives one-half of an inch interval between the pupil of the eye and the line that separates the two lips. The attitudes about which there can be no mistake are: full face, an exact profile, say, always showing the right side of the face, and an exact three-quarters, always showing the left; in this, the outer edge of the right eye-lid will be only just in sight. In each case the sitter should look straight before him. Such portraits as these go well into *cartes de visite*, and I trust that not a few amateur photographers may be inclined to make sets of all the members of their family, young and old, and of both sexes, and to try composites of them on the principles I have described. The photographs used for that purpose need not be in the least injured, for the register marks may be made in the case into which they are slipped, and not in the photographs themselves.

[1] "Hereditary Genius," p. 317. Column D. Macmillan, 1869.

[2] Example:—There are 5 brothers or sisters and 5 cousins whose portraits are available; the total period of desired exposure is 100 seconds. $5 \times 4 + 5 = 25$; $\frac{100}{25} = 4$; which gives $4 \times 4 = 16$ seconds for each brother or sister, and 4 seconds for each cousin ($5 \times 16 + 5 \times 4 = 100$).

THE SEICHES OF THE LAKE OF GENEVA[1]

AMONG the best-established phenomena of terrestrial physics is that which from ancient times has been known at Geneva under the name of *seiches*. Its true nature has been only recently recognised, and the obscurity which still envelops its causes gives it an interest calculated to attract the attention of men of science of every country. It is not necessary to have a knowledge of science to recognise in the neighbourhood of Geneva sudden irregular changes of very varied amounts, which sometimes affect the level of the lake. They are analogous to small tides whose existence is well established, but whose origin is not determined. For eight years Prof. Forel has worked at the problem by studying the characteristics of the progress of the phenomenon. The following is a *résumé* of his observations and conclusions.

Previous to Prof. Forel, eminent naturalists, among them Jallabert, Bertrand, H. B. de Saussure, J. P. E. Vaucher, had pointed out the existence of these *seiches*, described them, and hazarded hypotheses to account for them, hypotheses which for the most part cannot be maintained. Former observations furnish some guiding points to examine the question. They were made mainly near Geneva, where the oscillations of the level of the lake are much more marked than anywhere else on its shores. It is, however, at a much less favourably situated point of the shore that M. Forel has undertaken his regular investigations. Morges, his usual residence, is situated on the north shore of the lake, opposite its greatest breadth, and a little to the east of the line dividing its total length into two equal parts. There, by means of ingenious instruments, the movements of the liquid surface have been registered with regularity and sagacity, and veritable discoveries have been the result of the work.

Vaucher, at the beginning of the century, had already surmised that *seiches* must exist in all lakes, that they take place at all seasons and at all hours of the day, that they are more frequent in spring and in autumn, more frequent especially when the atmosphere is subject to strong variations in pressure. He valued approximately their duration, and predicted their character of permanence, which assimilated them to the incessant oscillations of the fluid mass. He believed, as H. B. De Saussure announced in 1779, that "prompt and local variations in the weight of the air could contribute to the phenomenon and produce momentary fluxes and refluxes by causing unequal pressures on the different parts of the lake." The movement of the liquid will then be an oscillation of libration. Vaucher admitted that this oscillation was progressive. His conclusion did not appear justified; he did not take into account the rhythm of the *seiches*, which, in reality, shows itself with a remarkable regularity whatever be their amplitude, and the duration of which is connected with the dimensions of the lake in length and depth.

[1] Researches by Dr. F. A. Forel, Professor at the Academy of Lausanne. *Bulletins de la Section Vaudoise des Sciences Naturelles*, 1873 and 1875. *Arch. des Sc.*, 1874–76.

It is to elucidate this question that the experiments of M. Forel tend. They were referred to in NATURE, vol. xii. p. 134, and a paper on them was read at the Physical Society, May 27, 1876 (NATURE, vol. xiv. p. 164). Working with an artificial basin, then on several Swiss lakes, he has proved the constancy of the duration of the wave of libration; the increase of this duration with increase of length of the basin; its diminution with increase of depth. The character of the wave of libration is, that the water rises at one extremity of the basin while it sinks at the other, and *vice versâ*. As to the intensity of the wave it varies much, either under the influence of the mysterious cause which produces it, or according to the region of the basin where it extends. The *seiches* are much more pronounced at Geneva than in the broader part of the lake, either on account of the contraction of the mass of the water on the south-west shore, or of the considerable decrease in its depth.

If we examine the *seiches* at Morges, their theory becomes infinitely more complicated, because, besides the phenomenon of the longitudinal *seiche*, mainly visible at the two extremities of the lake, the changes of level present other periods in relation with the transverse libration of the liquid mass. They are also modified by oblique or cross reflections of waves in motion, which are connected with the form of the basin. We may only expect a real regularity in the form of the waves of libration if that form of the basin is regular.

In order to be able to base his conclusions on more varied observations, M. Forel has measured the duration of the *seiches* of seven other Swiss lakes, besides that of Geneva, and he has been able to draw the following inference:—The duration of the wave increases with the length of the basin, and diminishes in proportion as the depth increases. In other words, the rhythm of the longitudinal *seiches* is a direct function of the length of the different lakes and an inverse function of their depth. From the figures which have permitted the inference of this general law, we may, by neglecting the influence of the depth, approximately conclude the duration of a *seiche* corresponding to a lake of a given length; and reciprocally infer the probable kilometric length of a *seiche* whose duration is known. It will then be possible to infer the direction of any particular wave, among those which show themselves on the shore of the Lake of Geneva, for example. The above enunciation is then generalised, and is thus modified:—The duration of *seiches* is a direct function of the length and an inverse function of the depth of the section of the lake along which they oscillate.

M. Forel has found a mathematical formula relative to the movement of liquids in basins in the works of a Bâle *savant*, Dr. J. R. Mérian (1828), completing the formula given by Prof. Guthrie, and applicable to the movement of *seiches* (NATURE, vol. xv. p. 91). It may be stated thus:—The duration of *seiches* is proportional to the length of lakes and inversely proportional to the square root of their mean depth.

By experiments made in the first place on the Lake of Neuchâtel, it was found that the movements of libration of the water were alternate and simultaneous. The water rises at one of the extremities, while it falls at the other. The amplitude of these movements is very variable, but their constancy is proved, preserving the same rhythm; their cessation or their absence would be the abnormal fact.

These conclusions have been extended and confirmed in a remarkable manner since the establishment of automatic instruments, intended to take a graphic tracing of these phenomena. A pencil in constant connection with the level of the lake draws a line on an endless paper, which is unrolled by a clockwork movement. Following the forms of this line, we discern in a very exact manner the influence of the various waves of oscillation, whose amplitude may vary from a few millimetres to a metre and more, if it has to do with the extremity of the lake, near Geneva. At Morges the first model of this apparatus was set to work, named by its inventor "registering limnimetre;" the curves obtained are compounded of the actions of various categories of *seiches*. They are more or less difficult to discern, but ordinarily recognisable and in connection with the rhythm corresponding to each. At Sécheron, near Geneva, where M. Ph. Plantamour has had an apparatus of the same kind constructed, the longitudinal *seiche* shows itself in a much more sensible manner, corresponding to a duration of about seventy-three minutes. The vibrations which affect the level of the lake under the influence of the wind or the passing of steamers disturb here much less than at Morges the study of the rhythm of libration, and above all, the measurement of the amplitude of this libration, which is, as we have said, much more considerable than at Morges.

This second registering limnimetre has only been working regularly since June, 1877. It has already served to confirm the fact of the alternation of the movement of the water, which rises at Geneva, while it falls at Morges, and *vice versâ*, following the period of seventy-three minutes for the great longitudinal *seiche* of the lake, thus confirming the presumption of an oscillation of the liquid mass around a median line, normal to its length. Morges, being situated at a short distance to the east of this line, offers a movement of the water of very inferior extent to that of the terminal part of the lake near Geneva. It is desirable that a similar apparatus be set up at the eastern extremity of the lake, between Vevey and Villeneuve, with a barometer comparable to that at Geneva, and giving constant indications.

It is generally presumed, in fact, that it is to changes of pressure on various parts of the lake that the variations in the intensity of the oscillatory movement of the water are due. These variations are nearly always marked during stormy weather. Before the storm comes on, before even the barometric column is disturbed, the libration increases in amplitude. But concordant observations at the two ends of the surface which is agitated have not yet been made to furnish data to determine if an increase of pressure at one of the extremities of the lake coincides with a depression at the other. The point remains, meantime, very doubtful, and very worthy of being investigated.

As to the oscillations of exceptional amplitude, such as those of October 3, 1841, when the difference of level observed at Geneva exceeded 2·14 metres, the presumption was that we ought to seek for the cause in some movement of the earth's crust, of the basin of the mass of oscillating fluid. Great, then, was the anxiety of Swiss observers, when, on October 8, 1877, they were awakened at 5·16 A.M. by a strong shock of earthquake, on running to their limnimetre, which they found working very regularly. But no trace of the action of that violent commotion was shown by the registering pencil. Not only must that absence of effect make us seek elsewhere for the cause of these mysterious accidents, but we have reason to be astonished at the insensibility of a liquid surface which remains calm though balanced on ground which was so agitated as to crack the woodwork of houses, ring bells, and displace furniture in all the neighbouring region. This disappointment is a new motive to continue this interesting research, and to enlist the physicists of every nation, all the more that wherever there are lakes there ought to be *seiches*. E. G.

EXAMINATION OF THE PHONOGRAPH RECORD UNDER THE MICROSCOPE[1]

MR. FRAZER referred to previous results obtained by him of some examinations of the tin foil which had been indented by the stylus, or needle point, of the pho-

[1] Abstract of paper presented at the meeting of the Franklin Institute April 17, 1878, by Persifor Frazer, Jun., A.M.

nograph. His object was to ascertain the shapes of the indentations made by different known sounds. The vowels and diphthongs were spoken into the mouthpiece of the apparatus with small panels in the order seen on the diagram.

These sounds were repeated thrice on each of three foils. They were then mounted on glass plates, separated, and labelled. Finally, they were cut out and mounted on another piece of glass vertically, instead of horizontally, in order that a number of the dents produced by any given sound might be on the screen at once.

Lissajous, Leon Scott, and König have provided the means of transforming sounds into form, in various ways, viz., by bright points on the ends of steel bars of different thicknesses; by vibrating membranes at the extremity of a "phonautograph," and by flames reflected in a rotating mirror. It was natural to conclude that the same vibrations, imparted to a steel point by means of a metal diaphragm, would leave an equally characteristic trace.

The same voice (that of Dr. Plush), speaking the following vowels and diphthongs as nearly as possible at the same distance from the mouthpiece, was relied upon for the matrices.

The first records tried, to ascertain whether the pronunciation was perfect, were afterwards thrown away, and the records which were studied were not in any way injured by a second passage of the point of the stylus.

By following along the nearly vertical line of impressions, which are at the same time in focus, it will be observed that this line consists of one long followed by two shorts (or two shorts followed by one long), the indentations bearing a general resemblance to each other and to seeds. This is long A, or "Ah." A glance at

[Note.—In the wood-cut, the forms made by the excursions of the stylus for the short letters are wider than they should be.]

short ă (as in "bat"), will show the same thing, but the seed-shaped hollows are narrower, and there are no abrupt terminations of the hollows by intervening parts of the foil, which have not been touched by the needle-point.

Ē (or ay), on the screen, looked like the magnets of two Bell telephones, with the small ends turned towards each other. In the diagram they look like two Indian clubs with the handles together. The same general resemblance is observed in E short, except that, as in ă short, the volume of sound being less, the intensity was less, or (what is the measure of intensity) the path of the needle-point was shorter, and it seldom entirely cleared the foil, the consequence being a continuous groove of irregular, but normally irregular, width.

Ī and Ĭ are much alike in general form, as also are Ō and Ŏ, the coupling of the pairs of the latter being the most striking feature. Ū and Ŭ, in the drawing, best show the difference in shape produced by less intensities, the short being drawn out, and more acicular.

OI is very interesting. The diphthong consists of ŎĬ, and the very moulds which characterise their sounds are to be observed in the cut.

OW presents a composite character, but its derivation has not yet been made out.

The above presentation of the subject is necessarily crude and imperfect, but will illustrate the possibilities of an exhaustive investigation.

THE LIFE-HISTORY OF A SEPTIC ORGANISM[1]

THIS was an account of a hitherto unrecorded organism, belonging to the septic series, which was found in the earlier stages of the decomposition of the macerating body of a vole. It was studied by the aid of the "continuous stage" used by the author and Dr. Drysdale in their "Researches on the Life History of the Monads,"[2] by means of which a drop of the septic fluid containing the organism can be kept under examination for an indefinite time, without evaporation; and be studied with the most delicate and powerful lenses. The method pursued was continuous study, first of the details of the several metamorphoses, and by the light thus gained, a continuous study, subsequently, of their sequences in the same individual form.

The majority of the most difficult and delicate work was done with a new $\frac{1}{35}$-inch lens, made for the author, with a special view to this class of observation, by Messrs. Powell and Lealand. He also had the advantage of the fine "new formula" lenses, made by the same firm recently, that is to say, two $\frac{1}{8}$ths, a $\frac{1}{12}$th, and a $\frac{1}{16}$th. He also used their $\frac{1}{25}$th and $\frac{1}{50}$th inch objectives.

The organism never exceeds the $\frac{1}{4000}$th of an inch in long diameter: it is oval, with a constriction slightly in front of its short diameter: and at its anterior extremity has a head-like protrusion, to which is attached a long delicate flagellum. At the sides of the shorter, or front segment of the oval, somewhat in the position of "shoulders," two long fine flagella proceed, and as a rule trail with exquisite grace behind; one on either side. It swims with great rapidity and has every variety of motion in the fluid: and in the accomplishment of its evolutions its lateral flagella are largely concerned. But besides its swimming power, it has the capacity to anchor both its trailing flagella to the floor, or the stage, or to a decomposing mass, and by coiling these flagella, and bringing itself down upon the body to which it is anchored, and then suddenly darting up so as to make its flagella, together, the radius of a circle, it darts down on the decomposing substance, and by the enormous numbers that are constantly doing it, aids in the rapid breaking up of the tissues.

By steadily following it in the free-swimming condition it was seen to undergo fission or self-division, which was a very complex and extremely delicate process; the division beginning in the front flagellum and proceeding until, by longitudinal division, a new lateral flagellum was, in the act of self-division, made for each half; and by the snapping of this both halves went free as perfect organisms, soon to commence the process again. A great deal of close and careful detail was given of this process, and was accompanied by illustrations drawn from nature. There were also accounts of a series of

[1] Abstract of Paper read before the Royal Society on the Life-History of a Minute Septic Organism: with an Account of Experiments made to determine its Thermal Death Point. By the Rev. W. H. Dallinger, F.R.M.S.
[2] *M. M. J.*, vol. xi. pp. 97—99.

observations on the frequency of the recurrence of the process of fission, by the continual following of one segmental product of the act; and also from its beginning to its cessation, in a series of separate organisms, making manifest the periods of greatest fissional intensity; and also showing the result following on the cessation of fission. In the majority of cases it was an exhaustion of vital action and death: but in a certain proportion, in which fission was not so long continued, it was a rapid change to an amœboid condition, resulting in the absorption or fusing of the lateral flagella with the body, and a change of form; the organism becoming now quite oval and having only an anterior flagellum. It swims easily, but has lost all the power and freedom of motion possessed before, moving only in a straight line. But it soon comes into contact with a colony of the organism in the springing condition, attaches itself to one of them, which then soon unanchors and both swim away. In the course of time their movements become sluggish; the sarcode of the bodies is palpably blending, they become quite still, except for amœboid movements, and then become one mass, oval in form, which elongates into a spindle-shape, remaining motionless and still in all respects for three or four hours; when, as was ultimately, and by long continued effort made out, it pours out exquisitely minute, opaque, apparently round specks, which, when carefully and steadily followed with the best appliances, were seen to develop into the adult form and size.

The author then desired to discover the relative heat-resisting power of the perfect form, and the germ or spore. The adult forms were proved by a very direct method, which was fully detailed, to be wholly destroyed at a temperature of 142° F. Two methods of heating were employed to test the resistance of the spore. One was the "dry" method which had been employed in the former researches; but which was somewhat modified and used with special precautions; and the result of an elaborate series of experiments proved, that by this mode of heating, the spore could resist a temperature of 250° F.

It was next determined to test the heat resistance of the spore when they suffered the heat, diffused in a fluid. The difficulty of accomplishing this, so as to secure an unmistakable result was carefully pointed out and dwelt on; and the opinion recently expressed by Dr. Bastian that it was "perfectly easy" shown to be an error.

The apparatus employed for the purpose was specially delicate, but enabled the author to test directly the results of heat on the spores as well as on the adult organism, without exposure after the vessel was once sealed. The form used was specially devised for these observations. The temperatures up to the boiling point of water were got in melted paraffin, and higher temperatures in a digester. The result was that 220° F. was found to be the limit of temperature which the spore of this organism could endure without destruction of vitality. That is to say 30° F. lower than the same spores could bear in a "dry" heat. But it was pointed out, that to endure this temperature, implied protection of some kind: but that this in the *undeveloping* germ, was not only capable of being understood, but would doubtless prove of immense value to the organism.

OUR ASTRONOMICAL COLUMN

THE UNIVERSITY OBSERVATORY, OXFORD.—Prof. Pritchard has published No. 1 of *Astronomical Observations made at the University Observatory, Oxford.* It comprises observations made between the autumn of 1875, when the establishment was first organised, and the end of 1877. They relate to the satellites of Saturn, double stars, and the five comets discovered in 1877, by Borrelly, Winnecke, Swift, Coggia, and Tempel, for which provisional elements and, in the case of Winnecke's comet, an extensive ephemeris are added; also elements of the orbits of ξ Ursæ Majoris, 70 Ophiuchi, and μ^2 Boötis, and comparison of the same with the interpolation curve drawn according to the method of Sir J. Herschel. The observations of the satellites of Saturn consist of differences of R.A. and N.P.D. from the centre of the primary, facilitated by the ephemerides which Mr. Marth has regularly supplied; together with the other observations now printed, they have been made with the refractor of 12¼-inches aperture, constructed for the observatory by Mr. Howard Grubb, of Dublin, Mr. W. E. Plummer, the first assistant, being credited with the greater part of them. In addition to the above work, it is mentioned that nearly twelve hundred measurable photographs have been secured by means of Dr. De la Rue's reflector, which he presented to the Observatory, and which is mounted in the eastern dome, and a very beautiful instrument for completing the measurement of these photographs has been recently received through the liberality of the same gentleman. The institution is under the control of a Board of Visitors, as usual in so many of the more important astronomical establishments at the present day, the Board being composed of the Vice-Chancellor, the Proctors, the Astronomer-Royal, the Director of the Cambridge Observatory, the Radcliffe Observer, and four other members elected by the Convocation of the University; these members are at present, Dr. De la Rue, Prof. Bartholomew Price, J. A. Dale, M.A., and W. Esson, M.A.

The position of the University Observatory is in latitude 51° 45′ 34″·15, and longitude 5m. 0·40s. west of Greenwich.

THE CINCINNATI OBSERVATORY.—No. 4 of the publications of this observatory, just issued, contains the results of measures of double stars made in the year 1877, with the 11-inch refractor, the object-glass of which was replaced early in the year after having been successfully refigured by Alvan Clark and Sons; in addition to this improvement a new driving clock was added. The stars measured are, with very few exceptions, situate between the equator and 40° of south declination, and this selection of objects gives a rather special value to the Cincinnati observations, though it has been notified from Melbourne that the remeasurement of Sir John Herschel's southern stars is in progress there. The methods of observing at Cincinnati, and the investigation of personal equation, are explained in the introduction, and the larger differences in the measured angles and distances, found on comparison with the catalogues of Struve, Sir John Herschel, Jacob (Poona), and Dembowski's measures of doubles discovered by Mr. Burnham, are indicated. Some of these larger differences occur in the case of well-known rapidly-moving binaries; but there are others which deserve further attention, to decide upon the cause of the observed changes. The following may be mentioned:—

Star.	Sir J. Herschel's Measures.	Pos.	Dist.	Cincinnati Measures.	Pos.	Dist.
		°	″		°	″
h 2036	1836·54...	40·4	—	1877·76...	25·1	1·40
Lalande...2416	36·96...	—	1·82			
h 3447	1837·11...	75·5	—	1877·80...	90·1	2·20
Lacaille ...462	37·51...	—	3·12			
h 3461	1836·54...	69·6	—	1877·85...	59·0	4·84
ϵ Sculptoris ...	36·70...	—	5·53			

Of stars observed by Sir J. Herschel with the 20-feet reflector, for Nos. 2,904, 3,494, and 5,113 (which are respectively Lacaille 8,262, 702, and 8,098), the Cincinnati measures show differences greater than 20°. The positions of these stars for 1880 are:—

	Right Ascension. h. m. s.	S. Declination. ° ′
h 2036	1 14 4	16 26
„ 3447	1 30 35	30 31
„ 3461	1 40 1	25 39
„ 3494	2 14 46	35 59
„ 5113	19 17 30	29 32
„ 2904	19 47 7	24 14

The "mean results" at the end of this publication apply to upwards of 500 objects.

THE REAPPEARANCE OF ENCKE'S COMET.—Dr. von Asten, in an extract from the *Bulletin* of the St. Petersburg Academy, has circulated an ephemeris of Encke's comet for the return in the present year, and it is also printed in No. 2,197 of the *Astronomische Nachrichten*. The elements have been perturbed to April 24, 1878, taking into account the attraction of the six old planets and the effect of a resisting medium. The perihelion passage takes place July 26·1159, G.M.T., and Dr. von Asten especially insists upon the importance of observations in the southern hemisphere after perihelion, for the improvement of the theory, and urges that at least two complete series of observations with moderately powerful instruments should be obtained, for reasons which he states are explained in a memoir now in the press. The following positions are interpolated from his ephemeris for Berlin noon, corresponding to 8h. 46m. mean time at Melbourne:—

	Right Ascension. h. m. s.	North Polar Distance. ° ′	Log. Distance from the earth.
August 1	9 46 0	79 19·8	0·0824
„ 5	10 17 23	83 53·8	0·0597
„ 9	10 47 39	88 28·0	0·0399
„ 13	11 17 24	92 58·0	0·0248
„ 17	11 47 0	97 18·8	0·0155
„ 21	12 16 30	101 24·3	0·0123
„ 25	12 45 47	105 8·7	0·0151
„ 29	13 14 34	108 27·3	0·0233
Sept. 2	13 42 30	111 17·4	0·0359

The elements of the orbit for April 24, 1878, are: longitude of perihelion, 158° 19′ 41″, ascending-node, 334° 39′ 10″ (M. Eq. 1878·0), inclination, 13° 6′ 40″, eccentricity, 0·8491669, semi-axis major, 2·210691. The perihelion distance is 0·33344, the aphelion distance, 4·08794, and the semi-minor-axis, 1·16752. The sidereal period at the above date is 1200·8 days.

NOTES

THE funeral of the late Prof. Henry, at Washington, was an imposing pageant, being attended by the President and the members of the Cabinet and the Congress—the latter body adjourning from respect to his memory—with a large number of prominent men from all parts of the country. Prof. Spencer F. Baird succeeds Prof. Henry as secretary to the Smithsonian Institution.

A MONUMENT to the late eminent physicist, Dr. Robert von Mayer, will be erected at Heilbronn, in Würtemberg. Herr Gustav Rümelin, the Chancellor of Tübingen University and well-known critic of Shakespeare, will shortly publish a biography of Dr. von Mayer.

PROF. HELMHOLTZ has written to the Royal Institution to obtain a bust of Faraday, and to the French Academy of Sciences for busts of Ampère and Regnault. No bust of Regnault being in existence, one will be executed at the expense of the Government, by M. Noel, and placed in the Hall where the Academy meets. A cast will be sent to Berlin as requested.

THE honorary membership of the Geographical Society of Italy, at Rome, has recently been conferred on Dr. George Bennett, of Sydney, who is well known as a naturalist and traveller, and who it seems had been exceedingly active in the furthering of Signor L. M. d'Albertis' late expedition to New Guinea.

PROF. ERNST HAECKEL has been nominated honorary member of the Geographical Society of Lisbon and of the Microscopical Society of San Francisco.

THE system of science and art education which centres at South Kensington and branches to the remotest parts of the kingdom, has years ago assumed the dimensions of a national organisation and done more, probably, than any other means, to foster a wide-spread artistic taste and a desire for scientific knowledge among the people. The well-trained teachers of the department are everywhere doing their humanising and elevating work. This immense organisation, every one now admits, is mainly due to the energy, intelligence, and foresight of one man, Sir Henry Cole, who has happily survived much that would have daunted a less enthusiastic and public-spirited man—survived to receive, as he did last Thursday, a well-earned and appropriate honour. On that day a large number of ladies and gentlemen assembled at Grosvenor House, by the permission of the Duke of Westminster, for the purpose of presenting to Sir Henry Cole a testimonial, the result of an effort originated some years ago. The memorial was in the form of a marble bust and memorial tablet in della robbia ware, containing a portrait of Sir Henry in mosaic. The total amount of subscriptions was 2,924*l.* 13*s.* 4*d.* After paying expenses for the monument, portrait, and bust, Sir H. Cole had already received 2,000*l.* The Duke of Westminster, in presenting the testimonial, bore testimony to the advantages which Sir Henry Cole had conferred upon the nation in his efforts to promote the development of science and art. Sir Henry Cole, in acknowledgment, said his words could but feebly express his hearty thanks to the princes, peers, commoners, men of science, art, and literature, industrial producers and handworkers, who had joined in this testimonial. After fifty years of public life, with his health declining from the constant strain of official work, he (Sir H. Cole) felt it right to resign his duties. He was not idle in his leisure. His health had improved, and he hoped still to do some useful public work. He was trying to obtain a national recognition for music, the first and most popular of all fine arts, to help elementary education to become the work of the people rather than of the State, and to promote improved health throughout the country. The portrait in mosaic of Sir Henry is to be offered to the South Kensington Museum. The marble bust will be presented to his Royal Highness the Prince of Wales, as president of the Albert Hall, with a request that it should have a suitable place in the Hall.

A STRANGE jubilee is proposed to be celebrated in Italy during 1879. Our readers know that next year 1,800 years will have elapsed since the two cities of Pompeii and Herculaneum were destroyed by earthquakes and eruptions from Mount Vesuvius. It is now intended to celebrate the anniversary of that year of destruction, and the site of the celebration is to be at Pompeii itself, as being the better known of the two buried cities.

IN the April number of the *Bulletin* of the Imperial Academy of St. Petersburg it is stated that a clergyman named Pervouchine has proved that the number $2^{2^{12}} + 1$ is divisible by $7 \cdot 2^{14} + 1$. Bouniakowsky has verified the result at the request of the Academy. Hitherto the only exception known to Fermat's statement, that all numbers of the form $2^{2^m} + 1$ are primes, is that of $m = 5$ where Euler showed that 641 is a divisor.

M. C. TH. LIEBE (*Proc.* Imper. Geol. Instit., Vienna, March 5, 1878) has found a considerable quantity of remains of the Marmot in the Diluvium near Gera (Thuringia), indicating

the existence of a larger form than the existing species, and intermediate in character to the European marmot and the bobak, and he regards it as representing the primordial stock from which the living species have proceeded in the course of time. The region in which these remains have been found bears a *steppe* character, the fauna and flora of steppes being met with both in mountainous regions and on plains. M. Liebe, who is a votary of Baron Richthofen's sub-aerial theory, admits the diluvial district of Germany to have been once a steppe region with an extreme climate and analogous to the present steppes of the Altaï.

THE French Ministry for Public Instruction has completed in the Central Palace of the Exhibition the installation of the "Salle des Missions Scientifiques." A large map has been exhibited on which all the names of the scientific missionaries are inserted on the countries which they have explored on behalf of the French Government. The collection of works published by the living members of the "Corps Enseignant" (French University) is ready; it is composed of more than 4,000 volumes neatly bound. This library is open every day from 8 to 10 in the morning. Admission to the palace is obtained at this early hour by a double ticket (price 9 francs). Not a single gallery in the Trocadero Palace has been yet opened. The success of the Exhibition is increasing daily. The average number of admissions on payment has been more than 40,000 a day for the first fourteen days. The sale of tickets by the agents is more than 1,200,000. The number of season-ticket holders was 1,000 in the beginning of last week, although no real advantage is offered to them. The system of conveyance by trams, railways, and steamers is excellent, and working very well. M. Bardoux has proposed a credit of 100,000 francs for the purpose of sending to Paris a number of *instituteurs* who will take part in their special congress during the Exhibition.

THE *Proceedings* of the Literary and Philosophical Society of Liverpool for 1876–77 forms, as usual, a thick volume, containing several papers well worthy of careful reading. The paper of chief scientific interest in this volume is that of Mr. A. J. Mott on Haeckel's "History of Creation," which, with the elaborate discussion that followed, is likely to interest all who are interesed in the subject.

A FIRM at Melbourne, New South Wales, claims to have improved the Abyssinian tube-well by attaching a drill to the first tube, and cutting through rock by imparting a rotary motion to it, instead of merely hammering it through as heretofore, which plan has been found not to succeed in hard soils.

IN confirmation of our remarks on the recent progress sciencewards in Spain, we may state that we have received further papers. One a pamphlet of thirteen pages is a paper entitled *El Alcorán*, by Señor D. Eduardo Saavedra, read at the eighth conferencia (February 25th, 1878) of the *Institucion libre de enseñanza*. The other is the prospectus of the *Revista General de Legislacion y Jurisprudencia*, publicada por D. José Reus y Garcia con la coloboracion de distinguidos Jurisconsultos y publicistas (now in the twentieth year of its publication).

FROM a Japan contemporary we learn that copper-smelting works are being built by Japanese near Kobe. The ore to be used will come from a mine near Ikeda, and is said to contain a considerable quantity of silver which is to be extracted first. Coal and copper appear to have been recently discovered in several places in this part of Japan.

A BRILLIANT meteor was seen at Geneva at 9.45 P.M. on Sunday week. It moved very rapidly from east to west, was in the shape of a pear, and was of a greenish hue, leaving behind it a slight train of light. This was evidently the same meteor that was seen by our correspondents referred to in last week's NATURE; the time was the same, making allowance for the difference of longitude between Geneva and this country.

THE total production of silk cocoons in Europe amounted upon an average to 58,000 tons per year during the last five years. Italy stands first in the list of silk-producing countries; it produces 39,000 tons per year. France produces about 10,000 tons, Turkey 4,000, Spain 2,200, Austria 1,900, Portugal 250, Greece 200, Russia 150, Germany 100, and Belgium and Switzerland only 100 tons together.

THE King of Italy has conferred the Cross of the Order of SS. Maurice and Lazarus upon Prof. Mommsen, of Berlin.

THE well-known geologist and academician, Gregor von Helmersen, of St. Petersburg, celebrated the fiftieth anniversary of his entering the Russian army, on the 5th instant.

IT is telegraphed from New York, May 14, that a despatch from Havannah announces that a terrific earthquake has occurred at Cua, in Venezuela, 600 persons having been killed. A heavy shock was also felt at Caracas. An earthquake is reported from Göttingen. On May 6 two shocks were felt, one at 10.34 the other at 10.37 P.M. The former was of greater force than the latter, and their duration was three and two seconds respectively. An earthquake was also felt in a large number of places in Morbihan (Britanny), on the 14th instant, in the morning; the hours vary according to the places. At Hennebout, a small seaport on the Blavet, it occurred at 7h. 3m., duration six seconds; at Vannes, 5h. 40m. local time (6h. 20m. Paris time). The direction was from west to east. Commotions were also felt in Lorient and Port Louis almost at the same hour as at Vannes.

WE have already referred to the *American Journal of Mathematics* promised us from the Johns Hopkins University, and were able to mention the names of some of the contributors, all of them of the first rank. We have not yet received the first number, but from a note in the *Nation* we see it has appeared, and that the programme will delight our mathematical friends. The first number contains, in 104 handsomely-printed quarto pages, eight articles, two of them from foreign contributors, in which the separate departments of astronomy, mechanics, physics, and pure mathematics are all represented. The first article is a short note of three pages, containing the proof of the proposition, that "if a fourth dimension were added to space, a closed material surface (or shell) could be turned inside out by simple flexure, without either stretching or tearing." This is followed by the first part of a paper upon the lunar theory, by Mr. G. W. Hill. Prof. Eddy, of Cincinnati, presents a simplified equation to express the relationship between the moments of flexure of a straight elastic girder at three successive points of support. An algebraic solution of the so-called irreducible case in cubic equations, with examples, covers eighteen pages. A short note on the theory of groups is communicated by Prof. Cayley, of Cambridge. Prof. Rowland's paper is a contribution to our knowledge of the theory of electric absorption. A very favourable review of Ferrero's treatise on the method of least squares is given by Mr. C. S. Peirce, and the balance of the number is occupied by Prof. Sylvester with a paper in which the new atomic theory of chemistry is applied to the graphical representation of certain mathematical conceptions. A paper on this subject, by Prof. Sylvester, has already appeared in NATURE.

WE notice the appearance of the concluding part of the *Jahresbericht der Chemie* for 1876. This almost indispensable companion of the chemist, founded by Liebig and Kopp, is now under the editorship of Prof. Fittica, of Marburg, assisted by a corps of twelve other leading German and Austrian chemists, and has reached its twenty-ninth volume. A glance at the space allotted to the various sub-divisions gives a general idea of the

tendencies of modern chemical research. General and physical chemistry occupy 160 pages, inorganic chemistry 140, organic chemistry 650, analytical chemistry 100, technical chemistry 170, chemical geology 40, and mineralogy 60. Over 1,000 authors are referred to in the course of the work. It is a strange circumstance that German should be the almost exclusive medium for the publication of exhaustive and elaborate annual reports of the progress made in each branch of natural science.

PROF. KOLBE, of Leipzig, has just added another to the numerous German text-books of inorganic chemistry, and justifies its appearance by the opinion that the existing works, with the exception of the translation of Roscoe's Chemistry, contain far too much material for elementary treatises. A vigorous warfare is waged against the now so prevalent use of Latin and Greek names among German chemists, a custom certainly from a foreign standpoint not to be regretted, bringing as it does the scientific nomenclature more in unison with that of England, France, and Italy. We notice also that Prof. v. Richter, of Breslau, has just issued a second edition of his Text-book of Inorganic Chemistry, and that Prof. Wislicenus is engaged on a new and modernised edition of Regnault's Chemistry, the ninth edition of this classical little work which has appeared in Germany.

A SECOND edition of Prof. Klenke's well-known work on the adulteration of food is now being published at Leipzig (Weber). The author has chosen the dictionary form for this edition, and the title is now "Illustrirtes Lexikon der Verfälschungen der Nahrungsmittel und Getränke."

THE last part published of the *Sitzungsberichte der Münchener Academie für* 1877 contains an interesting report by Herr Hermann Schlagintweit-Sakünlünski upon the ethnographical material in the large collections made by the brothers Schlagintweit on their celebrated travels, and gives a detailed account of its distribution at the royal "Burg" at Nürnberg.

THE publication of the eighth edition of Ed. v. Hartmann's "Philosophie des Unbewussten" is now announced. It is a long time since a purely philosophical work has run through eight editions.

THE experiment of using superheated water for locomotives has been successfully tried on the tramway connecting Reveil and Marly-le-Roi, in France. The engines are charged with water heated to 180° C., which is allowed to vaporize as fast as required; and by doing away entirely with furnaces in the locomotives, the dangers of explosion, as well as the causes of terror to passing horses, are easily avoided. A locomotive, propelled in this manner, and attached to two carriages, ascended a gradient of 5½ in the hundred at the rate of sixteen miles an hour.

M. L. A. FORSMANN shows, in a recent communication to the Swedish Academy, that solenoïds are able to produce the same unipolar induction currents as magnets, and that the same laws rule in both cases.

AN immense deposit of guano has recently been discovered in the Wershchowskij grotto near Oizowo, in the Russian government of Kjelze. Chemical analysis proved the quality of the guano to be in no way inferior to that of Peru. It is stated that Prussian agriculturists have already sent agents to Oizowo to purchase large quantities of this guano.

IN the April session of the Deutsche geologische Gesellschaft Prof. Beyrich exhibited two specimens of *Ammonites iphicerus*, one of which came from Lichtenfels, in Bavaria, and the other from Mombassa, in South Africa. On account of their close similarity he assigned to the Jura formations of Mombassa the same age as that of the Bavarian deposits. The specimens attracted especial interest on account of the complete preservation in both cases of the aptychus, the origin and use of which still remain an unsolved problem. Herr Römer presented a specimen of Archæocyathus from the strata in the Sierra Nevada immediately above the archaic deposits. It is the first fossil found in these formations, and places them probably in the old silurian. Papers were likewise presented by Herr Ladebeck on the regularity in the deformation of markasite crystals, and by Herr K. Lossen on the albite gneiss of Schweppenhausen.

SOME experiments have lately been made by M. Grehant with regard to endosmose of gases through lungs separated from an animal. He finds in the phenomenon two distinct phases; in the first, the lungs swell till they even touch the walls of the bell-jar in which they are contained, while a manomoter shows there is considerable increase of pressure. Then comes a second phase, in which the lung returns to its original volume and the pressure diminishes. From observations on the living animal, however, M. Grehant concludes that in this case the phenomenon is very slight indeed.

PROF. KIRCHHOFF has presented to the Berlin Academy a series of considerations on the movement of the electric current in submarine cables, based on Helmholtz's well-known equations for the components of the intensity of a current, and the electrostatic moment dependent on the capacity for dielectric polarisation. The conclusions deduced are, that the rapidity of propagation of the electric waves increases with the conductivity of the gutta-percha covering, while the breadth of the undulations decreases in the same ratio.

A GERMAN translation of Father Secchi's work, "On the Astronomy of the Fixed Stars," will shortly be published by F. A. Brockhaus, of Leipzig. It will form the thirty-fourth volume of the International Scientific Library.

IN No. 8 of the *Journal* of the Russian Chemical Society are two papers, by M. Lermontoff, on the employment of two galvanometers provided each with a Töpler's apparatus for reducing the oscillations of a magnet, and on the methods employed by M. Brauer for the construction and verification of balances of precision. This latter paper is a detailed description of the methods devised by the skilful optician of the Pulkova Observatory for manufacturing and adjusting the prisms of balances, special apparatus having been devised by him and constructed for these purposes. The verifying apparatus discovers any deviation from the straight line on which the prisms should be placed, if it exceeds 30″, and the equality of the length of the arms of the balance is verified with a precision of 0·0000125 of their length. We are all the more pleased to see the appearance of such a description, as the methods used by constructors of precise scientific apparatus are generally unknown. We notice also in the same number a note, by M. Borgmann, on Maxwell's theory on the tensions in the magnetic field; and a note, by M. Geschus, on the various theories proposed for explaining the radiometer.

A. ARZRUNI communicates, in a recentt number of Groth's *Zeitschrift für Crystallographie*, a number of interesting results from a study of the crystalline properties of various organic bodies. Triphenyl-benzene is found to possess the property of double refraction in a degree surpassing that of any other crystalline body yet known. In substituted compounds he shows also that the introduction of the nitro-group invariably causes a much slighter change in crystallographic properties than when hydrogen is substituted by bromine or iodine.

THE Phylloxera, which has been in Spain and Portugal for some time, is now reported to have got as far as Greece.

THE additions to the Zoological Society's Gardens during the past week include a Syrian Fennec Fox (*Canis famelicus*) from

Arabia, presented by Commander F. Catton; a Wood Owl (*Syrnium aluco*), European, presented by Mr. C. B. Wharton; a Copper Head Snake (*Cenchris contortrix*) from North America, presented by Dr. Painter; a European Bearded Vulture (*Gypaëtus barbatus*), South European, a Rattlesnake (*Crotalus durissus*) from North America, deposited; a Collared Fruit Bat (*Cynonycteris collaris*), a Reindeer (*Rangifer tarandus*), a Chinchilla (*Chinchilla lanigera*), born in the Gardens.

RECENT RESEARCHES ON THE PHENOMENA OF FLUORESCENCE

SOME time ago Herr E. Lommel drew attention to the fact that certain substances do not follow the rule mentioned by Prof. Stokes, viz., that each ray of light produces fluorescent rays of smaller refrangibility only; recently Herr Lommel found several other substances which partly follow and partly deviate from Stokes's rule, which is thus proved to be of somewhat limited validity. Herr Lommel communicated his further researches on this subject at a recent meeting of the Physical Society of Erlangen. He now divides all fluorescent bodies into three classes: the *first class* comprises those substances upon which each homogeneous ray of light, capable of producing fluorescence, produces the whole fluorescent spectrum (fluorescence of the first order); the *second class* contains those substances upon which the same ray of light produces only those rays of the fluorescent spectrum which are of a smaller (or at most of an equal) refrangibility than the ray itself (fluorescence of the second order). The *third class* finally, embraces those bodies whose fluorescent spectrum consists of two parts, one of which corresponds to fluorescence of the first, and the other to fluorescence of the second order (compound fluorescence). Herr Lommel enumerates nine substances of class I., twenty-five of class II., and seven of class III., and gives the commencement of their fluorescence in the spectrum and the extent of the latter in tables accompanying his paper.

If we examine the series of substances enumerated by Herr Lommel, we find no less than fifteen different ones, which deviate from Stokes's rule, *i.e.*, depart from it altogether (the nine substances of class I.) or only in part (the first six substances of class III.). Of course the substances in class II. which follow this rule are more numerous. Investigating the peculiarities of these three classes more closely, and in various directions, we arrive at the following conclusions:—

1. The first class comprises substances with very strong bands of absorption only, of which one remains visible even when the solution is greatly diluted and after the absorption in other parts of the spectrum has become imperceptible. Accordingly these substances are strongly and intensely coloured (green, red, orange, yellow). The absolute maximum of fluorescence corresponds to this absolute maximum of absorption in the fluorescence-spectrum.

2. The second class embraces all fluorescent substances which show only a one-sided absorption of the more refrangible end of the spectrum. They therefore appear yellow, brown, or colourless, the latter principally if the absorption bands lie only in the extreme violet and ultraviolet.

To this class certain bodies belong which show absorption bands to which maxima of fluorescence correspond at the same time. But these absorption bands appear broad and indistinct (so-called shadows) and they are not really maxima. If the solution is diluted they disappear very soon, and long before the absorption in the violet ceases to be perceptible. Nitrate of uranium shows a number of strongly marked absorption bands, which, however, are in nowise related to fluorescence. Uranium glass as well, which belongs to the first class, shows absorption bands in the red and yellow which have nothing to do with its fluorescence. The green colour of the fluor spar from Alston Moor is caused by an absorption band in the red, and this also has no reference to fluorescence.

3. The third class, like the first, contains only bodies with strong absorption bands and of intense coloration (green, blue, violet, red, orange).

Fluorescence of the first order therefore seems to be in causal connection with the existence of a prominent maximum of absorption and fluorescence.

4. The fluorescent spectrum of the substances in class I. is of equal colour everywhere, if we neglect the slight changes in the shades of colour, which are caused by the absorption exercised by the substance upon its own fluorescent light. At the parts which are less fluorescent, a proportionate diminution of the fluorescent rays most subject to absorption takes place, and hence a deepening of the shade; these rays are at the same time the more refrangible ones in all bodies belonging to this class.

5. The fluorescent spectrum of the substances in class II. is of unequal colour, changing gradually its tone of colour, and it only becomes equal where the spectrum of the fluorescent light ends. But even here the fluorescent spectrum may seem of equal colour everywhere to the naked eye. This is the case where the spectra of the exciting and the excited rays are superposed on one another only a little (morine-alumina solution; nitrate of uranium), or if the fluorescence begins in the blue or in the violet.

6. The fluorescent spectrum of the bodies in class III. consists of two parts, viz., one equally coloured in its whole length (less refrangible), and one coloured differently, with gradual change of colour (more refrangible), becoming equally coloured only where the total spectrum of the fluorescent lights ends. At the boundary of the two parts an almost sudden change of colour takes place.

7. The substances of class III. behave like mixtures of a substance of class I. and one of class II.

The solutions of orchil and litmus appear as mixtures of two fluorescent substances, from the fact that, according to the dissolving medium, now one and now the other fluorescence is prominent, and the aspect of the total fluorescence changed. From two different kinds of litmus Herr Lommel obtained alcoholic extracts, of which the one showed orange and the other greenish-yellow fluorescence in daylight; their fluorescent spectra, however, showed no difference, except that the yellowish-green part was proportionately more developed in the second specimen. Fluoride of aniline may also be considered as a mixture of two fluorescent substances. That brasileïne is a mixture of this kind is rendered very probable by the circumstance that the fluorescence of the second order is destroyed by the addition of soda, but strengthened by the addition of ammonia, while that of the first order remains intact. Whether the chamaeleïne colouring matters are really chemical compounds, as they certainly appear to be, must first be decided by a closer investigation of these substances.

If it could be proved beyond doubt that all these substances are mixtures of two fluorescent substances, we might abolish class III. altogether, and enumerate the separated substances in classes I. and II. But, since this separation has not yet been actually effected, and since the possibility, that a unit (*einheitlich*) molecule may possess both kinds of fluorescence simultaneously, cannot be discarded *à priori*, Herr Lommel felt compelled still to retain class III., *i.e.*, bodies possessing compound fluorescence.

By artificially mixing substances of the first and second class, fluorescences of the third order may be obtained in great variety. In this way wonderful effects of colour are often obtained, and they may be rendered still more astonishing by the addition of strongly coloured non-fluorescent substances.

Thus we see that yet other compound fluorescences than those belonging to class III. are possible, and may, indeed, be produced artificially; viz., confining ourselves to only two fluorescent substances, by mixing two substances of the first or two substances of the second class. A mixture of the first kind (of naphthaline-red with fluoresceïne, or with eosine, for instance) may be easily recognised as such when examined by its spectrum; their fluorescent spectrum consists of two parts separated by a minimum, neither of which follows Stokes's rule, but of which the second refrangible part (belonging to the fluoresceïne or eosine) disappears, as soon as the incident homogeneous light has been diminished down to that limit, where it ceases to cause fluorescence in these substances. It is, however, more difficult to recognise a mixture of two substances of the second class as such, as it behaves like a simple substance of that class. It is possible that amongst the substances enumerated in class II. there are mixtures of this nature, consisting of two or more substances not separated hitherto. The varying behaviour of the extracts of soot could, for instance, be easily explained by the supposition that in soot there are contained two or more fluorescent substances of the second class, which are dissolved in varying proportions by the dissolving media.

Herr Lommel did not feel justified in establishing separate classes for the two kinds of compound fluorescence just de-

scribed, since no natural instance of the former kind is known up to the present, and since the latter kind cannot be recognised as compound fluorescence by any optical means.

THE ARTIFICIAL TRANSFORMATION OF THE ALPINE SALAMANDER

THE success of Madame von Chauvin in producing the development of Amblystoma from the Mexican axolotl by gradually accustoming it to live in air, induced her to attempt a very interesting interference with the life-history of the black or Alpine salamander, *Salamandra atra*. This is an ovoviviparous species, and although its young possess large gills while within the body of the mother, they are born to commence a land life immediately; while other species of salamander, especially the spotted one, *S. maculata*, found in adjacent districts to the subject of inquiry, bring forth their young with gills, and they pass a considerable period in water before taking to land. The problem which it was desired to solve was, whether the young of the black salamander, taken from the mother before the normal time of birth, and placed in water under favourable conditions, could become adapted to an aquatic life. It is interesting to note that while only two eggs out of many come to full development in the black salamander, forty or fifty develop in the spotted one; yet individuals of the two species are about equally numerous in their respective localities. This shows the value of the avoidance of life in water with its attendant risks, though probably the diminution of terrestrial enemies in the more elevated localities frequented by the black salamander is a considerable influence in its favour.

Madame von Chauvin's researches are detailed in a recent number (vol. xxix. p. 324) of the *Zeitschrift für wissenschaftliche Zoologie*. They commenced on July 30, 1875, with twenty-three larvæ taken in various stages of development; eight were about $1\frac{1}{2}$ centimetres long, twelve were from $4\frac{1}{2}$ to 5 centimetres, and had almost completed their metamorphosis into land salamanders. One was a little less developed, 4·3 centimetres long, the gills and skin-glands were less perfect, and the skin was very transparent and unwrinkled. This larva, unlike the rest, appeared at ease when placed in water, and made no attempts to get out of it. The next problem was to feed the little creature, and the first attempt was made by supplying a number of various minute water insects; but although it evinced some desire to catch them, the insects were able to escape capture, while the larva seemed to become annoyed by their presence. Later on, a minute earthworm being offered, it was taken and swallowed, and the problem was solved. A daily supply of the same food was thenceforth taken by the young salamander.

The gills which the creature possessed in the oviduct appeared from the first little adapted for life in water; they were of so thin a texture that they could hardly be expected to endure exposure and motion, while their great extent evidently hampered the movement of the animal. Consequently the gills first became pale and bloodless, then shrank, and on the third day were entirely thrown off, down to the very base. But on the same day on the right side, a day later on the left, minute buds appeared, three on each side, which gradually enlarged into ball-like protuberances; from these, after three weeks, gill-fringes were put forth, which finally numbered nine on the first pair of gills. The fringes were mostly arranged along the external border of the gills, and they assumed a brown-spotted character, while the blood-circulation through them became plainly perceptible. They were very much less extensive than the previous set of gills, but appeared to perform the work of respiration perfectly; the creature remained completely beneath the surface of the water, without ever coming up to breathe air. While the new gills were being developed the larva remained at rest as if dead, only eating the earthworms when they were offered.

When the gills had attained a length of 2·2 mm. the larva became lively, and concurrent with this was the completion of another transformation. The delicate and transparent swimming membrane of the tail was lost, and replaced by a less transparent and stouter one, of greater dimensions. The creature now seemed to enjoy life much more than before, exhibiting greater interest in its living food, with which it would play before swallowing it. Finally, after six weeks' residence in the water, a skin-shedding commenced, the skin coming away piecemeal for a fortnight.

The larva continued to grow satisfactorily without undergoing further modification, until it had been fourteen weeks in the water, having attained a length of 6 centimetres. The gills then began to shrink, and the tail to assume a rounder form, and in three days the skin was shed, revealing the normal black and wrinkled skin of the land salamander. In nine days from their first shrinking the gills were nearly absorbed, only little stumps remaining. At last it crawled out of the water, and on the fourteenth day the gills were completely absorbed and the gill clefts closed. The remaining larger larvæ of this experiment lost their primary gills less satisfactorily and in a greater length of time. New gills commenced to bud, but the creatures were gradually destroyed by fungus-growths attacking various parts of their skin. The fact that they were altogether more advanced in their metamorphosis rendered them unable to adapt themselves quickly to their new conditions.

A second series of researches on the Alpine salamander was carried on in the summer of 1876, when a large number of individuals were collected at Thusis, at the confluence of the Rhine and the Nolla. The animals were collected thirteen days earlier than in the previous year, so that the development of the young was not so forward. Thirty-three larvæ were taken from the oviducts, eight of which were from 8 mm. to 10 mm. long, two 12 mm., and twenty-three from 35 mm. to 40 mm. All had their skin still transparent and their gills not yet of full size. After twelve of them had refused insects, minute earthworms were administered to them, but they did not eat them till after some hours. Two larvæ, immediately after being taken out of the mother and placed in the water, fastened respectively on the head and tail of a worm that was wriggling at the bottom of the water. Their difficulty was solved by cutting the worm in two, and each obtained half. This method of immediate feeding was thereafter successfully adopted, and it appeared to develop a good appetite in the larvæ.

One noteworthy circumstance in regard to these creatures was that, at a time when they would normally be still within the body of the parent, they were as active and as eager for food as new-born larvæ of the spotted salamander. They were often so greedy for their prey that they seized hold of the limb of a neighbour of their own kind instead of the desired worm. But nevertheless these creatures did not develop in the desired direction, the gills did not begin to shrink quickly, and when they did they were not got rid of as a whole, but the dead portions, remaining attached to the body, became the seat of fungus growth, which speedily increased and spread so as to kill the animal. Thus none of the subjects of investigation really became adapted to their life in water. In two cases it was attempted to succeed artificially by cutting off the gills nearly at the base; one died soon, owing to fungus growths, the other quickly became a land salamander.

Experiments like the foregoing have the highest interest, for they mark out for us the actual path of adaptation to changed physical conditions. It appears highly probable that the spotted and the Alpine salamanders were at no very distant period of time one species, and that as physical conditions became changed one variety became more and more adapted to more elevated and rocky habitats, where water for the early life of the larvæ was not commonly to be met with. Thus gradually the birth of the young was postponed, and they became non-aquatic; concurrently fewer and fewer of the many eggs were developed. The spotted salamander, meanwhile, became more and more specialised to inhabit the lowland districts. Such cases as Madame von Chauvin's, if they remained single instances, would suffice to establish natural selection as a *vera causa* of the mutation of species.

G. T. BETTANY

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

BRISTOL.—The Council of University College have resolved to found an Engineering School in connection with the scientific and technical courses of instruction already established. It is announced that the scheme will meet with the support of the local engineering firms. In accordance with this scheme the lectureships in Mathematics and Experimental Physics have been elevated into professorships, and the present holders of the lectureships, J. F. Main, M.A., D.Sc., and S. P. Thompson, B.Sc., B.A., have been elected to the new chairs.

It is stated that the new buildings of the College will be commenced at once, an excellent site having been secured several months ago. The number of students attending the present term exhibits a considerable increase upon the corresponding term of the preceding year.

SOCIETIES AND ACADEMIES

London

Royal Society, May 2.—"On the Reversal of the Lines of Metallic Vapours," by G. D. Liveing, M.A., Professor of Chemistry, and J. Dewar, M.A., Jacksonian Professor University of Cambridge. No. II.

Since their last communication to the Society the authors have succeeded in reversing characteristic lines of the vapours of rubidium and cæsium. They operated in glass tubes into which some dry rubidium or cæsium chloride was introduced, and a fragment of fresh cut sodium, and afterwards either dry hydrogen or dry nitrogen admitted, and the end of the tube sealed off at nearly the atmospheric pressure. Through these tubes placed lengthways in front of a spectroscope a lime-light was viewed. On warming the bulb of a tube in which rubidium chloride had been sealed up with sodium, very soon there appeared two dark lines near the extremity of the violet light identical in position with the well-known violet lines of rubidium. Next appeared faintly the channelled spectrum of sodium in the green, and then a dark line in the blue, very sharp and decided, in the place of the more refrangible of the characteristic lines of cæsium in the flame spectrum. As the temperature rose these dark lines, especially those in the violet, became sensibly broader; and then another fine dark line appeared in the blue in the place of the less refrangible of the cæsium blue lines. During this time no dark line could be observed in the red, but as the temperature rose a broad absorption band appeared in the red with its centre about midway between B and C, ill-defined at the edges, and though plainly visible not very dark. The lines in the violet had now become so broad as to touch each other and form one dark band. On cooling the absorption band in the red became gradually lighter without becoming defined, and was finally overpowered by the channelled spectrum of sodium in that region. The double dark line in the violet became sharply defined again as the temperature fell. There are two blue lines in the spectrum of rubidium taken with an induction-coil very near the two blue lines of cæsium, but they are comparatively feeble, and the two dark lines in the blue which the authors observed in the places of the characteristic blue lines of cæsium they believe must have been due to a small quantity of cæsium chloride in the sample of rubidium chloride.

When a tube containing cæsium chloride and sodium was observed, in the same way as the former, the two dark lines in the blue were seen very soon after the heating began, and the more refrangible of them broadened out very sensibly as the temperature increased. The usual channelled spectrum of sodium was seen in the green, and an additional channelling appeared in the yellow, which may be due to cæsium or to the mixture of the two metals. They have at present no metallic cæsium wherewith to decide this question. Indeed the cæsium chloride used was not free from rubidium, and the dark lines of rubidium were distinctly seen in the violet.

It is remarkable that these absorption lines of cæsium coincide with the blue lines of cæsium as seen in the flame, or in the spark of an induction-coil without a jar, not with the green line which that metal shows when heated in an electric spark of high density. In like manner both the violet lines of rubidium are reversed in the tubes, and both these violet lines are seen when the spark of an induction-coil, without jar, is passed between beads of rubidium chloride fused on platinum wire, though only one of them appears when a Leyden jar is used.

The authors have extended their observations on the absorption of magnesium and of mixtures of magnesium with potassium and sodium, using iron tubes placed vertically in a small furnace fed with Welsh coal, as described in their former communication.

The result of several observations, when commercial magnesium (*i.e.*, magnesium with only a small percentage of sodium in it) was used, is that the absorption produced by magnesium consists of—

1. Two sharp lines in the green, of which one, which is broader than the other, and appears to broaden as the temperature increases, coincides in position with the least refrangible of the *b* group, while the other is less refrangible and has a wave-length very nearly 5,213. These lines are the first and the last to be seen and very constant, and they at first took them for the extreme lines of the *b* group.

2. A dark line in the blue, always more or less broad, difficult to measure exactly, but very near the place of the brightest blue line of magnesium. This line was not always visible, indeed rarely when magnesium alone was placed in the tube. It was better seen when a small quantity of potassium was added. The measure of the less refrangible edge of this band then gave a wave length of very nearly 4,615.

3. A third line or band in the green, rather more refrangible than the *b* group. This is best seen when potassium as well as magnesium is introduced into the tube, but it may also be seen with sodium and magnesium. The less refrangible edge of this band is sharply defined, and has a wave-length about 5,140, and it fades away towards the blue.

These absorptions are all seen both when potassium and when sodium are used along with magnesium, and may be fairly ascribed to magnesium, or to magnesium together with hydrogen.

But besides these other absorptions are seen which appear to be due to mixed vapours.

4. When sodium and magnesium are used together, a dark line, with ill-defined edges, is seen in the green, with a wave-length about 5,300. This is the characteristic absorption of the mixed vapours of sodium and magnesium, it is not seen with either vapour separately, nor is it seen when potassium is used instead of sodium.

5. When potassium and magnesium are used together, a pair of dark lines are seen in the red. The less refrangible of these sometimes broadens into a band with ill-defined edges, and has a mean wave length of about 6,580. The other is always a fine sharp line, with a wave-length of about 6,475. These lines are as regularly seen with the mixture of potassium and magnesium as the above-mentioned line (5,300) is seen with the mixture of sodium and magnesium, but are not seen except with that mixture.

6. On one occasion, with a mixture of potassium and magnesium, another dark line was seen in the blue, with a wave-length nearly 4,820. This line is very near one of the bright lines, seen when sparks from an induction-coil, without a Leyden jar, are taken between electrodes of magnesium, and may very likely be due to magnesium alone, and not to the mixture of vapours, as we only observed it on one occasion.

There is a certain resemblance between the absorptions above ascribed to magnesium and the emission spectrum seen when the sparks of a small induction-coil, without Leyden jar, are taken between electrodes of magnesium. This emission spectrum is the same, with the addition of some blue lines, as that seen when the sparks are taken from a solution of magnesium chloride, as accurately described by Lecocq de Boisbaudran, and as that seen in burning magnesium (Dr. Watts, *Phil. Mag.*, 1875).

The pair of lines (1) correspond nearly with the *b* group, but slightly displaced towards the red; the shaded band (3) corresponds less closely to the series of seven lines 5,000 to 4,930, which progressively decrease in brightness towards the blue, and is also a little less refrangible than that series; the broad line in the blue (2) corresponds to the pair of lines 4,570 and 4,590, and the remaining line (6) to the line 4,797; also both displaced a little towards the red. No absorption corresponding to the extreme lines 4,481 and 5,528 was observed. There is plainly no exact reversal except of the line b_1, and even in that case it may be an accident if we suppose the two dark lines (1) to represent the extreme lines of the group *b*. It may be noted in connection with this that the absorption lines described by the authors in their former communication as seen with sodium and potassium (wave-lengths 5,510 and 5,730) are near to, but *more* refrangible than, well-known emission lines of those elements.

They observe that there is in the solar spectrum an absorption line, hitherto unaccounted for, closely corresponding to each of the above-described absorption lines. Thus, on Angström and Thalén's map there are dark lines at 6,580 and 6,585, with more or less continuous absorption between them, a broad dark line between 6,474 and 6,475, and a dark line at 5,300. There are also dark lines nearly, if not exactly, coincident with the series of seven bright lines of magnesium above described, which they have not seen strictly reversed. The coincidences of the series of the solar spectrum hitherto observed have, for the most part, been with lines given by dense electric sparks; while it is not improbable that the conditions of temperature, and the admixtures of vapours in the upper part of the solar atmosphere, may resemble much more nearly those in their tubes.

They intend to pursue their observations, using higher temperatures, if they can obtain tubes which will stand under those circumstances.

May 2.—"On the Determination of the Scale Value of a Thomson's Quadrant Electrometer used for Registering the Variations in Atmospheric Electricity at the Kew Observatory," by G. M. Whipple, B.Sc., Superintendent of the Kew Observatory.

The Meteorological Council, being desirous of discussing the photographic traces produced by their electrograph at the Kew Observatory some time since, requested the Kew Committee to institute a series of experiments, with the view of determining the scale value of the instrument, in order to prepare a suitable scale for measuring the curves.

The author having found, in some preliminary experiments with 300 Bunsen's elements, that the greatest potential to be obtained with these was inadequate for his purpose, he was enabled through the kindness of Dr. De la Rue, to make use of that gentleman's large chloride of silver battery for determining the scale value of the electrometer. There were in all nine experiments made, in five of which the deflections were read off with the eye, whilst in the remaining four they were registered by photography. Deflections were measured for potentials varying from 0 to 900 cells positive, and from 0 to 300 negative.

By combining the results of these experiments and taking the means for every hundred cells, the following table is obtained:—

POSITIVE.				NEGATIVE.	
No. of Cells.	Deflection in Inches.	No. of Cells.	Deflection in Inches.	No. of Cells.	Deflection in Inches.
100	0·93	600	3·95	100	1·04
200	1·77	700	4·20	200	2·34
300	2·48	800	4·42	300	3·75
400	3·09	900	4·69		
500	3·57				

On laying down these values in a curve, making use only of those between the limits of − 200 and + 700, as the others are beyond the capability of correct registration by the electrograph, a regular smooth curve is produced, which, being projected upon one of the ordinates, gives a scale by means of which the electrograms are now easily tabulated.

The value of the electromotive force of one De la Rue chloride of silver cell being 1·03 volt, as determined by Messrs. De la Rue and Müller (*Proc.* Roy. Soc., vol. xxvi. p. 324), the scale thus formed has been assumed to represent volts with sufficient accuracy for the required purpose.

Chemical Society, May 2.—Dr. Gladstone, F.R.S., president, in the chair.—A lecture on the chemical aspects of vegetable physiology was delivered by Sidney H. Vines. The lecturer commenced by giving a historical sketch of our knowledge of the absorption of carbonic acid and the evolution of oxygen by plants, the circulation of starch grains, and the functions and nature of chlorophyll. Sachs first proved that starch grains were not formed in plants which are bleached, from the absence of light, and that their formation in the chlorophyll corpuscles depended on the exposure of the plant to bright sunlight. Godlewski showed that if no carbonic acid was present no starch grains were formed. So there are two sets of phenomena, viz., the evolution of oxygen (with absorption of carbonic acid) and the formation of starch grains, for both of which three conditions are essential, viz., sunlight, chlorophyll, and carbonic acid. These two sets of phenomena are therefore probably connected and belong to the same function. Great diversity of opinion exists both as to the composition and functions of chlorophyll. The lecturer gave a short account of the views brought forward by Pringsheim, Karl Kraus, Pfaundler, Wiesner, &c., and entered more in detail into the statements and theories advanced by Sachse. In the second part the lecturer considered the formation of vegetable acids, and pointed out that the views of Liebig and Mulder had not been confirmed by subsequent experiments. The part played by pyrocatechin, asparagin, &c., in the formation of carbohydrates was next considered, and the lecturer concluded by pointing out the necessity for quantitative work before we could hope to attain clearer and more certain views on the important functions of assimilation, excretion, &c., in the vegetable kingdom.

Anthropological Institute, April 9.—John Evans, F.R.S., president, in the chair.—The following new Members were announced:—Messrs. G. J. Romanes and R. J. Hutton.—Mr. W. M. Flinders Petrie read a paper on inductive metrology, the purpose of which, as explained by him, is to deduce the units of measure employed by ancient peoples from the dimensions of existing remains. Where units derived from several different buildings coincide, a high probability of the accuracy of the result in units is obtained. This principle has been tested by application to the monuments existing among the peoples of the Mediterranean. Mr. Petrie had also applied it to the earthworks of this country. At Hill Devereux he had obtained an unit of 691 inches. At Steeple Langford, an unit had been derived which varied only by 5 inches. Near Orcheston is an earthwork forming a perfect ellipse. From this Mr. Petrie argued a considerable knowledge of mensuration on the part of the flint-workers, by whom it had been constructed. He urged the necessity of accurate measurement on the part of observers.—Dr. E. B. Tylor read a paper on the game of Patolli, in ancient Mexico, and its probable Asiatic origin. The game is a combination of dice and draughts. It was similar to a game called Patcheesi, in use in India, played by throwing cowries on to a board divided into squares of a certain pattern. So devoted are the natives to this game, that a story is told of a Provincial Governor who habitually won back his servants' wages from them at it, and thus got served for nothing.

Linnæan Society, May 2.—Dr. W. B. Carpenter, F.R.S., vice-president, in the chair.—M. C. Chambre and Mr. T. Comber were elected Fellows of the Society, and five Foreign Members, to fill the annual vacancies, were likewise unanimously elected.—Mr. J. R. Jackson exhibited specimens of fruits, leaves, and portions of the stem (used as a substitute for soap) illustrating peculiarities of *Yucca baccata*, Torrey. This plant extends from South Colorado far into Mexico. Northwards it is acaulescent, southwards it develops a trunk ten feet high. The fruit, a dark purple berry, is preserved and eaten as winter provision, and the plant is commonly known as the Rocky Mountain Banana.—A note was read from the Rev. H. H. Higgins concerning a large new Tubularian Hydrozoon (probably allied to Clava?) from New Zealand.—On behalf of Mr. Thomas Higgin there was exhibited a photograph of *Chitina ericopsis*, Carter, and also microscopic specimens of this rare species of the Hydractiniidæ from New Zealand.—Mr. J. C. Galton called attention to a spined dermal plate of the Ray tribe of fishes, mistaken for a fossil, and obtained near the Barking Priory.—The Secretary read in abstract a paper on Marupa, a genus of the Simarubaceæ, by Mr. J. Miers. This is founded on a curious fruit and specimens of wood exhibited in the Brazilian department of the Paris Exhibition, 1857. Signor Netto, in 1856, described a Brazilian plant under the designation *Odina francoana*, and bearing the vernacular name "Pão Pombo," as did the above-mentioned woods. Mr. Miers, however, is of opinion that Netto's species cannot belong to Odina as that genus is Anacardiaceous, and quite foreign to the American Continent. Then follows the technical characters of the new species *Marupa francoana* and *M. paraensis*.—A short paper was read by Mr. R. Irwin Lynch on the seed-structure and germination of a species of Pachira. The seeds were received at Kew, July, 1877, and labelled the "Provision Tree." Varying in size, they consist chiefly of one fleshy-lobed cotyledon, the second being exceedingly diminutive and functionless. Germination occurs in a fortnight after sowing, and in one instance the larger persistent cotyledon did not appear to be exhausted for nearly six months.—The main facts of a detailed communication on the occurrence of conidial fructification in the Mucorini, illustrated by Choanephora," by Dr. D. D. Cunningham, was, in his absence, read by the Secretary. According to observations and experimental investigations conducted for a series of years in India, Dr. Cunningham proved that Choanephora is a genus of Mucorine, and not Mucedine fungi, as Currey had regarded it in 1872. It is, moreover, capable of producing four kinds of fructification, as follows:—By (1.) Zygospores = sexual fructification; by (2.) Conidia; (3.) Sporangial spores; and (4.) Chlamydophorous = asexual fructification. These phenomena afford a possible explanation of certain otherwise conflicting conclusions which have been arrived at by such competent observers and authorities as Brefeld, Van Tieghem, and Le Monnier. At all events, it yields a note of warning that classification of fungal organisms based alone on one form of fructification may lead to false conclusions. The present researches likewise show that M. de Bary's suggested analogy between the Mucorini and Ascomycetes, in respect of their fructification,

is well founded, although the observations which originally suggested it have since been shown to be fallacious. Dr. Cunningham states that the presence of Choanephora on plants certainly greatly accelerates decay, but it is a cause, not a consequence, of advanced putrefaction.

Physical Society, March 30.—Prof. W. G. Adams, president, in the chair.—The following candidates were elected Members of the Society:—S. Bidwell, M.A., LL.B., W. Grant, E. Gurney, and J. H. Smith.—Mr. W. H. Preece described Byrne's pneumatic battery and exhibited some of the results that may be obtained by its means. It is especially devised with a view to provide the medical profession with a portable battery capable of producing a considerable amount of heat, as is required for cauterising operations. The negative plate consists of a very thin plate of platinum to which a lead backing is soldered, and this is covered with a sheet of thick copper also coated with lead, the whole being then covered with a non-conducting varnish with the exception of the exposed platinum face; such an arrangement is found to be advantageous in that it increases the conductivity of the negative plate. Two of these plates are arranged to face the zinc plate as in Wollaston's form of cell, and the exciting liquid consists of twelve ounces of bichromate of potash, one pint of sulphuric acid, and five pints of water. By using such a mixture the sulphuric acid attacks the zinc and the three atoms of very loosely combined oxygen exercise a depolarising effect by absorbing the evolved hydrogen. A fine tube dips into the exciting liquid and is so arranged that it conducts a current of air, from a small pair of bellows, against the face of the negative plate; by this means any bubbles of hydrogen are, as it were, brushed off, and the current obtained from a given electromotive force is materially augmented, since the resistance is diminished. Mr. Preece then referred to several old forms of battery in which such an agitating principle is introduced, notably those of Grenet, Chutaux, and Comacho, and he went on to describe a series of experiments he has made with a view to ascertain the cause of the great heating and illuminating effects that could be obtained with the apparatus exhibited. He showed that the effects were due to the mechanical agitation of the liquid on the face of the negative plate; but whether the great production of heat in the battery, and the great lowering of its internal resistance were chemical, thermal, or electrical effects, remains for further investigation. By means of a small battery of four cells, in which the plates were 4 inches by 2 inches, a length of 6 inches of platinum wire, No. 18 (0·05 inches), could be heated to bright redness, and much more powerful effects were obtained by a large battery of ten cells made by Ladd; in this case, about 2 feet of a No. 14 (0·089 inches) wire were heated, and it was shown that, when connected with an 18-inch inductorium, kindly lent by Mr. Spottiswoode, sparks of over 17 inches could be obtained, but this length was reduced to about 8 inches on stopping the current of air. A similar effect was also very marked when the poles were connected with two carbon points, the light given out when the air-current was introduced being remarkably bright and steady.—Mr. Preece then exhibited an ingenious method of showing the vibrations of a telephone plate to an audience, which has been devised by Mr. H. Edmunds. A vibrating plate is employed to break contact as in Reiss's original telephone, and is introduced into the primary circuit of a small induction coil. The induced current is employed to illuminate a rapidly-rotating Gassiot's tube, and, on making and breaking contact by speaking into the resonator, an illuminated star is observed, the number of whose arms varies with the pitch of the note; with a very low note it may resolve itself into a single straight line.—Lord Rayleigh exhibited and explained an arrangement which he has employed with advantage in certain acoustical experiments, in order to secure absolute uniformity in the rate of rotation of an axle. After referring to the mathematical principles involved in such a problem, he explained that the only hope of its solution lay in the employment of a vibratory movement, which by some suitable device must be converted into a motion of rotation. The axle whose motion it is required to maintain uniform is usually driven, at an approximately uniform rate, by means of a small horizontal water-wheel, or, in some cases, the electro-magnetic regulating apparatus presently described is sufficient by itself to supply the necessary power. At equal distances round the axle are arranged four soft iron armatures which successively come in front of the poles of a horse-shoe electro-magnet placed in the circuit of a four-cell Grove's battery. The current is rendered intermittent by the following arrangement. Passing into the body of a tuning-fork vibrating about forty times per second, it leaves by a small platinum stud which is touched at each vibration of the fork; the current then traverses a second small electro-magnet between the prongs, and by this means the vibrations are maintained; passing to the magnet above referred to the current then returns to the battery. The velocity of the axle is such that it performs about one complete revolution for every four vibrations of the fork, and the exact adjustment is effected as follows. If the driving power be just sufficient to produce the desired speed, the armatures will be so attracted by the magnet as to be exactly opposite to it at the middle of its period of magnetisation, and so long as this position is maintained the magnet will not (on the whole) affect it. But if a disturbance occur in the driving power the armature will be displaced from its former position and will be attracted by the magnet until the error is compensated. Besides the armatures this axle also carries, concentric with it, a hollow metallic ring filled with water, and as this possesses a certain momentum in virtue of its rotation, it will act as a drag tending to check the velocity in case it increases, and in the converse manner when a diminution occurs. A blackened disc perforated with rings of holes of various numbers also rotates with the axle and by placing the eye behind the ring of *four* holes and observing a prong of the fork it is easy to ascertain whether the uniformity is maintained, since in that case the prong will appear to remain stationary.

Entomological Society, May 1.—H. W. Bates, F.L.S., F.Z.S., president, in the chair.—Henry John Elwes, F.L.S., F.Z.S., of Cirencester, was elected an ordinary member, and Mr. Peter Cameron, jun., was elected a subscriber. Mr. Dunning drew attention to the fact that the present meeting marked the forty-fifth anniversary of the foundation of the Society.—Mr. Distant exhibited a specimen of the Hemipteron, *Tetroda bilineata*, Walk., as a remarkable instance of immunity from the effects of damp, the same having been kept in a relaxing-pan for more than four months.—Mr. Distant also communicated a paper "Notes on some Hemiptera-Homoptera, with Descriptions of New Species," in which he drew attention to the uncertainty of generic calculations as to geographical distribution, the Homoptera affording a good illustration in the family Cercopida, especially the genus Cercopis. Part 1 of the *Transactions* for 1878 was on the table.

Royal Microscopical Society, May 1.—H. J. Slack, president, in the chair.—Four new Fellows were elected, and Professor Abbe, of Jena, was elected an Honorary Fellow of the Society.—A paper by Mr. Michael, on new British Cheyleti was read by the Secretary. It minutely described the structure and habits of the insect, and was illustrated by drawings. The name proposed by its discoverer was *Cheyletus flabellifer*.—Mr. Chas. Stewart gave a *résumé* of a paper which had been received from Dr. Oscar Schmidt, of New Orleans, in continuation of a former communication on the blood-corpuscles of Amphiuma, frog, and man. The president suggested to the meeting a series of experiments, which he thought might be of value in the interpretation of optical images, by the examination of microscopic drawings of Lissajous curves under various powers. He also brought before the notice of the Fellows a species of fungus which he had found infesting the leaves of the bay, but which did not appear to derive its nutriment from the leaf itself. After some discussion, the fungus was identified by Dr. M. C. Cooke as *Capnodium*, Footii, which was stated to live upon the honey-dew found upon the surface of the leaves of a large number of trees, particularly in the autumn months.

Institution of Civil Engineers, May 7.—Mr. Bateman, president, in the chair.—The paper read was on the construction of steam boilers adapted for very high pressures, by Mr. James Fortescue Flannery.

WELLINGTON, N. Z.

Philosophical Society, December 11, 1877.—Mr. Carruthers, C.E., the vice-president, occupied the chair.—Dr. Buller read a further paper on the ornithology of New Zealand. Among the species treated of were the Kaka parrot, with an interesting account of the Maori mode of trapping it by means of decoy birds; the two species of migratory cuckoo, with observations on their parasitic habits; the black fantail, the occurrence of which as far north as Auckland has been communicated by Mr. Cheesman; the knot (*Tringa canutus*) which has lately been met with in this island; the sandpiper (*Limnocinchis acuminatus*), and many others. Among the latter

was the New Zealand godwit, of which the author gave an interesting sketch. This bird spends a portion of the year in Siberia, and visits in the course of its annual migration the islands of the Indian Archipelago, Polynesia, Australia, and New Zealand. In summer it frequents the south coast of the Sea of Ochotsk, and it has likewise been observed in China, Japan, Java, Celebes, Timor, Norfolk Island, and the New Hebrides. They leave New Zealand towards the end of March or beginning of April, and return to us towards the end of November.—On *Nephrodium decompositum* and *N. glabellum* by T. Kirk, F.L.S.—On *Hymenophyllum montanum*, a new species discovered by Mrs. Mason in the mountains between Lake Wakatipu and the West Coast, by T. K. Kirk, F.L.S.—On the relative ages of the Australian, Tasmanian, and New Zealand coalfields, by Dr. Hector, F.R.S. The speaker's remarks were illustrated by diagrams and maps, and by a large collection of fossils which he had obtained during a recent tour in the Australian colonies. After describing the extent and position of the various coalfields at present worked, he stated that from a comparison of the fossils he had arrived at the following results :—*Cretaceous epoch:* Chief New Zealand coal; wanting in Tasmania and Australia, except perhaps in Queensland. *Jurassic epoch:* Mataura series of New Zealand; Cape Paterson coalfields of Victoria; Clarence River coal of New South Wales; and the coalbeds at Hobarton. *Liassic epoch:* Clent Hill beds of New Zealand; wanting in Tasmania and Australia, except Queensland. *Triassic epoch:* Wairoa beds of New Zealand; upper coal formation of New South Wales; and wanting in Tasmania. *Permio-carboniferous:* Maitai series of New Zealand; lower coal formation of New South Wales; Mersey coalfields of Tasmania. This view of the relative ages of those formations had just received remarkable confirmation by a late discovery. Mr. McKay, of the Geological Survey, who has recently been at work in the Canterbury Alps, having found plant beds beneath the spirife beds of Mount Potts that are full of the leaves of glossopteris, a fern very characteristic of the upper and middle coal formation of New South Wales, and with them beds of graphite of considerable commercial value, which represents in an altered form the Newcastle coal seams. Along with these occur remains of saurian reptiles of immense size, of which large collections have been made. In conclusion, it was stated that only a very small portion of the area coloured on the map of New South Wales as coal formation contained valuable coal seams, and that they were not without drawbacks. At Newcastle, where the principal collieries are situated, the seams have to be worked to an increasing depth by shafts, and require pumping. In the southern coalfield the coal is worked by adits into the face of the mountain, and lowered by steep inclines in the same manner as our own Buller coal will be worked; but it has to be shipped from an exposed coast. The western district coal has all to be carried over the Blue Mountains by a railway that ascends and descends by zigzags, that answer well enough for passengers and light traffic, but must be rather costly for transporting coal. Dr. Hector stated that all he had seen increased his confidence in the value of our West Coast coalfields, both as regards the quality and extent of the coal and the facilities for working it.

PARIS

Academy of Sciences, May 13.—M. Fizeau in the chair.—The following among other papers were read :—Observation of the transit of Mercury, on May 6, at Montsouris Observatory, by M. Mouchez. The observations were vague, owing to the bad weather, but so far confirmed the theory of Mercury. M. Picard got three photographic images, two of which seemed very good, but there was no trace of the planet. M. Mouchez was struck with the much more rapid succession of the phases in the transit of Mercury than in that of Venus; the times of contact should thus be obtainable with greater accuracy.—Researches on the law of Avogadro and Ampère, by M. Wurtz. Bioxalate of potassium becomes hydrated much in the same way in an atmosphere of hydrated chloral, and in one of moist air or chloroform, containing aqueous vapour with the same tension as the atmosphere of hydrated chloral. M. Wurtz infers that the latter is entirely dissociated.—M. Du Moncel read a paper on the Hughes microphone, from information communicated by Mr. Crookes.—Report on two memoirs of M. Dien, concerning (1) defective notes of instruments played with a bow, (2) resonance of the minor seventh in the grave chords of the piano. By placing small movable nuts on the short prolongations of the strings above the bridge, so as to tune those parts to the unison or octave of certain defective notes known in the violin, M. Dien gets rid of the disagreeable effect of the latter. In the second memoir he shows the resonance in question to be due to pressure of the damper on touching one of the nodes which produces the triple harmonic minor seventh. He employs a second damper acting simultaneously with the other.—On the refraction of organic substances in the gaseous state, by M. Mascart. It appears, generally, that no method based on the sole consideration of elementary composition enables one to calculate the refraction of a compound from those of its constituents. The notion of equivalents of refraction does not apply to gases any more than to solids or liquids. Each case has its special considerations, not easily determined.—On the production of sulphurised oils having insecticide properties, by MM. De La Loyère and Muntz. The considerable amount of combined sulphur found in fetid oils got by distillation of the bituminous limestone of Orbagnoux, the authors augment by introducing sulphate of lime or pyrites into the mineral before distillation.—On the telephone, by M. Izarn. A curious case of intelligible sounds being produced in one single-wire telephone system, by a derivation of current from other systems through wet ground and a system of pipes.—On a new electric lamp with incandescence, acting in free air, by M. Reznier. If a thin rod of carbon, pressed laterally by an elastic contact, and pushed in the direction of its axis, against a fixed contact, be traversed by a pretty strong current, it becomes incandescent at this part and burns, growing thinner towards the end. As the end gets used up, the rod, still pushed, slides in the elastic contact, always pressing against the other. The heat developed by passage of the current is greatly increased by the combustion of the carbon.—On a production of heat by chemical action, by M. Phipson. If a piece of chloride of lime be held in a rapid current of sulphuretted hydrogen from a narrow glass tube, the smell of sulphuretted hydrogen at once disappears, and is replaced by that of chlorine; a very light deposit of sulphur is formed on the piece, which becomes too hot to hold in the fingers. The reactions here are notable.—Action of aqueous vapour on hydrocarbons raised to red temperature, by M. Coquillion. It facilitates their dissociation while producing a fall of temperature, which in blast furnaces is added to that caused by transformation of carbonic acid into carbonic oxide.—On investigation of ozone in atmospheric air, by M. Daremberg. He finds ozonoscopic researches useless. An ozonograph should be used which should expose the paper only during a few minutes.—Observations of the moon, made with meridian instruments of Paris Observatory during 1876, by M. Villarceau.

CONTENTS

PAGE

THURSDAY, MAY 30, 1878

BALFOUR ON ELASMOBRANCH FISHES

A Monograph on the Development of Elasmobranch Fishes. By F. M. Balfour, M.A., Fellow and Lecturer of Trinity College, Cambridge. (London: Macmillan and Co., 1878.)

MR. BALFOUR has finally completed and issued in the form of an octavo volume the researches on the embryology of the dog-fish and its allies, which he commenced at the now celebrated zoological station of Naples in 1874. His results have been made known from time to time during the progress of his work by a preliminary paper in the *Quarterly Journal of Microscopical Science*, October, 1874, and by a series of articles in the *Journal of Anatomy and Physiology*, the latter, indeed, being identical with the successive chapters of the present volume. Looking at the work as a whole, we may heartily congratulate not only Mr. Balfour, but English science, on the very great value of this contribution to knowledge. Mr. Balfour, before entering upon the study of the deveopment of the shark-like fishes, had thoroughly qualified himself for the task by a careful investigation of the development of the common fowl, a subject which, although it had always been and remains the favourite, because the most handy, for the embryologist's study, yet yielded several new and interesting results to Mr. Balfour's examination. The methods which are applicable to the hardening and slicing, staining and clarifying of the embryo chick are precisely those which it is necessary to employ in the investigation of the very similar egg of the Elasmobranchs, and accordingly Mr. Balfour had well trained himself for the latter task. The prominent position in Vertebrate morphology which had been assigned to the group of Elasmobranch fishes, through the researches of Gegenbaur, rendered a minute examination of their developmental history urgent. It had become clear that we have in these fishes the nearest living representatives of the common ancestors of the great group of Gnathostomous Craniate Vertebrata, and it was to be expected that a full knowledge of their ontogeny or individual development would carry us yet further back in the line of primitive Vertebrata, and yield a mass of explanatory evidence, exhibiting the development of complex and heterogeneous structures from simpler and more homogeneous forms, likely to serve as a satisfactory starting-point for all Vertebrate morphology.

Mr. Balfour has shown in the course of his investigation of this subject not only that he is possessed of the technical skill necessary for the manipulation of such embryos, but that he is gifted with a very large measure of patience and perseverance, and has, moreover, the high critical and speculative capacities which the subject demands for its full and successful treatment.

We shall very briefly notice the successive chapters of Mr. Balfour's monograph, and point to the more important novel observations recorded, having especial regard to those which may be considered as fundamental for the morphology of Vertebrata.

Mr. Balfour begins with the ovarian ovum of the Elasmobranch, this portion of his observations having been made on the skate. He shows that the germinal vesicle atrophies before impregnation. He then proceeds to describe the process of segmentation, which, in its general features, is similar to that of the bird, the only other egg containing so large a proportion of food-material mixed up with the protoplasm of the egg-cell. In the study of the division of the first-formed cells resulting from the segregation and cleavage of the mixed materials of the egg, Mr. Balfour observed and has figured the remarkable spindle-shaped condition of the nuclei, which since has become such a prominent subject of investigation through the initiative of Auerbach, Strasburger, Bütschli, and van Beneden. Very remarkable and important nuclei are also described and figured as making their appearance in that part of the egg *not* concerned in the process of cleavage or the formation of the primitive disc of embryonic cells, and from their occurrence Mr. Balfour is led to the conclusion that the supposed distinction at this period of a purely embryonic and a purely nutritive region in the egg of the Elasmobranch, is imaginary. This is important, because similar observations have necessitated the abandonment of similar erroneous divisions of the egg of the fowl, of osseous fish, and of cephalopods. The mass of cells which form the small commencement of the embryo on the surface of the great unsegmented yelk-mass divide into an ectoderm and "lower layer-cells," and a true "segmentation cavity" comparable to that of the frog is described. The most important of Mr. Balfour's observations and suggestions which have a general bearing upon the formation of the embryonic cell-layers throughout the Animal Kingdom are those in which he points out and gives its probable significance to the fact that in the Elasmobranchs the primitive alimentary cavity (archenteron) arises as a sort of in-pushing beneath the hinder end of the embryo, a cavity being there formed between the "lower-layer-cells" and the nucleated yolk. The orifice of this cavity is spoken of by Mr. Balfour as the "anus of Rusconi," and is identified by him accordingly with the orifice so named in Amphibians. At the same time it is *not* at this orifice that the final closure of overgrowing ectoderm or epiblast takes place, that is to say, of that layer of cells which, increasing by division, spreads from the cleavage disc so as to gradually cover in the whole of the large surface of uncleft yolk. The gradually narrowing margin of these epibolic cells does not in sharks have a centre coincident with the anus of Rusconi; in fact, the blastopore, as the orifice bounded by this gradually narrowing margin is termed, lies behind the embryo altogether. Mr. Balfour suggests (and it is necessary to remember that his statements on this subject were first published three years ago) that *the primitive-streak of the bird's blastoderm is a rudimentary representative of this portion of the blastopore;* it seems necessary to say "this portion," and not the whole blastopore, as Mr. Balfour does; for tracing these various structures back with Mr. Balfour to the blastopore of the Amphioxus, we must admit that in the meroblastic ova of Sharks and Birds the blastopore has become greatly extended along the median line and has its most *anterior* portion represented in the anus of Rusconi of the Elasmobranch, a *middle* portion in the orifice of final closure of the elasmobranch's blastoderm and the primitive streak of birds and mammals, whilst a more *posteriorly* placed extension of the same

structure (blastopore) is seen in the actual orifice of final closure of the bird's blastoderm at the antembryonal pole of the yelk-sac. The continuity of the nervous and alimentary tubes, after closure of the Rusconian anus, is a striking feature which Mr. Balfour shows to be common to Elasmobranchs, Ganoids, Osseous fishes, Amphibians, Amphioxus, and Ascidians. To Kowalewsky we are indebted for the first observation of this remarkable disposition in various types of lower Vertebrata, and its full significance is not yet understood.

The next point of great importance which we find in Mr. Balfour's monograph is the derivation of the notochord from the hypoblast or archenteron, from which also the protovertebræ are developed constituting the mesoblast. That the vertebrates' body-cavity, like that of other animals, was primitively a portion of the alimentary cavity appears likely from this observation, coupled with Kowalewsky's more recent results as to the development of Amphioxus, whilst it also seems likely that the notochord made its first appearance as an organ appertaining to the alimentary tract, from which it became gradually separated in function and in structure.

The next thing which we come to is of even more special interest for the limited department of Vertebrate morphology. The *unpaired* and the *paired fins* alike make their first appearance in the Elasmobranchs as lateral ridges of epiblast, and Mr. Balfour accepts the hypothesis that the limbs are remnants of continuous lateral fins. The muscles of the limbs are shown to be derived from the "muscle-plates" of the body which develop from protovertebræ.

It is to the nervous system that some of Mr. Balfour's most original and important observations have reference. He has elsewhere conclusively shown that, contrary to Stieda's statements and in accordance with Owsjannikow's, the spinal nerves of Amphioxus have no anterior roots, that is have only dorsal roots. He now shows that the early condition of the spinal nerves of Sharks agrees with this, they having at first no anterior roots. An enigmatical commissure parallel to the medulla unites the posterior roots in the embryo. The cranial nerves—exclusive of the first and second, and the nerves to the orbital muscles which have peculiar features of their own—are shown to retain permanently the primitive condition implied in the absence of anterior roots. The vagus nerve is shown to be the result of the morphological fusion or concrescence of several segmental nerves—their separate roots (which are all dorsal ones) being "caught" (so to speak) in the sharks in process of disappearance. The identity of the nature of these roots with those of the following spinal nerves is shown by the connection with them of the enigmatical commissure above mentioned.

The segments which are represented in the Vertebrate head have been reduced and blurred by the integration of that region of the axis, but by the aid of the embryonic relations of the cranial nerves, and of a very important and remarkable series of cavities representing the body-cavity of the head (the terms are not contradictory since "head" is chiefly developed from "body") in a segmented condition, which Mr. Balfour has discovered in the Elasmobranchs, he is able to indicate distinctly at least seven post-oral segments in the cephalic axis, and he adduces cogent reasons for supposing that a larger number existed, and have been suppressed by a kind of integration.

As to the brain, Mr. Balfour gives important evidence against the fanciful interpretations of Miklucho-Maclay, whom, strangely enough, Gegenbaur has followed. What most persons call mid-brain, Miklucho-Maclay has identified with the thalamencephalon or twixt-brain (*Zwischenhirn*) of other Vertebrates, being induced by the large size of what is usually called the Elasmobranch cerebellum to consider it as the mid-brain. Mr. Balfour gives strong embryological evidence against this view.

As to the relation of nerves to the primitive germ layers, it is shown (in accordance with Hensen's observations in Mammalia) that the spinal nerves are *outgrowths* of the medulla, and Mr. Balfour, though he is unable actually to demonstrate it, yet brings a variety of evidence to show that the whole growth of the nerves is a centrifugal one, and that therefore the peripheral elements of the nervous system *may* have the same primary origin as have the central.

The important question as to how the axial medulla arose, and whether it is homogeneous with the ventral nerve-cord of Annelids and Insects is discussed in the light of the facts ascertained as to the development of the nerve-medulla in dog-fish. Mr. Balfour on the whole favours the view that the nervous system of elongated animals consisted primitively of two lateral cords, and that in Annelids and Insects these cords have met and fused *below* the alimentary tract, whilst in Vertebrates they have met and fused *above* the alimentary tract.

A curious modification of a part of the nervous system, the meaning of which is as yet entirely beyond the most hazardous speculation of either physiologist or morphologist, is shown by Mr. Balfour to present itself in the supra-renal bodies. They develop from ganglia of the sympathetic portion of the nervous system.

Lastly, we have to mention the series of results relating to the origin of the renal organs and the ducts of the generative system. These are already the most widely known and discussed, though possibly not actually the most important of Mr. Balfour's numerous discoveries. The fact that Prof. Semper, of Würzburg, occupied himself with the investigation of the renal organs of Elasmobranchs at the same time as did Mr. Balfour, and that the two investigators nearly simultaneously arrived at the same results, has given a special value to this part of the observations embodied in the present monograph. Mr. Balfour shows that the Vertebrate kidney is a condensation of tubules, of which primitively one pair existed in each segment of the body, opening into the body-cavity each by a ciliated funnel, and therefore exactly comparable to the segmental-organs or nephridia of the Annelids. Whether, as Gegenbaur holds, these organs were originally a simple pair which became segmented, that is, provided with a separate funnel in each metamere or body-segment, or whether each tubule or nephridium originally opened to the exterior, so that an unconnected series of nephridia existed on each side of the body—a pair in each segment—which subsequently became joined to one another by longitudinal common ducts—one on each side of the body—is still matter for consideration.

The adaptation of the most anterior funnel to the

purposes of an oviduct, and of a portion of the middle tubules to those of sperm-ducts is what the observations of Balfour and Semper have established—and more especially the open funnel-like character of the tubules to begin with.

Minor details and important confirmations of the more familiar facts of Vertebrate development I have not space to mention here, the whole series of embryonic phenomena is described with more or less detail by Mr. Balfour, and I have singled out the more striking facts and speculations of the monograph for brief notice.

In commenting on such a work as this, it is strongly brought to one's perception that the method of publication of the results of such laborious investigations is necessarily very imperfect—and subject to a serious deficiency in logical continuity and artistic effect. Mr. Balfour has studied the very widely-diverse phenomena of interest which the developing Elasmobranch presents from the first separation of the egg up to the nearly complete formation of all its organs. In order to state *all* the different results he has obtained, he is obliged, as is usual in embryological monographs, to give them in historical sequence. To the experienced student of embryology this method of statement and the presentation of drawings copied from actual sections and specimens is sufficient. It would be impossible to publish observations within a reasonable period of the date of investigation by pursuing any other method of statement. And yet the monographical and historical method, together with the objective "nature-true" drawings of sections is one which prevents an author from fully exhibiting the import of his observations, and from duly imparting to the reader in a clear and simple form what is, after all, the thing which the reader desires to know, namely, what is the net result of such observations in relation to the great questions of morphology. The fact is, there is no such thing as a science of embryology; it is not even a definite branch of a science. The development of organic form is a necessary part of the science of Organic Morphology, and the results of the study of development can be given with full clearness and in an intelligible manner only when formulated as parts of the general doctrine of the science under which they fall. The conclusion from this is, that the great value of Mr. Balfour's work will not be fully appreciated or rendered clear to the majority of zoological students until they are re-stated, not from the monographical standpoint; but from the more general point of view of Animal Morphology. This more systematic exposition of his Elasmobranch studies and of other like researches in combination with a general survey of the morphology of all groups of the Animal Kingdom as revealed by their developmental histories, we may expect before long to receive from Mr. Balfour himself in the form of a continuation of his well-known Elements of Embryology. E. Ray Lankester

OUR BOOK SHELF

Gold. By Edwin W. Streeter, F.R.G.S. Fifth Thousand (London: Chapman and Hall.)

The lettering on the cover of the book, as given above will hardly prepare the reader for the statement on the title-page, that the work is a translation and abridgment of Herr von Studnitz' "Die gesetzliche Regelung des Feingehaltes von Gold und Silber-Waaren," by Mrs. Brewer, with notes and additions by Mr. Streeter.

The work itself contains information which it is useful to possess. It embodies brief abstracts of the law of various countries concerning the standard of gold and silver wares, and discusses the question whether the manufacture of articles in the precious metals should be subject to legal control. Mr. Streeter's notes occupy 10 out of the 150 pages. He states that the system of "Hall-marking" was "instituted on the supposition that the assay and test of precious metals was a matter too recondite to render a power of adequate discrimination for so valuable a transfer of property a thing reasonably to be expected of the public generally."

This is a very obscure way of saying that, as the value of gold and silver wares could not be recognised by inspection, it was advisable that all articles should be stamped by authority. The necessity for such control has long been felt, and it was well justified in 1677 by the author of the "New Touchstone for Gold and Silver Wares," who says: "The truth is, the gain by adulterating gold and silver works is so sweet and enticing that what excuse will not these adulterators find that they may have their unlawful liberty."

In London the control has been wisely vested in the Goldsmiths' Company since the fourteenth century, and in the country there are several assay offices which were reported on by a Select Committee of the House of Commons in 1856. Mr. Streeter urges that gold of one standard only—18 carat—should be used, or that if other alloys are employed the tradesman should "mark them with his own name, state the value of the composite matter, and trust to his genius for the sale." Trusting to genius for the sale of articles is all very well, but the practice of a person stamping the wares he sells with his own mark surely affords no protection against the fraudulent tradesman as the marks are not likely to outlive the age in which they are impressed, and would be as readily counterfeited as those of a responsible authority. It should also be added that the initial or distinctive mark of the maker of an article of gold or silver is already included in the Hall mark.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Alternate and Stereoscopic Vision

With reference to Mr. Galton's observation in his instructive paper in Nature, vol. xviii. p. 98, that "sometimes the image seen by the left eye prevails over that seen by the right, and *vice versâ*," I may mention that, as I had noticed some years ago, this may be best observed without a stereoscope. If on looking at any object a few feet distant, a nearer object be placed about midway between the first and the eyes, there will of course be two images seen of the near, when the eyes converge on the distant; one of these images seen, say by the right eye, overlaps the distant object as seen by the left eye, and if the two objects be about equally illuminated (or the near one rather the brighter), the overlapping image will alternately solidify and disappear, according to the alternate waxing and waning of sensibility in the eyes. This alternation may be made at will, by desiring to see the near or the distant object; the fluctuations take place about every ten seconds normally, but the changes may be willed (though not so completely) as often as every second.

If the observer can see stereoscopically without an instrument, *i.e.*, can dissociate the usually coincident motions of focussing and convergence, this alternate action of the eyes is seen very

plainly; and not only may stereographs be combined by the eyes, more readily and with less fatigue than when using an instrument, but they may as readily be inverted (the near objects appearing distant, and *vice versâ*, as if falsely mounted) by applying each eye to the picture in front of the other, in fact, squinting at it. Thus, pictures of any size can be properly combined by reversing the pictures and crossing the eyes, and the width of the pictures is not limited to the distance between the eyes as in the ordinary way.

An important use of stereoscopic vision is to throw one eye out of use when doing delicate measurement, &c., by directing it to some other and darker object, instead of shutting it; this is less fatigue, and the attention may be willed on to the eye required, so that the image of the other is not noticed, especially if the eyes be changed occasionally.

How far the fact of the eyes changing guard naturally by alternations, may suggest that all duplicated organs of the body have alternate periods of rest, I must leave physiologists to investigate. W. M. FLINDERS PETRIE

Bromley, Kent

Inside Out

IT appears in NATURE, vol. xviii. p. 105, that "if a fourth dimension were added to space, a closed material surface (or shell) could be turned inside out by simple flexure." This implies that flexure is necessary. But without displacing a point or a line in the surface we may consistently suppose a rotation of the normals at each point of it through two right angles in a plane polar to the tangent plane. That seems to do the business. C. J. MONRO

May 28

Physical Science for Artists

MR. NORMAN LOCKYER, in NATURE, vol. xviii., pp. 59, 60, gives some valuable hints to artists, which, if carried out, will go a great way towards preventing our eyes being hurt by the lunar monstrosities we see at the Royal Academy and elsewhere.

May I be permitted to add a hint which he appears to have overlooked, and that is, that the inside boundary of a crescent moon is an ellipse; and in this consists the peculiar beauty of a true crescent. The usual Turkish crescent is struck with two circles, and always looks gouty and bad. Of course the rough edge of a gibbous moon is also an ellipse.

Scientific Club, 7, Savile Row, W., May 25 ROBERT J. LECKY

Dr. P. P. Carpenter's Collection

MAY I ask you to correct an error in the "Notes" of your number for April 25th, relating to the collection of the late Dr. P. P. Carpenter. This collection was permanently placed by him in the museum of this university; and, mounted under his direction on glass tablets, it now occupies a separate fire-proof room erected for it by the university, and constitutes one of the principal scientific treasures of this university and of Canada. Your correspondent was probably misled by the fact that one of the best duplicate sets was reserved by Dr. Carpenter for his own use in his private residence. This has not been publicly offered for sale, but I believe has been privately offered to certain persons and institutions considered likely to value it.

McGill College, Montreal, May 10 J. W. DAWSON

Menziesia Cærulea

IN confirmation of the recent occurrence of the above plant on the Sow of Athol, I may say that it was gathered by Miss Crawford in 1877, from whom I received a specimen. Like the cotoneaster on the Orme, which has also been reported extinct, careful and prolonged search has generally been rewarded by finding specimens, although the cotoneaster is now very rare. I might take this opportunity of saying that the rare spider orchis, *Ophrys aranifera*, which the Rev. M. J. Berkeley has gathered at Southorpe, in Northants, has been destroyed there by the planting of larch. I made a most careful search not only at Southorpe but on the Barnack hills last week, but without seeing a trace of the *orchis*, although *Anemone pulsatilla* and *Aceras anthropophora* are still abundant on the unplanted quarries.

Northampton Natural History Society G. C. DRUCE

Landrails

IT would prove very interesting to know whether landrails are as plentiful in other parts of the country this season as they are in the neighbourhood of Sheffield. They have not visited us in any numbers since the spring and summer of 1875; in 1876 and 1877 scarcely one was heard; while at the present time we hear their well-known calls in almost every meadow. I know of no migratory British bird in whose case this peculiar irregularity of appearance occurs in such high degree as in the landrail.

If the advice of one interested in the subject may be humbly offered, I would recommend ornithologists to pay strict attention to this matter, this season, with a view of elucidating this peculiar trait in the life-history of this singular bird; for the cause of this irregular appearance has, for aught I know to the contrary, yet to be learned. CHARLES DIXON

Heeley, near Sheffield, May 20

Hereditary Transmission

THE letter of Mr. Watt reminds me of a similar instance of "Hereditary Transmission" mentioned in the ninth edition of the "Encyclopædia Britannica."

It is there stated that "George Bernhard Bilfinger was born on January 23, 1693, at Cannstadt, in Würtemberg. His father was a Lutheran minister. By a singularity of constitution, hereditary in his family, Bilfinger came into the world with twelve fingers and as many toes."

After being a Professor of Logic at St. Petersburg University Bilfinger became one of the "best and most enlightened ministers" of state that Würtemberg had yet produced.

Burngreave Road, Sheffield, May 25 GEORGE S. WATSON

THE PHONOGRAPH AND ITS FUTURE

WHAT a surprise is in store for the children next Christmas if Mr. Edison's expectations are realised. Dolls that can say "papa" and "mamma," will be quite at a discount and will bear much the same relation to the doll of the future that the anthropoid ape does to the man of to-day, and the time will probably have come for some Darwinian toy-maker to write the history of doll development, if, indeed, he does not extend his researches to the whole world of toys. We are promised dolls that can speak, sing, cry, laugh; musical-boxes that will grind out the voice and words of the human singer; locomotives and every other species of "animal and mechanical toy," that will give out their natural and characteristic sounds.

But these are only some of the trifles to which Mr. Edison, in an interesting article in the current *North American Review*, tells us his miraculous invention will certainly or probably be put in the near future. And, indeed, a very little consideration will show that there is no end to the uses to which the principle of the phonograph may be applied; that it may be the means of actually realising some of the wildest dreams and speculations of the "frenzied" poet and preacher, and creating a revolution in human intercourse only to be paralleled by the invention of printing, or even of speech itself. Indeed, at first sight it may seem a step backwards, as it is likely to lead to the abolition, to some extent, of writing and printing, and the substitution of the human voice as the main means of intercourse at a distance. Talk of the solidification of the gases! Why we have here the solidification of something infinitely more impalpable—human words and human thought. We referred above to the musical-box of the future, and this suggests the phonographic barrel-organ, which will doubtless by and by take the place of that instrument of torture which makes the lives of delicate-eared artists and *littérateurs* miserable. Instead of having our musical sensibilities harrowed by a murdered reproduction of our favourite operatic air, or our political sympathies shocked

by some wretched effusion of the Jingoid type, we shall have those picturesque Italian girls, with their bandit-looking companions, turning out for us a ballad by Sims Reeves or Santley, or a witching air in the voice of Patti. Alas! the invention came just too late to preserve to us for ever the matchless voice of Titiens, for now we need not wish in vain for "the sound of a voice that is still."

Music inevitably suggests love, and the tender cooings of the "lover and his lass, with a heigh and a ho and a heigh no nino." No longer will the far-separated pair have to wait weary weeks or months for a clumsy letter, when phonograph offices are as plentiful as telegraph stations; and when Mr. Edison has managed to make those improvements on the instrument of which he is confident, it will be quite possible for the fond pair to have a daily meeting and exchange across the world all sorts of tender cooings—for sounds of every kind can be registered on and given out by the phonograph.

Mr. Edison tells us that for these and similar purposes he is now perfecting the instrument in mechanical details. "The main utility of the phonograph, however, being for the purpose of letter-writing and other forms of dictation, the design is made with a view to its utility for that purpose.

"The general principles of construction are a flat plate or disk, with spiral groove on the face, operated by clock-work underneath the plate; the grooves are cut very closely together, so as to give a great total length to each inch of surface—a close calculation gives as the capacity of each sheet of foil, upon which the record is had, in the neighbourhood of 40,000 words. The sheets being but ten inches square, the cost is so trifling that but 100 words might be put upon a single sheet economically.

"The practical application of this form of phonograph for communications is very simple. A sheet of foil is placed in the phonograph, the clock-work set in motion, and the matter dictated into the mouth-piece without other effort than when dictating to a stenographer. It is then removed, placed in a suitable form of envelope, and sent through the ordinary channels to the correspondent for whom designed. He, placing it upon his phonograph, starts his clock-work and *listens* to what his correspondent has to say. Inasmuch as it gives the tone of voice of his correspondent, it is *identified*. As it may be filed away as other letters, and at any subsequent time reproduced, it is a perfect *record*. As two sheets of foil have been indented with the same facility as a single sheet, the 'writer' may thus *keep a duplicate* of his communication.

"The phonograph letters may be dictated at home, or in the office of a friend, the *presence* of a stenographer *not being required*. The dictation may be as rapid as the thoughts can be formed, or the lips utter them. The recipient may listen to his letters being read at a rate of from 150 to 200 words per minute, and at the same time busy himself about other matters. Interjections, explanations, emphasis, exclamations, etc., may be thrown into such letters, *ad libitum*.

"The advantages of such an innovation upon the present slow, tedious, and costly methods are too numerous, and too readily suggest themselves, to warrant their enumeration, while there are no disadvantages which will not disappear coincident with the general introduction of the new method."

Then as to books there seems some chance that, ere long the printer's, if not the publisher's, occupation will be to a great extent gone, and the present unwieldy form of communication between an author and his readers be abolished. What would not one give to have the "Christmas Carol" bottled up for ever in Dickens's own voice to be turned out at pleasure? Books, as Mr. Edison truly says, would often be listened to where none are read, and the possibilities of the instrument in this direction may be learned from the fact that a book of 40,000 words might be recorded on a single metal plate ten inches square. We need not point out the uses to which the invention might be put for the preservation of the greatest efforts of our greatest orators, but when Mr. Edison speaks of our thus collecting and preserving "the last words of the dying member of the family" and of great men, we feel as if he were approaching both the ludicrous and the shocking.

Then the compositor will be able to set up his type by ear instead of eye, and we shall have phonographic clocks which "will tell you the hour of the day, call you to lunch, send your lover home at ten," &c.

"Lastly, and in quite another direction, the phonograph will *perfect the telephone*, and revolutionise present *systems of telegraphy*. That useful invention is now restricted in its field of operation by reason of the fact that it is a means of communication which leaves no record of its transactions, thus restricting its use to simple conversational chit-chat, and such unimportant details of business as are not considered of sufficient importance to record. Were this different, and our telephone conversation automatically recorded, we should find the reverse of the present status of the telephone. It would be expressly resorted to *as* a means of perfect record.

"'How can this application be made?' will probably be asked by those unfamiliar with either the telephone or phonograph.

"Both these inventions cause a plate or disc to vibrate, and thus produce sound-waves in harmony with those of the voice of the speaker. A very simple device may be made by which the one vibrating disc may be made to do duty for both the telephone and the phonograph, thus enabling the speaker to *simultaneously transmit and record his message*. What system of telegraphy can approach that? A similar combination at the distant end of the wire enables the correspondent, if he is present, *to hear it while it is being recorded*. Thus we have a mere passage of words for the action, but a complete and durable record of those words as the result of that action. Can economy of time or money go further than to annihilate time and space, and bottle up for posterity the mere utterance of man, without other effort on his part than to speak the words?

"The telegraph company of the future—and that no distant one—will be simply an organisation having a huge system of wires, central and sub-central stations, managed by skilled attendants, whose sole duty it will be to keep wires in proper repair, and give, by switch or shunt arrangement, prompt attention to subscriber No. 923 in New York, when he signals his desire to have private communication with subscriber No. 1001 in Boston, for three minutes. The minor and totally inconsequent details which seem to arise as obstacles in the eyes of the groove-travelling telegraph-man, wedded to existing methods, will wholly disappear before that remorseless Juggernaut—"the needs of man;" for, will not the necessities of man surmount trifles in order to reap the full benefit of an invention which practically brings him face to face with whom he will; and, better still, doing the work of a conscientious and infallible scribe?"

Mr. Edison is certainly very hopeful of the future of the wonderful instrument he has invented, but we think, not too hopeful; for, after the invention itself and its most recent development, the microphone, it would be rash to say that any application of it is impossible. Certainly some substitute or substitutes for the clumsy mode of recording our thoughts by pen and ink, so inconsistent with the general rapidity of our time, must be close at hand; and what form one of these substitutes may take seems pretty clearly pointed out by the actual uses to which Mr. Edison's invention has been put.

THE ENGLISH ARCTIC EXPEDITION[1]

THE edge has been to some extent taken off the public appetite for a narrative of our last great Arctic expedition. The two ships had barely touched the Irish shores ere the papers of the day were teeming with details of the adventures and results of the expedition that had left England scarcely eighteen months before amid the enthusiasm of the nation, and with the strongest expectations of eclipsing all previous expeditions, and returning with the long-sought-for secret of the pole. These newspaper narratives were shortly followed by Capt. Nares's report (which we gave in full with a map in NATURE, vol. xv. p. 24), followed some months after by a thick Arctic blue-book, which those who have seen it may prefer, with its wealth of maps and illustrations, even to the two handsome volumes before us. (See NATURE, vol. xv. p. 505.) Under these circumstances it will not be necessary for us to repeat the story of the *Alert* and *Discovery*. We shall endeavour briefly to sum up the main results obtained by the well equipped and much instructed expedition.

Many a wonderful story lies buried in a blue-book; comparatively few, we believe, have seen the official narratives to which we refer above. The great majority of those, both at home and abroad, who are interested in the expedition commanded by Sir George Nares, have no doubt been waiting for the publication of these volumes, to learn all the details of the story of the hardships endured by our ever-brave sailors "far from all men's knowing," in the most inhospitable region under the heavens. The red-tapeism and stupid conservatism of our government are in nothing more forcibly exhibited than in their obstinate adherence to the unattractive "blue-book" for publications of all kinds that may be considered in any way official. In this respect they present a marked contrast to the United States Government, the story of whose *Polaris* expedition was issued not long ago in a magnificently got-up volume that would do credit even to Messrs. Sampson Low and Co.; and many of our readers must be familiar with the splendid library, issued at the expense of the Austrian Government, on the productive *Novara* expedition.

We are sure Sir George Nares does not expect to be complimented on his skill as a *raconteur;* he has wisely not attempted to do more than give a plain statement of the proceedings of the expedition day by day from the time it left England till its return. Those of our readers who have read the eloquent and methodical narrative of the Payer-Weyprecht expedition, when they look into the one before us, will not fail to be struck with the contrast in this respect. Still, we believe, by many, Sir George Nares's "plain, unvarnished tale" will be preferred to a carefully redacted and condensed narrative; and we are sure that in his pages the simply told successes and failures of the English Arctic Expedition of 1875-76 will fascinate many a reader: it is almost impossible to make the story of an Arctic Expedition uninteresting.

"The scope and primary object" of the expedition was, as contained in the "Sailing Orders," "to attain the highest northern latitude, and, if possible, to reach the North Pole, and from winter quarters to explore the adjacent coasts within the reach of travelling parties." Notwithstanding the ambiguous wording of these orders—no doubt "the highest northern latitude *possible*" was meant—it is a great mistake to imagine, as many did on the return of the expedition, that it was a failure because it did not reach the pole. No doubt it was a primary part of the programme to make the most determined attempt to reach 90° N. lat., and had "the People" not been allowed to believe that this was the main object of the expedition, probably their enthusiasm at its departure would have been no greater than when the *Challenger* left our shores; but, without doubt, the essential point in this matter was to get as far north as possible. No one who reads Sir George Nares's interesting but often sad pages will hesitate to conclude that if a higher latitude than that attained by the forlorn hope, led by Commander Markham, was not reached, neither officers nor men were to blame. Under hardships that could only be paralleled by those which led to the unknown deaths of the members of the Franklin expedition, was the attempt made to carry out the popular part of the programme—hardships, however, which did not surpass those endured by the sledging parties west and east under Aldrich and Beaumont. This is not the place to enter upon the question of the outbreak of scurvy, to which we have, indeed, referred in a former volume (xv. p. 505). After the searching inquiry of the Scurvy Commission; after all that has been written on the subject in the public and medical journals; and after a careful perusal of these pages, we are not inclined greatly to blame either Captain Nares or his officers for their neglect of lime-juice. Evidently we have yet much to learn about the causes and means of prevention of scurvy. All we have to do with here is the fact that under the most adverse conditions imaginable officers and men did more than could reasonably have been demanded of them—though not expected of English sailors—to carry out the purely geographical part of their orders. Markham and his men really reached the highest latitude *possible* under the circumstances, 83° 20′ 26″ N., the highest latitude reached by any expedition. "C'est magnifique, mais ce n'est pas la guerre." It was heroic, but it is not what we want.

The other part of the geographical section of the programme was carried out with equal faithfulness by the sledge parties under Lieuts. Aldrich and Beaumont, and, in the case of the latter, under even greater hardships and with greater fatality than in the case of the northern party. Lieut. Aldrich succeeded in adding to our maps a stretch of 220 miles of coast along what may be regarded as the northern boundary of America, while Lieut. Beaumont considerably extended our knowledge of the north coast of Greenland, and has given us reason to believe that it is bordered by islands. Many rectifications were made, moreover, of the geography of the coasts and islands in Kennedy and Robeson channels, and a considerably fuller and more accurate idea of the nature of the coast regions both on the east and west sides of these channels. The fact is that, geographically, there appears to be little to discover in the region around the *Alert's* winter quarters, and what is really worth knowing in this direction could only be brought to light by an expedition colonised there for some years; Capt. Howgate's proposed experiment will therefore be anxiously watched. Though it was often difficult to tell where the sea-ice ended and the land began, enough was observed both by Aldrich and Beaumont to indicate that these northern shores are mostly rocky, rising rapidly into hills and mountains, and often, especially on the Greenland side, steep and imposing, and deeply cut into by fiords. Markham saw no sign of land as far beyond his farthest north point as he could see, and seems inclined to believe that if there is land it must be a great way off.

Even had the men maintained their health and strength, it is doubtful if any of the sledging parties would have been able to do much more than they did, unless, indeed, they had been able to stay another winter, and make their furthest points bases for farther operations. The great hindrance to progress was the character of the ice which the sledge-parties had to traverse. The nature of this characteristic feature of these regions, the "palæocrystic ice," as it has been named, is already

[1] "Narrative of a Voyage to the Polar Sea during 1875-76, in H.M. ships *Alert* and *Discovery*." By Capt. Sir G. S. Nares, R.N., K.C.B., F.R.S., Commander of the Expedition. With Notes on the Natural History, edited by H. W. Feilden, F.G.S., C.M.Z.S., Naturalist to the Expedition. Two vols. (London: Sampson Low and Co., 1878.)

well known to our readers, and some further idea of it may be obtained from the specimen shown in our illustration (Fig. 1). Things were bad enough for the shore-parties, but, to judge from the description, it would be as easy to go from the Crystal to the Alexandra Palace over the tops of the houses dragging a heavily-laden sledge after you, as to accomplish what Markham and his party did. The valuable result of this expedition is an extension of our knowledge of the nature and formation of the ice which covers these polar regions. That the inconceivably rugged and hilly nature of the ice is partly due to the movement of the pack, and the consequent piling of floe on floe at all sorts of angles, there can be no doubt; but the observations of Dr. Moss (see our Royal Society Report this week) and Lieut. Parr seem to show that the immense thickness of the floes, exceeding eighty feet sometimes, is not due entirely to the piling of floe on floe. Rather it would seem that the same causes are in operation here as in the Alpine glaciers, and that on a thick substratum of sea-ice, snow-fall after snow-fall has been accumulating season after season,—for how long the daring geologist alone can hazard a guess—becoming gradually condensed into ice by pressure. What may be the limit of this process we have no data on which to build conclusions. "The Névé-like stratification, the embedded atmospheric dust, and the chemical characters of our polar floes, indicate, in my (Dr. Moss's) opinion, that they are the accumulated snow-fall of ages, rendered brackish by infiltration and efflorescence." The great "domed" floes, he thinks, tell of gradual decay, "because, wherever we got a section of them, the horizontal strata were cut by the outline of the domes, and the ice of the top of the dome was invariably salt. Occasionally deposits of atmospheric dust were to be met with throughout the stratified ice."

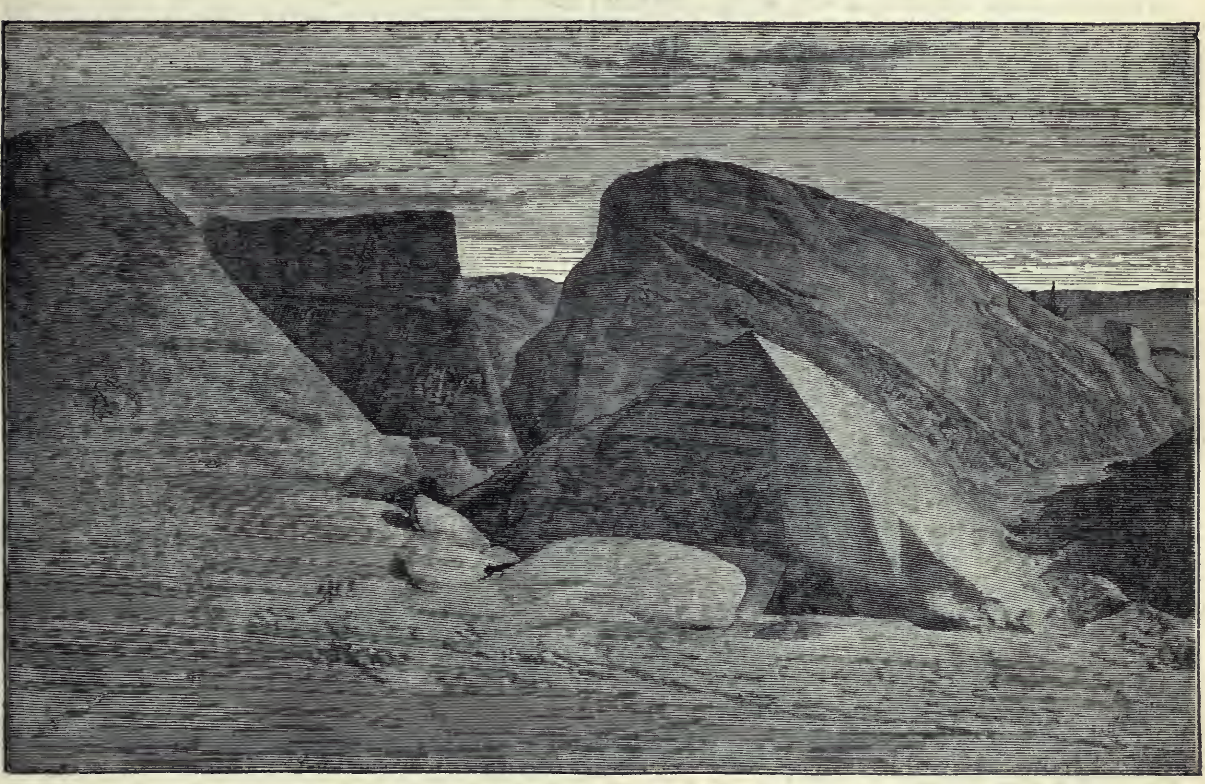


Fig. 1.—Newly-formed Floe-bergs.

As to the movements of the ice in the Polar Ocean, the expedition was unable to make any observations of consequence, though it has made some contributions to a knowledge of those of the ice in the channels through which the ships passed. The general conclusion seems to be that beyond the *Alert's* winter quarters, though there may be occasional open spaces, or polynias, the ice never breaks up sufficiently to enable a ship to pass further northwards, notwithstanding the observations made by the *Polaris* expedition. But this question of the movements of the polar ice is just one of those that can only be satisfactorily settled by a long series of observations, such as those that could be made from the ring of stations proposed to be established by Lieut. Weyprecht.

The tidal observations made, especially on board the *Discovery*, were of great value, in the opinion of Dr. Haughton, who gives an abstract of the results in the Appendix, and seem to confirm the observations made in the *Polaris* expedition:—"The expedition, proceeding up Smith Sound, met the tide coming from the north, at or near Cape Frazer, lat. 79° 40', and left behind the tides of Baffin's Bay. The new tidal wave, observed on board both ships, is *specifically* distinct from the Baffin's Bay tide, and from the tide that enters the Arctic Ocean through Behring's Straits; and it is, without question, a tide that has passed from the Atlantic Ocean, round Greenland, northwards, and then westwards."

As might be expected in these high northern regions, there were few auroral displays, and though one, at least, was remarkable, none were brilliant, and all comparatively colourless. We do not read, however, that any attempt was made to study this or any other light phenomenon by means of the spectroscope, though, we believe, several of the officers were specially instructed in the use of the instrument before the expedition set out. In this connection we may notice a most interesting solar phenomenon

exhibited on p. 300 of vol. i. (see Fig. 2), which will give the reader some idea as to how snow-blindness may be produced, and which might have reminded Capt. Nares that the expedition was provided with the instrument we speak of. When the sun reaches a certain height, above 14°, during clear weather, "the most brilliant prismatic colours are displayed by each minute snow-prism, and in combination form a sparkling arc on the snow-covered ground, the bright light from which is too powerful for the unprotected eye. The 'diamond-dust,' as we term it, becomes more open as the length of the radius is increased. Consequently, when the sun is between fourteen and twenty-three degrees in altitude, the refraction of its rays is set forth with the greatest effect, and snow-blindness has to be guarded against. In the bright arc, while each tiny prism displays its complete set of colours, the red tint is the most prominent nearest the sun, the purple lying on the outside indistinctly defined." We regret that such observations were so rare, and that so little use was made by the expedition under Capt. Nares of the fine set of apparatus for physical observations with which it was provided. This is the weak point of the expedition, and, so far as physical science is concerned, the "Arctic Manual" need hardly have been written. The 26th paragraph of the sailing orders runs:

FIG. 2.—Diamond Dust.

"The most approved instruments have been furnished to you for the purpose of pursuing research in the several branches of physical science, and as certain of your officers have been specially instructed in the modes of observing, you will take care to give them every fair opportunity of adding their contributions thereto." Very few "fair opportunities" seem to have occurred to the expedition.

But after all it is doubtful if the commander of the expedition is so much to blame. The truth is that the instructions to the expedition said more than should have been said about trying to reach the pole. What we wanted and what we still want are steady continued observations of meteorological and other phenomena in the polar area. The Royal Society might have saved itself all trouble if the instructions had been published beforehand. The comparative meagreness of the scientific results is, we believe, due more to the tone of the instructions than to Sir George Nares.

Thanks mainly to Capt. Feilden, however, the expedition has not been altogether barren in scientific results, as the Appendix filling half the second volume will testify. With the exception of the short paper on the tides by Dr. Haughton, this appendix deals with the natural history and geology of the region visited. Each of the departments of natural history, from the mammalia downwards, has been worked out by a specialist, and the results, though seldom novel, are all highly interesting. Life was found in the sea at the highest point reached, and not far from the same point the tracks of a hare were seen. Dr. Hooker has some important observations to make in connection with the flora brought home, which confirms his previous conclusions as to the essentially Greenlandish nature of the Greenland flora. He is inclined to think that vegetation may be more abundant in the interior of Greenland than is supposed, and that the glacier-bound coast-ranges of that country may protect a comparatively fertile interior. We are almost driven to conclude, he thinks, that Grinnell Land, as well as Greenland, are, instead of ice-capped, merely ice-girt lands. The geological results are fully and ably discussed by Mr. De Rance and Capt. Feilden, who indeed traverse summarily the whole ground of Arctic geology, to which their paper is a valuable contribution. Their conclusions are essentially the same as those already formed as to the very different climate that must have at one time prevailed in these regions. Dr. Coppinger's report on the great glacier that discharges into the Petermann Fjord is interesting, though his observations do not seem to agree entirely with Dr. Hooker's conclusions.

On the whole Sir George Nares's two volumes confirm the opinions we have already published with regard to this expedition. One and all exerted themselves nobly and bravely to carry out the main object of the expedition; the results, geographical and scientific, brought home are of great value, and repay to a considerable extent the outlay and the hardships endured; at the same time, now that the full narrative has been published, we must express regret that the scientific results are not more abundant than they are, and that they contrast so markedly with those of previous English expeditions, and with the expeditions of Germany and Austria, where, however, the officers are all trained men of science. Notwithstanding the results we cannot regret that the expedition was sent out; it has solved the question of Arctic exploration so far that it is clear the Pole is not to be reached by the Smith Sound route—if at all, indeed by any means hitherto tried—unless some line of land be met with that will enable the sledge to be utilised. Meantime this narrative of the last great English Expedition will prove attractive and instructive to many readers. We cannot conclude without saying a word in praise of the many fine illustrations of Arctic scenery, a number of the finest being permanent Woodburytypes. There is also a large map showing the new discoveries, and a special one of Markham's journeys.

TRANSPLANTATION OF SHELLS

It is well known that animals and plants inhabiting freshwater have, as a general rule, a very wide distribution; yet each river system, with all the pools and lakes in connection with it, seems completely cut off from every other system of the same country. Still more complete is the separation between the freshwaters of distinct continents or of islands; nevertheless they often possess freshwater species in common. In my "Origin of Species" I have suggested various means of transportal; but as few facts on this head are positively known, the case given in the adjoined letter of a living Unio, which had caught one of the toes of a duck's foot

between its valves, and was secured in the act of being transported, seems to me well worth recording.

CHARLES DARWIN

DEAR SIR,—The following case will, I think, prove of interest to you, as it corroborates your belief that freshwater shells are sometimes transplanted by the agency of aquatic birds.

In the sketch I have endeavoured to give you a correct idea of the way in which the shell was attached to the duck's foot.

It was given to me by Mr. H. L. Newcomb, who shot the bird, which was a blue-winged teal (*Querquedula discors*), while flying, near the Artichoke river at West Newbury, Mass., September 6, 1877. The shell, the common mussel, or clam (*Unio complanatus*), is a very abundant species, being found in nearly all the rivers and ponds of the Atlantic slope. How long the shell had been attached is only a matter of conjecture, but it had abraded the skin of the bird's toe, and left quite an impression. It was living when the bird was shot.

It would have undoubtedly been transplanted to some pond or river, perhaps miles from its original home, had the bird not been shot, and might then have propagated its kind.

ARTHUR H. GRAY

Danversport, Mass., May 8

To C. Darwin, Esq.

THE NATIONAL WATER SUPPLY

THE Congress convened by the Society of Arts, at the suggestion of His Royal Highness the Prince of Wales, their President—and which has been presided over by Sir Henry Cole, K.C.B., and numerously attended by Mayors of Provincial Towns, Chairmen of Local Sanitary Authorities, Medical Officers of Health, Members of the Thames Conservancy Board, Engineers, and men of science—may fairly be considered a sufficiently representative body to discuss with some amount of authority a national question.

The papers prepared at the request of the Council of the Society, and discussed by the Congress, may conveniently be divided into three groups:—the quantity of rainfall available for water-supply; the necessity of improved legislation to give it quickly and cheaply to the people; the necessity of compulsory powers being given to a government department, to carry out the amended law.

The first head, quantity available, was appropriately opened by a paper on the rainfall, by Mr. G. J. Symons, the indefatigable head of the 2,000 unpaid observers, whose results leave little to add to our knowledge upon this matter. "No part of the British Isles has, on the average, less than 20 inches of rain per annum," and "the bulk of the supply falls upon elevated mountain tracts, where it ranges from 50 to more than 100 inches per annum."

Mr. Symons did not give the results of his experience on the probable amount of the rainfall evaporated on different soils, under different atmospheric conditions, and in different parts of the country; this figure must ever be an important factor in estimating the quantity of water available in a district. Mr. John Evans, F.R.S., however, informed the congress that while on bare hard rocks nearly the whole of the rainfall is carried off by the *surface streams*, on some porous rocks, such as chalk, an average quantity of not more than six or eight inches per annum finds its way to a depth of three feet from the surface, the remainder being carried off by *evaporation*, and vegetation, and he adds "that for the supply of the population in districts of different geological character, different means must be adopted."

Numerous speakers insisted at some length on the influence of the varying degree of permeability of the rocks, in determining whether the rainfall is thrown off in *floods*, which should be collected and stored in reservoirs, or whether it is *absorbed* into the ground, where it can only be reached by wells, or by carefully preserving lines of springs. Mr. Chadwick, C.B., pointed out that the Map of the Geological Survey of the United Kingdom, and the other publications of this Department, formed an admirable basis for any inquiry into the water-bearing facilities of the British rocks. Mr. Whitaker's Memoir on the London Basin, which contains the particulars of more than 500 wells sunk in and around the Metropolis, is a good illustration of this, and useful as showing the facilities already possessed by a Government Department, which is capable of greater extension in this direction, and whose officers now constantly work in concert with those of the Local Government Board.

Mr. Lucas exhibited a useful map, showing the underground contours of the surface of the water in the chalk, of some 800 square miles of the Thames and Hampshire basins, so that the level of the water in regard to the Ordnance datum line can be seen at a glance. He has also in some cases indicated the underground level of underlying impermeable strata, a method which has been long used in the Geological Maps of Paris; and it is a matter of surprise that a map of London which should answer the purpose of a section in all directions was not published before. However useful such a map may be amongst the permeable rocks of the greensand, chalk, oolites, and new red sandstones, which are penetrated by deep wells,—in the more ancient formations, consisting almost entirely of impermeable rocks, it would be impossible to construct such a map, and the ordinary Geological Map is all that is required.

These porous secondary strata occupy an area of 26,000 square miles in England and Wales, and in Scotland and Ireland are practically absent, and wells of any depth are rare; while the more shallow wells, penetrating the overlying drift, are in all districts, as pointed out by the Rivers Pollution Commission, dangerous sources of supply, though in some cases, as Prof. Prestwich, F.R.S., pointed out to the Congress, the gradual removal of cesspools, and improvement of house-drainage, has caused the shallower well-waters to again improve. He, however, gave a remarkable instance of a retrograde character, that of a deep "dry well," being carried through the London clay, to drain a cemetery near London, into the underlying Thanet Sands, which still give an important quota to the Metropolitan potable waters.

Next to the quantity of water available, there is no question so important as the quality and purity; and on this point Dr. Frankland gave important and reassuring evidence; for though he tells us that the increasing pollution of rivers and streams "renders the supply of wholesome water from them more and more difficult," yet "two sources of wholesome water" still remain in England, viz., "upland surface water and subterranean water." The tables accompanying a paper laid before the Congress by Mr. De Rance, show that the formations yielding water of these two characters occupy the following areas in England:—

FORMATIONS YIELDING:—

Subterranean Waters.	Sq. Ms.	*Moorland Waters.*	Sq. Ms.
Permian and Trias	8,645	Granite, Metamorphic Rocks, Cambrian, Silurian, & Devonian	11,455
Oolites	6,671	Carboniferous Rocks, (without the Carb. Limestone)	10,080
Hastings Sands, Green Sands, and Chalk	11,371		
	26,687		21,535

Of the more permeable rocks constituting the first list, probably four-fifths of the area would yield unpolluted water, and receive into its mass not less than six inches of rainfall annually, or a quantity, if all yielded up to wells, of no less than 240,000 gallons per day for each square mile of area.

Of the second list the rocks are for the most part impermeable, and the most porous portion of the carboniferous generally return the water that has percolated into the strata to the same river basin; these rocks receive the heaviest rainfall of England, seldom falling below forty inches, and often rising to more than a hundred, of which quantity not less than thirty inches per annum may safely be calculated on, as the quantity run off by streams. Assuming that the rainfall is only available for water supply purposes, over one-tenth of the area, or 2,153 square miles, and that one-third of the supply is given back to the streams as compensation to manufacturers, to preserve fish, and for the purposes of inland navigation, the quantity remaining off this selected tract would be more than sufficient for the whole population of England, without recourse to the subterranean supply, which Dr. Frankland more especially recommends for domestic use; so that there can be no shadow of doubt that the quantity of water available for supply to towns and rural populations, of a standard of purity approved by Dr. Frankland, is far in excess of the requirements of our population.

The question of the amount of compensation water which should be returned to streams which are impounded for the purpose of water-supply is one of the gravest national importance. In one case, the River Roddlesworth, taken by the Liverpool Corporation Waterworks, the Legislature permitted "the compensation water," ordered to be returned to the stream by the Act of 1847, to be bought up, for the purpose of supplying a new reservoir, and thus deprived the district drained by the stream, in the words of Mr. Bateman, speaking of a similar proposal,[1] "of all possible participation in the extension of manufactures and in the commercial prosperity of the surrounding district." Mr. Bateman has strongly expressed similar views in his evidence before the Duke of Richmond's Commission, and it is with regret, we notice that though he proposes to take eventually 50 million gallons per day from Thirlmere, he only intends to return 5½[2] million gallons a day to St. John's Beck.

On the second head, the necessity of legislation to give cheaper and more easily acquired water powers to sanitary authorities, through the agency of provisional orders of the Local Government Board—which do not now possess compulsory powers to acquire water-rights, under the Local Government Act of 1875. Mr. A. H. Brown, M.P., read a paper describing the work done by the Select Committee on the Public Health Amendment Bill, of which he was chairman, and which has now been read a third time in the House of Commons and passed. The Bill introduces many sweeping changes, and not only gives to the Local Government Board increased powers, but empowers them "to confer the powers of this Bill or any of them to urban authorities," and further ordains that the Board shall have power to permit Local Boards to purchase water compulsorily, under provisional orders, confirmed by Parliament, "such Provisional Orders to put in force the Water Clauses Act, 1847."

Should Mr. Brown's Bill pass the Upper House but little additional legislation would appear to be necessary, for the labours of the various Royal Commissions, Parliamentary Committees, and the British Association Committee of Inquiry "into the Secondary rocks of England, as a source of water supply," have amassed, as we have seen, a large volume of information as to the rainfall, available yield, and quality of the water suitable for domestic purposes; and the new powers obtained by the Local Government Board will enable them more quickly and cheaply to give facilities for the construction of works for water supply than heretofore. All that is still wanting, should the Bill pass, is some further machinery for ascertaining by Government inspection what rural districts and isolated hamlets are not at present properly supplied, and how the difficulty is to be solved.

The Congress have met it by passing a resolution to "urge upon Her Majesty's Government the importance of taking steps, with the least possible delay, by means of a small scientific commission to investigate and collect, for the information of the public, the facts connected with water supply, in the various districts throughout the United Kingdom, in order to facilitate the utilisation of the national sources of water supply for the benefit of the country as a whole, as suggested by H.R.H. the Prince of Wales."

The high value and important character of the work done by several bodies similarly constituted to the proposed Water Commission is so well known, that it would be needless to mention as examples the Charity, Civil Service, and Ecclesiastical Commissioners, were it not to point out that these gentlemen exercise functions of a special character, which could not well be undertaken by any other Government department; while in the case of the proposed Water Commission, this very important *raison d'être* appears to be absent, for special knowledge and long experience appear to be already possessed by the staff of the Local Government Board, and by that of the Geological Survey, which under the interchange system of Government officers recommended by Dr. Lyon Playfair's Commission could easily be directed to assist them, especially as to a limited extent it already does so, and has besides aided the Rivers Pollution Commission in its labours.[1]

The Congress has certainly been useful in showing how impossible it is to separate *water supply* from *drainage*, and the absolute necessity of there being a central authority, with supreme power over water, whether at the surface or underground, whether *used* for the purpose of water supply, canalisation, supplying motive power, or *disposed of* in the form of drainage and sewerage. As Dr. G. W. Child well remarked, "the bane of all local government in England is the chaos of different and often conflicting authorities, existing each for a special purpose." How the formation of the proposed permanent Water Commission will facilitate matters by adding another to the long list of governing bodies it is difficult to see.

Facts are useful; but we first want simplification and unification of the law, and the carrying out its provisions entrusted to one central authority, directed by a Minister of Public Health, with power so to modify and increase his department as to be able to collect information at the same time that he administers the law, and remove from us the possibility of the reproach that we have carried on scientific investigations to complete our knowledge of water supply, without applying, for the use of our population and the prevention of disease, the information we already possess.

[1] "Borough of Liverpool New Water Supply Report," by Mr. John Frederick Bateman, C.E., F.R.S. Liverpool, 1875.

[2] The compensation of 13½ million gallons, stated in several journals, and lately quoted by us, is incorrect, the amount being only 5½.

[1] Annual Reports of the Science and Art Department.

PHYSICAL SCIENCE FOR ARTISTS[2]

IV.

I AM afraid there is no use in shirking the notion that my last paper may have seemed wofully dull, frightfully technical, and terribly wide of the mark to some artists who took it up, always supposing of course that

[2] Continued from p. 89.

any of them did. Although I must plead not guilty to the last count, still I am so persuaded that in the nature of things these opinions were likely to be held that I feel compelled, before I go further into the experimentation with our improvised spectroscope, to draw attention to the kind of knowledge I hope to show can be gained by its use.

It might at first sight appear that, limiting ourselves to the sun as a great radiator of light, the artist has only to do with white light in his pictures. This is true in one sense, but only in one sense; for the artist has to deal with sunlight after it has been filtered through—after it has been absorbed by—many substances, notably the aqueous vapour of our air, and after reflection from others. I shall show in the sequel, for instance, precisely how it is that the sun is red at sunset; at such a time as this the sun practically gives us coloured light because our atmosphere has abstracted from it some of the constituent rays which fell on the upper air. A familiar instance of this may be referred to. The colour of a ripe cornfield, in an autumn sunset, comes from the fact that, for the moment, in consequence of this absorptive effect, our sun has been a red star instead of a white one. Cause and effect are there before our eyes, and science connects them, and yet, alas, I have seen pictures in which the grand colour of the corn has been given in perfection, while the painter has been so ignorant of the cause of it that he has given us a cold, grey, cloudy sunset, instead of a red, cloudless one.

Further, as all the ordinary colours of natural objects depend upon the way in which they reflect or absorb white light, the colours of all must change at the hour of sunset or sunrise, if the light which they usually transmit to us is wanting in the light which they then receive. An object, for instance, which appears blue at noonday will appear black if only the red light of sunset falls upon it. The blue sky over head is really a rich source of light of all colours; it is not a true blue, it is a mixture of blue and white; so that sunrise and sunset effects are much more potent when a great bank of cloud overhead leaves only the eastern or western horizon open. A striking thing for an artist to observe under this condition is the difference of luminosity of a red brick house, and such objects as trees and fields; the house seems glowing with light as if it were red hot, and for the simple reason that it gets as much light of the red kind, which is what it wants, and which it reflects to us, from the setting sun, as it does from the noon-day one; whereas the trees and grass are no longer supplied with that light which they throw back to us usually, and so appear black for the same reason that lampblack appears black at noonday.

It has been a favourite theme with many astronomers to enlarge upon the marvellous coloured phenomena which must take place in those planets which are near enough to stars of strongly contrasted colours to receive their light, now from one, and now from the other, producing not only a perfect modulation and combination of the colours of both, but also the strongest contrasts, such as, for instance, the setting of a red sun followed by the opposite rising of a blue one. No doubt such phenomena would be very enthralling, but to my mind the chromatic effects produced by the aqueous vapour in our own air absorbing the various elements in the light of our single white sun when it is not more than ten degrees above the horizon, supply us with a world of beauty with which we may well be content, and I for one have not found the beauty one whit less enthralling since I have endeavoured to picture to myself the causes to which it is due.

All this, however, by way of anticipation: the causes so far as I am acquainted with them at work in the circumstances I have named, and in ten thousand others, are easily to be got at by a little simple experimentation.

In my last paper I suggested a simple form of spectroscope. Here is a figure which will show how the prism should be placed and what it will do to the light coming through the slit C.

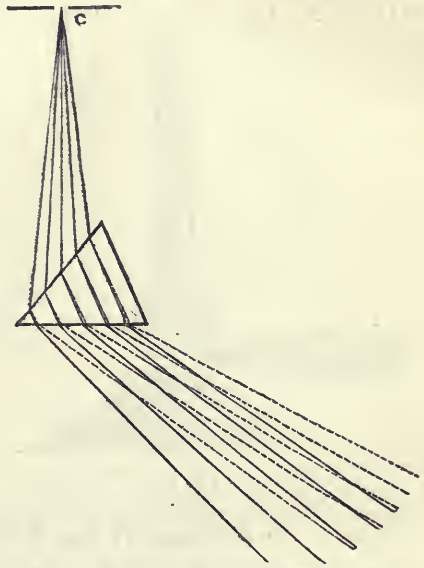


FIG. 1.—Showing arrangement of slit and prism.

We may place a lens behind the prism as shown in Fig. 2, and throw an image on a screen, which may conveniently be a piece of white paper.

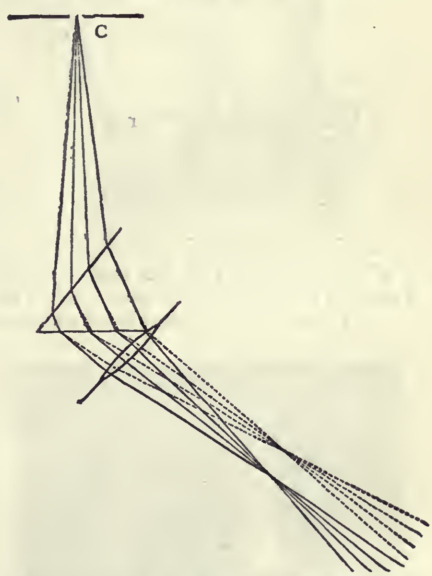


FIG. 2.—Introduction of a lens to produce an image.

If we are content to use the lens and screen which constitute the human eye it must be placed near the prism.

An expenditure of a few shillings is all that is required to enable the origin of one class of coloured phenomena to be investigated, that class, namely, in which the colour is due to the giving out, by the light source, of certain kinds of light only. This money should be expended in buying two little glass or brass tubes, $\frac{1}{10}$ inch and $\frac{1}{2}$ inch in diameter, and 5 inches long, a little glass tubing of very small bore, a few inches of platinum wire, a small quantity of red and green fire, and india-rubber tubing to convey gas from an ordinary burner to a table. Of the two tubes a Bunsen burner can be easily constructed

This is an apparatus for burning a mixture of ordinary gas and air; the gas should be supplied through the smaller tube inserted into the lower end of the larger one, as shown in Fig. 3. The gas should be lit by holding a

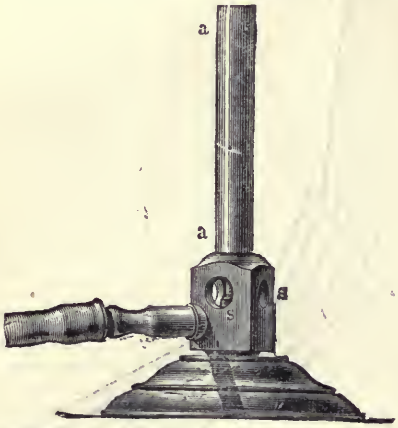


FIG. 3.—Bunsen burner, ordinary form. a, exterior tube; s, holes to admit the air.

match some 4 inches above the upper orifice of the wide tube. One end of the platinum wire may be fused into

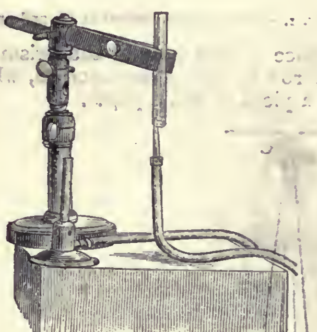


FIG. 4.—Improvised Bunsen burner

the piece of glass tubing, and the other twisted into a loop, fine enough to hold some common salt in the flame. A piece of coke or charcoal soaked in salt and water will

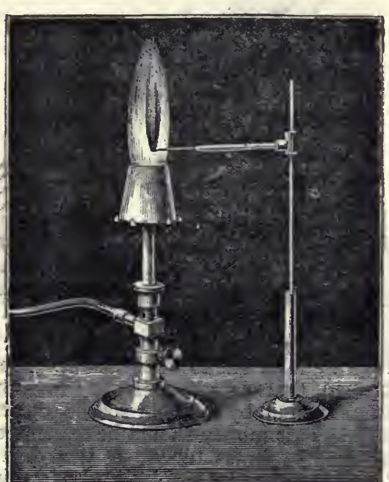


FIG. 5.—Method of inserting platinum wire and salt into the flame.

do almost as well. This tube may be supported by a piece of wood after the fashion of Fig. 5. The Bunsen burner will give us a very hot bluish flame, into which the loop of platinum containing common salt, *sodic chloride*, may be inserted, as shown in the accompanying woodcut. We shall get a brilliant yellow flame, which is worth notice on its own account, and if the artist is performing these experiments in his studio, let him look at some of his choicest pictures by means of this light, before he goes any further. He will be considerably surprised at their appearance, and I hope he will set himself to think about the cause of it—a point on which there will be a good deal to be said in the sequel. It will be better, however, to get at the physics in the first instance. To do this, put the improvised Bunsen burner and the platinum wire with the salt on it in front of the slit, and look at the slit through the prism; it will be found that there is only a yellow image of the slit visible. If the things have been nicely arranged, the appearance of the spectrum will be so entirely changed that a beginner will be apt to fancy that something has gone wrong. Nothing has gone wrong however; we have simply passed from the spectrum of polychromatic to that of monochromatic light—from white light to coloured light.

These experiments touching the giving out of light can be easily and cheaply varied by burning green and red fire in front of the slit; the effect of these differently-coloured lights on a picture is also very striking.

The next thing we have to do then is to represent the action of an absorbing body,—to study the action of our theoretical screen—the action of bodies when they absorb light, and *therefore transform the original colour which that light possessed.* Liquids will form our most convenient screen to illustrate this, and they can be placed in a "cell" like that shown in the accompanying woodcut.

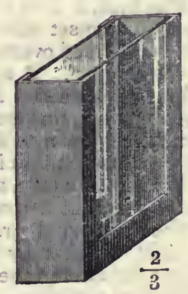


FIG. 6.—Common form of cell to hold solutions.

It will not be necessary to buy such an apparatus; two squares of glass, with a piece of india-rubber tubing between them, bent in the shape of a U; the glasses being kept in contact with the tubing by two india-rubber bands, form a cell which is wonderfully tight, and will serve our present purpose. This cell should be placed in front of the slit.

A little potassic permanganate added to the water in the cell, will act as a screen, and cut off the yellow part of the spectrum, and the adjacent regions of the orange and green. Solutions of blood or magenta will give also very definite indications of absorption, and if we have one of those handy little pocket spectroscopes, which now, I am glad to think, are becoming common, the absorption of the light of a candle by the blood in the lobe of a friend's ear, or in the interval between two closed fingers can be well seen by placing it between the slit and the light.

Let us now sum up as tersely as may be the conclusions at which we have so far arrived.

I. White light analysed by a prism gives us a continuous spectrum.

II. Coloured light analysed by a prism gives us a discontinuous spectrum.

III. Light may be coloured because originally the series of its components was not complete.

IV. Light may be coloured because although the series of its components was once complete, some parts of the light have been absorbed in its passage to the eye.

V. The bodies which give us white light are generally complex as to their molecules.

VI. The bodies which give rise to the phenomena of bright or dark lines are generally simple as to their molecules.

I now begin, with fear and trembling, to touch upon a part of my subject in which I ought to be the first to acknowledge that our ideas are not of the sure and certain kind. In what has gone before I have been careful to point out that, though the effect of incandescent bodies in producing and absorbing light was not likely to be directly applied by the artist, it was still in this sure and certain region that he should endeavour to follow the workings of laws clearly made out which might be in force elsewhere.

This brings me to state that in my opinion the colours of most natural bodies depend upon the fact that there are definite molecular states of all kinds of matter lying between those two extreme stages to the phenomena presented by which attention has been directed. Some years ago, in a communication to the Royal Society, I drew attention to evidence which seemed to indicate that many substances which emit under certain conditions a white light giving a continuous spectrum, and coloured light associated with the spectrum of lines under others, exist also in molecular groupings between these extreme conditions, giving us for one grouping a continuous spectrum at the red end, and for the other a continuous spectrum in the blue.

This is the most general statement that I can make, and I make it on account of the utility of such a statement. It has not yet been proved to be universally true, but the evidence I have already accumulated justifies me in setting it up as a working hypothesis. Not least valid among the lines of evidence on which I rely is the curious fact that the colours of almost all natural bodies can be at once explained by assuming them to be built up of these two molecular groupings to which I have called attention.

Let us take gold as an instance. It is yellow; but why is it yellow? Because the molecules of gold, as I believe, generally exist in two complexities, one of them competent to harmonise with the red rays of light, and therefore to absorb them, the other doing the same thing with the blue light, and for the same reason.

Gum a piece of gold leaf on a piece of glass for easy manipulation, and look at a bright light through it; it will be seen that the gold is green, or, in other words, that the blue and red have been absorbed, we have

V I B G Y O R

changed into

V I B G **Y O R**

by one set of molecules stopping

V I B

and another

Y O R

The reason that we get yellow light by reflection is that more of the central light is reflected than is transmitted.

If we do not consider reflection, the thing becomes simpler: for instance, if I take a tube one foot long and fill it with chlorine gas, and observe the spectrum of a white light through the tube, we find that chlorine absorbs only in the blue; the yellow and red are freely transmitted; a glass coloured red is so coloured because its molecules absorb the blue, and a blue glass is blue because it absorbs the red.

Prof. Stokes, in one of his lectures in South Kensington, dealt with the colours of natural bodies in connection with the absorption of light by them, and I may be permitted to close the present paper with the following extract from so great an authority:—

"What is the cause why a green leaf is green, or why a red poppy is red? It is frequently said that the reason why a red poppy is red and that a white lily is white is, that the lily reflects rays of all kinds, but the poppy reflects only the red ones, and if you place the red poppy in a pure spectrum it is luminous, like a white lily, in the red; but if you place it in the green it will be almost black, whereas the white lily will be brilliantly green. Now the common explanation, properly understood, is true; but it is not the whole truth, and if understood as it is liable to be understood, it is false. It is true that a red poppy reflects red rays, and a white lily reflects rays of all colours; but it is not true that the preference for the red to the green in the one case and the equality of action in the other takes place *in the act of reflection*. It is not a phenomenon of coloration by reflection. The coloured light is reflected or you would not see it; it is sent out of its course before it enters your eye, and it is true that the light, in its life's history, undergoes reflection; it is not true that it is in the act of reflection that the one colour gets the preference over the other. Here I have some solution of the colouring matter of green leaves in alcohol, and here is some more alcohol, with which I will dilute the former. I have obtained a beautiful green solution, although the green colour is not seen now by reflected, but by transmitted light. As regards the light which falls upon the surface there is a little white light reflected, just as there would be from water, but very little is reflected from the surface where the fluid is in contact with the glass, the chief portion of that reflected being from the outer surface of the glass itself. You would not see any green at all in it unless there were something placed behind so as to reflect the light backwards. You see there that the colour of the green leaf, as ordinarily seen, is due to the combination of reflection with the phenomena of absorption, or the swallowing up of certain kinds of light when light is sent through a perfectly clear medium. I may illustrate this in another manner. Here is a vessel of water into which I will pour some blue solution. If I send light through it, it will appear of a deep blue, but if I hinder the light from coming behind, which I can do by putting black cloth behind it, it is simply dark; you do not see the blue colour at all. Why? Because there is nothing behind to reflect the light. Suppose I make it a little muddy by pouring into it some pounded chalk, you see the blue colour immediately. Why is that? You know that if powdered chalk were put into water it would not colour the fluid. But here each little particle of uncoloured chalk reflects a small quantity of light falling upon it, so that it fulfils the same office as a mirror placed behind the fluid. You may imagine that the particles of chalk are so many minute mirrors capable of reflecting light. If you take any one particle of chalk, say one-tenth of an inch deep, in the liquid, the light from the sky falls upon the fluid, it undergoes absorption in passing through that first tenth of an inch, and then the portion of light which is left is reflected by that little particle of chalk, and passes out again, and so, as regards that single particle, the light which reaches your eye from beneath that depth has itself gone through a stratum of fluid of one-fifth of an inch in thickness, and accordingly you see the colours produced by selective absorption; that is to say, by the absorption of certain kinds of light, which are more greedily devoured by the fluid than the other kinds. This is what takes place in the green leaf and in

the petals of flowers. Let us take the white lily. If the petal of the flower had been merely a sheet of thin glass you would not have seen that white colour. There would have been a little light reflected from the first surface and the back surface, but the petal is really composed of a vast assemblage of little cells, at each of which partial reflection takes place, so that it resembles some finely-powdered glass, which would form a white powder, because each little surface is capable of reflecting the light, although a single sheet of glass would not be white. The petal of the white lily is just in the condition of the powder. It is full of little cells, full, optically speaking, of irregularities, from each of which a portion of light is reflected, so that, all kinds being reflected alike, and there being nothing in the white lily to cause preferential selection of one over the other—nothing to sift the light, as it were—you get a considerable quantity of light reflected back to the eye, but it is white. What is the difference between that and the red poppy? The red poppy is, as it were, a white lily infused with a red fluid; there is light continually reflected backwards and forwards, just as before, at the surface of the cells; but that light, in going and coming, passes through the coloured juice of the plant. It is the same thing with a green leaf. The structure is irregular, optically considered; there are constantly reflections, backwards and forwards, of light, which penetrates a little depth and is reflected, and has to pass through a certain stratum of this colouring matter, to which the name chlorophyll has been given, but which is really a mixture. That is what takes place generally as regards the coloration of bodies; it is a phenomenon not of reflection, not of selection of one kind of light for more copious reflection than another, but of absorption, or the swallowing up of certain kinds of light. Reflection comes in in order to enable us to see the light which otherwise would not enter the eye at all, but would go off in another direction."

J. NORMAN LOCKYER

COSMIC METEOROLOGY

SEVERAL articles have appeared at different times under the title of "La Météorologie Cosmique," from the pen of M. Faye: the last and most complete forms an exceedingly interesting "Notice Scientifique," appended to the *Annuaire* of the Bureau des Longitudes for 1878. In this memoir, written with the usual clearness and talent of the distinguished French astronomer, a number of results connected with real or supposed solar, lunar, and planetary actions on our earth are examined and criticised. As M. Faye has omitted several facts of considerable importance in his "Notice," and has misunderstood others, a reconsideration of some of the questions he has studied, with the additional light to be obtained from the facts alluded to, may not be without interest and use.

M. Faye's thesis is given in the first words of his article "Meteorological phenomena have their origin in solar heat." It is added, "This is now no longer sufficient. Cosmic influences are introduced, those of the planets, of the spots and rotation of the sun, of shooting-stars, the moon, and besides these, magnetic and electric actions are supposed to intervene incessantly between the bodies of the solar system." I shall refer to some of the most important questions under their different heads.

The Moon's Influence in producing Atmospheric Variations.—The popular beliefs in the moon's influence on the weather are first disposed of; they are conclusions from unrecorded observations where the coincidences are remembered and the oppositions are forgotten; and they are opposed to strict deductions when all the facts are employed.

Agreeing, as all men of science do, with this decision, the question remains, Whether the moon may not have some slight effect in producing meteorological variations? She reflects, absorbs, and radiates the solar heat; may this heat, in accordance with the thesis, not produce some effect on our atmosphere?

Sir John Herschel had observed the tendency to disappearance of clouds under the full moon; this he considered a fact which might be explained by the absorption of the radiated lunar heat in the upper strata of our atmosphere. He cited Humboldt's statement as to the fact being well known to pilots and seamen of Spanish America. I may add the testimony of Barnardin de St. Pierre, who, in his "Voyage à l'Ile de Réunion," says: "I remarked constantly that the rising of the moon dissipated the clouds in a marked way. Two hours after rising, the sky is perfectly clear" ("Avril, 1768"). Herschel also cited in favour of his "meteorological fact," a result supported by the authority of Arago, that rather more rain falls near new than near full moon.

Arago's conclusion that the phenomenon was "incontestable of a connection existing between the number of rainy days and the phases of the moon" was founded on the observations of Schübler, of Bouvard and of Eisenlohr, three series which, on the whole, confirmed each other. Schübler also, as Arago showed, had found that the *quantity* of rain which fell was greater near new than near full moon. These results, accepted by Arago, have not been noticed by M. Faye when he cites Herschel only, as one of those "men of science who interest themselves in popular prejudices, take bravely their defence in hand and exert themselves to furnish not facts but arguments in their favour." It seems, indeed, to have been forgotten that Herschel's argument was given to explain what he considered a meteorological fact.

M. Faye founds his argument wholly on the conclusions of M. Schiaparelli from a weather register kept at Vigevano by Dr. Serafini during thirty-eight years (1827-1864).[1] The Italian physician entered the weather as clear, cloudy or mixed (*misti*), or rainy from morning to evening. M. Schiaparelli finds from this register that the sky was clearest in the first quarter of the moon. It has not been remarked that if the moon's heat has any effect in dissipating clouds, as Herschel and others believed, this must be seen best when the moon is near full, that is to say, during the night hours, for which Dr. Serafini's register has nothing to say. In confirmation of the conclusion that the moon does not dissipate the clouds, another result from the Vigevano weather register is cited, namely, that the greatest number of rainy days happens near full moon. This result is opposed to that derived from the observations of Schübler, Bouvard, and Eisenlohr.

The value to be given to observations of the number of rainy days must evidently depend on whether the observations include the rainy nights; and an investigation on this question, to have any considerable weight, should depend rather on the measured rainfall than on the term "rainy day," for which no distinct definition is given.

The great objection to M. Faye's conclusions, as far as the facts go, is to be found in their entire dependence on the Vigevano weather register (*da mane a sera*). No notice is taken of other observations and results showing a lunar action on our atmosphere, such as those already mentioned, which Arago considered incontestable, those of Mädler and Kreil, and the more recent investigations of Mr. Park Harrison and Prof. Balfour Stewart. All of these, and many others, must be carefully considered before we can accept the conclusion that the moon has no influence on our atmosphere. The subject is, however, too large to be entered into here at present, and it will be possible to study it better after other conclusions of the learned French Academician have been examined.

There is, however, a part of the argument, whatever the results obtained may say, which merits particular

[1] *Memorie del R. Istituto Lombardo*, L. 10.

consideration; and that is, that the moon's *heat* cannot produce the phenomena in question. M. Faye shows that if the moon's reflected heat is in the same proportion as the reflected light, such heat cannot produce a change of temperature of a thousandth of a degree Fahrenheit. I would remark that Lord Rosse's carefully-made experiments with the most delicate apparatus have shown for the total heat radiated and reflected, nearly ten times the proportion given by the reflected light; but, as M. Faye observes, if the proportion were increased a hundredfold the effect would still be insensible. "How then," it is added, "can we expect such an action to dissipate the clouds when that of the sun does not always succeed?"

If, however, we can establish that real lunar actions exist which cannot be explained by the moon's heat reflected or radiated, the only philosophical conclusion will be that the moon must act in some other way, which it will be in the interests of science to seek out.

Influence of the Sun-spots on the Earth's Magnetism.—It has been found that since the first accurate series of magnetic observations, towards the end of last century, to the present time, the maximum and minimum of sun-spot frequency have occurred at the same times as those of the diurnal oscillation of the magnetic needle; but because Dr. Wolf has believed that in the interval between 1787 and 1818, when the observations of both phenomena were very incomplete, there were only two cycles for both, and because Dr. Lamont and myself believe there were three for both, M. Faye concludes that the mean length of the period for the magnetic needle was different from that for the sun-spots. The true conclusion is—if Dr. Wolf is right the mean length of the period for *both* phenomena since 1787 is nearly twelve years; if the other view is right the mean length for *both* is 10·45 years. I have already considered this question in NATURE (vol. xvii. p. 262). The *egalité rigoureuse* of the lengths of the periods sought by M. Faye is thus established whatever view be taken.

It is next sought to show that any variation in the earth's magnetism in the decennial period cannot be due to variations in the solar heat produced by the spots. Founding on Mr. Langley's observations for the temperature of the photosphere, of the nucleus and the penumbra of spots, M. Faye shows, making use as before of the absolute temperature of the earth, that the variation of temperature due to the spotted surface of the sun cannot exceed $\pm$ 0°·3 Fahr.

I venture here to remark, in the first place, that I have given strong grounds elsewhere for believing that the sun-spots are not the cause of the decennial period in the magnetic variations; but that both are due to the action of a common cause.[1] I quite accept then M. Faye's conclusion. It may, however, be argued that more heat proceeds from the sun in years of many, than in those of few, spots, but the observations of temperature, which have been made with so much accuracy in many countries for the last twenty years, will prove at once that no marked variation of the mean temperature occurs in the decennial period.

We must here, however, consider the reasoning which has been employed on this subject. It is shown that the variation of temperature due to the spotted surface of the sun cannot explain the change in the amplitude of the diurnal oscillations of the needle. It is, however, a mere assumption that this oscillation is due to the solar heat, an assumption for which there is really no sound basis, unless some very rude attempts at a hypothesis can be so considered.

M. Faye says, "This phenomenon is absolutely general; it appears over the whole earth following the same laws, only the amplitude of the oscillation, which in the mean is at Paris 10′, is reduced to 1′ or 2′ between the tropics, and goes on increasing thence towards the poles." This variation of range is afterwards compared with the inverse law for the diurnal variation of the barometer, the range of which *diminishes* from the tropics towards the poles.

This view, however, is founded on a misconception of the facts; we might just as well say that the earth's magnetic force diminishes from the equator towards the poles (which is just the reverse of the truth), because its horizontal component does so. The mode in which this movement varies in amount from the tropics towards the poles appears to be imperfectly known, and as it is essential to make this clear before we can compare the facts with any hypothesis, I shall now attempt to do so.

Diurnal Variation of the Magnetic Declination.—The horizontally suspended magnetic needle performs an oscillation in twenty-four hours, during which the north end is most westerly in the northern hemisphere, and most easterly in the southern hemisphere, between 1 and 2 P.M. If we consider needles at different stations on the same meridian, it might be supposed that as we approached the equator this opposition of movement would result in the needle becoming stationary. That was Arago's conclusion. This idea, however, neglected the position of the sun. The range of the oscillation is greatest in our hemisphere when the sun is north, and greatest in the southern hemisphere when the sun is south of the equator. It is only when the sun has the intermediate position, near the time of the equinoxes, that the forces are nearly balanced at equatorial stations.

If we suspend an unmagnetic steel needle horizontally on its centre of gravity, so that it can move both in a horizontal and vertical plane, and then magnetise it, the needle *dips* with one end below, the other above, the horizontal plane passing through its centre; and the direction in which it lies is that of the earth's magnetic force. If this needle moves up, or down, or sideways, this is because other forces pull it one way or another, or because the direction of the earth's magnetic force has changed. Let us suppose in the first instance that the latter hypothesis is the true one, that is to say, that the earth's magnetic axis moves with the sun, the north pole having the greatest movement when the sun is in the northern hemisphere. If the dipping needle moves in the plane perpendicular to the vertical plane, through an angle u, and we wish to know what would have been the corresponding movements eastwards or westwards, if the needle had been made horizontal, by the addition of a small weight to the end above the horizontal plane, we must *divide* the angle u by the cosine of the *dip* below the horizon. Now in England the cosine of the dip is about $\frac{1}{3}$, so that the horizontal needle would have moved through an angle of $3u$. We obtain similarly the former from the latter by *multiplying* by the cosine of the dip.

We may now see, from observations made at different stations, what is the range of the monthly mean diurnal variation of the horizontal needle multiplied by cosine *dip* in a month for which it is a maximum in the northern hemisphere. The following are approximations to the mean ranges thus obtained from several years observations in the month of August for the northern hemisphere.

Station.	Lat.	Dip.	Range of hor. needle × cos. dip.
Makerstoun	56° N.	71° N.	4′·0
Toronto	44 ,,	75 ,,	3·5
Simla	31 ,,	42 ,,	4·6
Bombay	19 ,,	19 ,,	5·4
Madras	13 ,,	8 ,,	4·8
Trevandrum	9 ,,	3 S.	3·6

In a similar way we find for the month of February for southern stations—

[1] "On the Decennial Period" (*Trans.* Roy. Soc. Edinb., vol. xxvii. p. 593).

St. Helena	16° S. ...	22° S. ...	4′·8
Cape of Good Hope	34 ,, ...	53 ,, ...	4·6
Hobarton	43 ,, ...	70 ,, ...	4·3

It will be seen that in the months in which the sun's action is a maximum,[1] for each hemisphere the east and west movements of the needle in its true position (that which is independent of gravity) do not vary with latitude; the maximum range appears, in fact, to take place near the tropics, and when the sun is in the zenith.

If, however, we should prefer to consider the oscillations of the horizontal needle due to the direct action of electrical currents upon it rather than upon the earth, we must remember the change of the force which directs the needle as we proceed from one latitude to another. If we wish to compare the vertical heights through which a body will fall in a second of time on an inclined plane at two stations, we must take into account not only the force of gravity at each station, but also the angle which the inclined plane makes with the horizontal plane. When we employ the same unit of directive force at the stations in the north hemisphere for the month of August, we obtain the following comparative ranges :—

Makerstoun ...	4·3
Toronto ...	4·8
Simla	4·4
Bombay ...	4·6
Madras	3·8 (September = 4·2).

The values are less for Trevandrum and St. Helena, but there is no appearance of a law which can be referred to latitude; and there is no way in which we can examine the question which will satisfy such a relation. If we take any zone or zones of the earth which will include as much of the northern as the southern hemisphere, the mean movement for them will be nearly zero, on account of the opposite directions of the oscillations; it is for this reason that there is a diminution of range, especially in the months near the equinoxes, for equatorial stations.

On the whole the conclusion is, that the diurnal law of oscillation east and west of a magnetic needle is nearly the same in all latitudes for a given position of the sun and a given directive force. The deviations from this rule are connected with magnetic disturbances which have most effect near the poles, and with the opposition of forces near the equator. We have thus to deal with a phenomenon which is little dependent upon local causes, and which may, in its great features, be considered cosmic.[2]

These facts understood, we are now in a better position to consider the great change in the range of this oscillation, which occurs in the decennial and sun-spot period. Let us examine what that change really is. It does not matter here whether we refer to the motion of the horizontal or of the dipping needle; we find that if the mean range is counted 10 in England in the years when it is a minimum, the years of fewest spots, then it becomes 16 or thereby when it is a maximum, that is, in the years for which the sunspots are most numerous.

Now this great change in the effect of the solar action is felt in nearly the same way, and to the same proportionate amount, all over the globe. The law of the oscillation is not changed; the needle attains the most westerly position in one hemisphere, and the most easterly in the other, at the same hour as before, but the oscillation is nearly sixty per cent. greater at Hobarton in Van Diemen Island, at Trevandrum on the magnetic equator, at Toronto in Canada, and in England.

When the observations have been continued sufficiently long with equal care, we can find that the ratio of the maximum range to the minimum is undergoing, at some stations at least, a slow change. Thus, at Trevandrum the successive ratios of the maximum of 1860 and 1870 to the preceding and following minima are—

$$\frac{1860}{1856},\ 1·59;\ \frac{1860}{1866},\ 1·57;\ \frac{1870}{1866},\ 1·55;\ \frac{1870}{1877},\ 1·51.$$

It must be remembered that the thesis which M. Faye supports is, that the diurnal oscillations of the magnetic needle are due to the solar heat, and that he has shown that no appreciable change of temperature is due to the spotted surface of the sun. We may ask then, Where are we to find the change of temperature which causes so great a variation in the sun's action? We need not calculate the difference of temperature between the photosphere and the nucleus of a spot, and we need not theorise on the possible difference between the solar radiation when there are few and many spots; we have got thermometers; we have even observations of the evaporation of water, from which solar action M. Faye finds the atmospheric electricity which should produce the magnetic variations. What do they say? If effects have any relation whatever to their causes, surely when the effect of the solar action in producing the diurnal variations of the earth's magnetism is increased by a half or three-quarters of its value from the time of most spots till the sun shines with unspotted surface, we should expect some marked changes of temperature from year to year, if change of temperature is in question. I have already remarked that no such change of temperature has been found.

M. Faye says with reference to the question, What is the cause of the decennial magnetic variation? "The question would perhaps be embarrassing if we had only that to ask, but the elements of terrestrial magnetism present other variations which are as much independent of the sun-spots. Such are the secular changes which displace gradually the magnetic poles of our terrestrial globe. We must seek the cause not in the heavens but in the slow modifications of which the earth's surface is the theatre. They are due probably to the works of men and above all to the continued action of geological forces." Of course these ideas refer to the secular changes, even for which they will not be readily accepted, but they do not touch on the fact that an explanation is offered, that of the solar heat, for the diurnal variations, and that no evidence is produced that the supposed cause undergoes any change in the decennial period.

I have referred only to oscillations of the magnetic needle, which may be considered due to the variations of an easterly force; but the force of the earth's magnetism which directs the needle north and south, obeys also a law of diurnal variation, and the range of this variation follows the same law in the decennial period as that for the ranges of the oscillations. Thus at Trevandrum, near the magnetic equator, the *range* of the diurnal variation of the total force of the earth's magnetism in the year for which it was a maximum, was to the range in the year of minimum as 17·7 to 10. The corresponding ratio of the ranges for the oscillations of the needle having been as 15·9 to 10.[1]

As there is not the slightest evidence of a decennial variation in the solar heat, and as there is an absolute certainty that if any variation exists it is of an amount so very small that it could not account for the great changes in the magnetic variations, the conclusion appears to me inevitable, that these variations are *not* due to the solar heat. We fortunately possess the means of deciding this question by the study of phenomena due to our satellite, whose heating action M. Faye has shown to be quite inappreciable.

JOHN ALLAN BROUN

(*To be continued.*)

[1] The month of maximum varies within the tropics: at Madras it is in September, and for that month the range multiplied by cos. dip = 5′·3 nearly.

[2] For these reasons no such inverse relation with latitude exists as M. Faye has supposed between the diurnal oscillation of the magnetic needle and of the barometer.

[1] I would remark here that the epoch of maximum range of force was not exactly the same as that for the maximum oscillation, a fact which may not be without importance when the mode of solar action is investigated.

THE MICROPHONE

WE have received the following communications on this subject:—

AT a discussion upon Mr. William Preece's paper on the microphone, which took place before the Society of Telegraph Engineers on Thursday last, the Duke of Argyll called attention to the important part which that invention was likely to play in physiological research. As chairman of the meeting I took occasion to refer to the intimate connection between the microphone and its two elder sisters, the telephone and the phonograph, in conjunction with which it formed a discovery which would probably be hereafter regarded as one of the greatest achievements in natural science of the present century. I ventured further to draw an anology between the action of the phonograph and the action of the brain in the exercise of memory, and with your permission I will enlarge upon this speculation to the extent of making my reasoning clear enough to submit the same to the critical test.

All impressions received by us from without, either through the tympanum of the ear, the retina of the eye, or through the sensitive nerves of the skin, are, it is generally believed by physiologists, communicated to corpuscular bodies in the brain, which lie embedded in a grey substance, the nature and precise function of which have not yet been fully explained. It would appear that the corpuscular bodies in which the sensitive nerves terminate are connected, through the medium of extremely delicate filaments, with the nervous system of volition, the reaction of the one system upon the other being attributable to mental energy. It may be conceived that any fresh impressions received on the extremely complex sensitive network of the brain may give rise then and there to acts of volition; but how, it may be asked, can acts of volition arise from impressions that were communicated through the sensitive nerves years before, having been committed in the meantime to what we term the memory? But in order that the mind can deal with an impression previously received it seems necessary that it must have the power of reproducing the same from some material record by which the impression has been rendered permanent. Take the case of a tune that we have heard in early youth and which may not have since recurred to us. By some incident or other that tune and the words connected with it become suddenly revivified in the mind. If the tune had been sung into a phonograph it could have been reproduced at any time by releasing a spring moving the barrel of the instrument; and it seems a fair question to ask whether the grey substance of the brain may not, after all, be something analogous to a storehouse of phonographic impressions representing the accumulated treasure of our knowledge and experience, to be called into requisition by the directing power of the mind in turning on, as it were, one barrel or another.

Such a hypothesis might possibly serve also to explain how in sleep, when the directing power of the mind is not active, a local disturbance in the nervous system may turn on one or more phonographic barrels at a time, and thus produce the confused images of dreamland! A powerful mind would exercise a complete control over the innumerable barrels constituting our store of knowledge, whereas in a weak mind the impressions of the past would be brought back into evidence in a confused and irregular manner. Such a supposition might also account for the more vivid recollection of impressions received in early life, when the mechanical record stored up in the brain may be supposed to have been more distinctly and indelibly rendered. In speaking of these impressions as phonographic it does not follow that they were originally conveyed through the tympanum of the ear. Mr. Willoughby Smith, at the meeting above referred to, called attention to the fact that, by substituting crystalline selenium for carbon in the microphone, a ray of sunlight directed upon the selenium produces a noise comparable with that produced by a Nasmyth hammer; and it is quite feasible that the impressions received through the retina of the eye, and the nervous system generally, would be equally susceptible of being recorded in the cerebral storehouse. The record itself might be supposed to be of a mechanical, or, more probably, of a molecular character, the one thing important being that it must be material.

These observations are, no doubt, extremely crude, but may serve possibly to direct the attention of physiologists to a point of interest to their science; nor would it be the first occasion on which a phenomenon of inanimate nature had revealed the secrets of animate organisation.

C. WILLIAM SIEMENS

I HAVE been much interested in your account of the microphone of Prof. Hughes, and I have made, as doubtless many of your readers have also done, the different forms of instrument described by him. The action of the instrument is there apparently attributed to the change of conductivity of the charcoal or carbon or of the mercury globules therein, under the influence of sonorous waves; and whether this is correct is a question worthy of consideration in your columns, and I therefore write more for the purpose of leading others into the inquiry than of making assertions on the subject. My experiments point to another cause, viz., the variation of conducting sectional area of a bad conductor due to the increased or diminished pressure on the point of contact. I am not, of course, referring to the action of the instrument when the vibration is sufficient to absolutely sever the contact, which simply causes the telephone plate to vibrate either in its own period, or some other than that due to the acting sound, as is the case when a musical box is placed on the same table with the instrument; but to the forced or articulate vibrations—the reproduction of the sound acting on the microphone.

Of the several forms of instrument described by Prof. Hughes I have chiefly used that consisting of a rod of charcoal pointed at both ends, supported in a vertical position with its lower point resting in a hollow in a similar piece of charcoal, while its upper end rests against the sides of a similar hollow above. This form is extremely sensitive, and it is difficult to prevent the circuit being broken while having it sufficiently near the source of sound to be reproduced; the sound of a musical box is perfectly rendered, when so far away that there is an absence of jarring from breaks in the circuit; but in talking to the instrument, any rise in the voice breaks contact and produces the jarring sound in the telephone, to the exclusion of all articulation.

I find that any sort of charcoal or carbon will answer, whether soaked with mercury or not; I therefore conclude that the mercury has little or nothing to do with the action. I have tried the effect of sound on rods of carbon and charcoal both saturated with mercury and not so saturated, so arranged that the vibrations could not alter the area of contact, and have obtained no sound whatever from the telephone in the circuit; I therefore conclude that the action takes place at the point or points of contact, and is due to the change of conducting area. To Prof. Hughes is due the credit of inventing a means of varying the electric current with extreme rapidity and slight motion without absolutely breaking the circuit, but I doubt whether a *microphone* is a proper term for describing the instrument. In gently brushing the stand of the instrument, sound is heard in the telephone, but it does not at all follow that what we hear is a magnified reproduction of the brushing sound; for if the rapidity of the vibrations or motion produced by

brushing is insufficient to produce sound, still they may move the charcoal sufficiently to produce alternations of current, each of which may be able to set up vibrations in the telephone plate in its own period, or a modification of it, giving what I call the jarring sound. If, therefore, we have this sound, we know that either the microphone is exposed to sounds so loud as to produce complete break of contact, or that there is a motion going on affecting it, of insufficient rapidity to be audible.

With the object of reproducing the voice or musical notes, I have made the following modification of the instrument:—A ferrotype plate 3 inches in diameter is fastened over a hole 2½ inches in diameter in a thick piece of wood; a flat piece of gas carbon weighing a few grains and having a fine copper wire attached to it is fastened to the top of the plate in the centre; over the piece of carbon is suspended by a wire spring another piece of carbon finely pointed, weighing about ¼ oz, and adjusted so as just to touch the carbon plate. The current is then led by the wires through the carbon point, and by careful adjustment of the latter almost any degree of sensitiveness can be attained. Whenever the sound becomes too loud the current is broken, and minute sparks are seen at the carbon point, and the jarring sound is heard at the same time in the telephone. The sound of a musical box is perfectly reproduced when the box is held in the air; the instrument is therefore sensible to sound-waves in air as in solids. GEO. M. SEABROKE

Rugby

I SEND an account of an experiment with the microphone which may interest some of your readers.

A microphone, made of three pieces of gas carbon (as described by Prof. Hughes) and the primary wire of a Du Bois Reymond's induction-coil, are placed in the circuit of a single Daniell cell. The wires from the secondary coil (pushed home) are attached to the poles of a Lippmann's capillary electrometer. The Daniell and microphone are twenty-five feet distant from the electrometer. If an observer watches the capillary-tube and speaks or sings to the microphone (*which is twenty-five feet distant*) definite and large movements of the mercury-column will be seen. The movements for various letters resemble those which have been previously observed to take place with the telephone, the "w" giving its curious double movement.

F. J. M. PAGE

Physiological Laboratory, University College, London

EARTHQUAKE IN VENEZUELA

IN the evening of the 12th of this month a severe earthquake destroyed the town of Cua, in this country. Cua is situated on the left bank of the River Tuy, in 10° 8′ 15″ L. N. and 66° 55′ W., Greenw. The height over the level of the Caribbean Sea I found in 1873, by barometrical measurement, 232 metres. It was the centre of a very flourishing agricultural district (annual produce, about 80,000*l.* a year), and had about 3,000 inhabitants.

The weather had been for weeks exceedingly hot, as generally this year in Venezuela. At 5 o'clock in the afternoon, before the earthquake, a temperature of 100° is said to have been noticed, and six days later, at the same hour, I observed myself 95°. The sky was clear, and the moon in perfect brightness. The shock occurred some minutes before a quarter to nine o'clock, and so violent was it that in less than two seconds all the centre of the town was a heap of ruins. It is impossible to fix the exact time of the shock, but it was felt in Carácas at 8h. 41m. 34s., the distance in a straight line between both places being about twenty-six English miles.

The centre of the town was situated on a small hill, about 20 metres over the lower part. The hill is composed of gneiss, micaceous and chloritic schists, rising rather steep towards W.S.W. This hill is surrounded by strata of clay and marl, covered by a deep stratum of alluvial soil, and resting on dark limestone and argillaceous schists, containing numerous crystals of iron pyrites.

Only the upper town was laid waste; the lower part suffered comparatively very little. From actual observations I found that the angle of emergence of the shock was about 60°. The centre cannot have been very deep, as the destruction was limited to a spot measuring only one square mile, although the shock of the transverse wave was felt in places 100 miles distant. The soil had burst at different places, giving issue to water highly impregnated with sulphuretted hydrogen. The shocks continued for several days, and are not yet entirely gone, but no further damage has been caused. About 300 people were killed; the loss of property is said to be about 300,000*l.* sterling.

I have reason to think that this earthquake had nothing to do with volcanic forces, but was due to an interior subsidence or downfall of calcareous rock, as I intend to prove in a special memoir on this subject, as soon as I shall have visited the locality once more.

Carácas, April 30 A. ERNST

OUR ASTRONOMICAL COLUMN

TEMPEL'S COMET, 1873, II.—We continue the ephemeris of this comet, for the latter half of June, as given by M. Schulhof in the Paris *Bulletin International* of May 7. If the calculated epoch of perihelion passage be approximately correct, the intensity of light will be increasing, and the comet would arrive at its least distance from the earth early in July. But the possible error in the mean motion determined from the observations of 1873, may render a search over a wide extent of sky unavoidable, if the comet is to be recovered at the present return. Shortly before the completion of his calculations M. Schulhof informed the writer that the probable error in the mean daily motion would not exceed ± 7″, but this degree of uncertainty involves a difference of nearly ± 20 days in the date of perihelion passage, so that the comet may be found after close search in a position considerably distant from the computed one. As in other similar cases, if the observer has the command of an equatorially-mounted instrument of good aperture, the most promising plan of search will be to commence at the calculated declination for the day, extending the sweep to 30m. or 40m. on each side of the calculated R.A., and to continue the same proceeding for 3° or 4° on each side of the calculated declination. It may be remarked that the computed R.A. for a certain change in perihelion passage, varies more rapidly than the computed declination. Perhaps there is a greater probability of the comet being detected at the latter end of June than subsequently, if the weather is generally favourable for a careful search.

The following positions for Paris midnight are deduced on the assumption that the comet will arrive at perihelion Sept. 1·5, the most probable date:—

	Right Ascension. h. m. s.	N. Declination. ° ′	Distance from earth.	Intensity of light.
June 15	15 34 44	5 6	0·667	0·90
„ 19	15 32 15	4 17	0·659	0·95
„ 23	15 30 20	3 20	0·654	0·99
„ 27	15 29 5	2 15	0·651	1·03
July 1	15 28 31	1 1	0·649	1·06

THE RECENT TRANSIT OF MERCURY.—In the instructions for observing this phenomenon suggested by Prof. Newcomb, and circulated by the United States Naval Observatory, it is remarked that "its accurate observation is of especial importance as affording data

which will be decisive of the question whether the result of Leverrier, that the motion of the perihelion of Mercury is much greater than that due to the action of the known planets, is really correct." So far as the results of observation have been made known, there is every indication that the theory of Leverrier will receive a striking confirmation therefrom. The observations of first internal contact in Europe are closely accordant with calculation; and in a telegram from M. André, in charge of the French expedition, despatched through the liberality of M. Bischoffsheim to Ogden, in the Utah territory, for the observation of the transit, it is stated—"sortie confirme théorie."

Adopting Leverrier's diameters of sun and planet, deduced from his elaborate discussion of the transits of Mercury observed to 1832, and the value of solar parallax determined by Professor Newcomb (8″·848), we have the following equations for the reduction of observed times of first external and internal contacts to the centre of the earth :—

First ext. cont. ... $3^{h}\ 13^{m}\ 1^{s}{\cdot}0 + 74^{s}{\cdot}53,\ \rho \sin l + 80^{s}{\cdot}89,\ \rho \cos l,\ \cos (L - 56^{\circ}\ 49'{\cdot}3)$

First int. cont. ... $3^{h}\ 16^{m}\ 8^{s}{\cdot}4 + 74^{s}{\cdot}92,\ \rho \sin l + 81^{s}{\cdot}32,\ \rho \cos l,\ \cos (L - 56^{\circ}\ 2'{\cdot}4)$

Where l is the geocentric latitude, which may be obtained together with ρ, the radius of earth at the point of observation, from Bessel's Tables in the *Berliner astronomisches Jahrbuch* for 1853, and L is the longitude from Greenwich counted towards the east: the resulting times are for Greenwich. We shall give next week a comparison between observation and calculation.

ENCKE'S COMET.—The *Fürstl. Jablonowski'schen Gesellschaft* of Leipsic have offered a prize in 1881 for a new investigation on the motion of this body, their former similar offer for 1877 not having met with a response. It is urged that the researches of Dr. von Asten, so far as they are known, have not led to any definite result, and other periodical comets not having shown any indications of a resisting action such as is apparent in the motion of Encke's comet, a further complete and separate investigation (*vollständige Neubearbeitung*) is much to be desired. Accordingly the Society's prize of 700 mark is again offered. It is stipulated that all known perturbing forces are to be taken into account, and the calculation is at least to include the period from 1848 to the last appearance of the comet. A similar work for the earlier portion of the interval elapsed since the first discovery in 1786, is reserved as the subject for a future prize.

In connection with the anomalous motion of Encke's comet it may be remarked that Brorsen's comet of short period appears deserving of much closer computation than it has yet received. After that of Encke's comet its perihelion distance is considerably less than in the case of any of the other comets forming this particular group, as the following statement will show. Biela's and De Vico's comets are omitted :—

Perihelion Distance of		Encke's Comet	0·333
,,	,,	Brorsen's	0·595
,,	,,	Winnecke's	0·781
,,	,,	D'Arrest's	1·280
,,	,,	Tempel's (1873, II.) ...	1·339
,,	,,	Faye's	1·687
,,	,,	Tempel's 1867, II.) ...	1·769

GEOGRAPHICAL NOTES

THE Anniversary Meeting of the Geographical Society on Monday was not marked by any unusual feature. The address of the president—his retiring one, as it turns out—consisted as usual of a comprehensive review of the geographical work of the past year, an unusually eventful one in exploration. The Society is as prosperous as ever in members and money. There were on April 30 3,334 Fellows on the register, of whom no less than 762 are life members On the motion of Sir Henry Rawlinson, the meeting adopted an alteration of the rule regulating admission to meetings of exceptional interest, with a view to obviate certain difficulties which have arisen in this respect. The Royal Medals, the award of which we have already announced, were presented to Count Münster, the German ambassador, on behalf of Baron F. von Richthofen, the President of the German Geographical Society, and to Capt. H. Trotter, R.E., personally. The schools' prize medals were also presented to the successful competitors whose names we have before recorded. From the new president, Lord Dufferin, we may next year expect an address marked by unusual raciness, eloquence, and intelligence; Lord Dufferin will probably return to this country in autumn.

THE Committee of the African Exploration Fund of the Royal Geographical Society have at length definitely resolved to despatch a carefully organised expedition to explore the unknown tract of country lying between the caravan road which, as we have before mentioned, is being constructed from Dar-es-Salaam (a few miles south of Zanzibar), to the northern end of Lake Nyassa. Mr. Keith Johnston will, we believe, be in command, and, will be accompanied by another European not yet selected. Should this expedition prove successful, and, what is equally important, sufficient funds be forthcoming, the committee contemplate pushing their explorations to the southern end of Lake Tanganyika, a further distance of 190 miles, thus completing approximately two of the routes sketched out in the circular issued last summer. In order to enable the committee to despatch this expedition, which is expected to furnish important and valuable geographical information, the Council of the Geographical Society have just made a further grant of 500*l.* to the fund, and it is hoped that the public and the subscribers will lend it such additional support as will be required to carry out the objects in view.

AT the last meeting of the Geographical Society of Paris M. de Lesseps stated that Col. Gordon had pushed the Egyptian advanced posts up to the equator, and that now any traveller can go from Paris to the equator within sixty days if he has procured a letter of introduction from M. de Lesseps. Abbé Debaize, who, as we have already stated, intends to cross Africa, has availed himself of this privilege and is probably now on the banks of the Albert Nyanza. M. de Lesseps states, moreover, that the number of lakes is greater than was supposed after Stanley's mission, and Col. Gordon is making a careful survey of the newly Egyptianised country. He has sent to M. Daubrée, Director of the School of Mines, some specimens of gold and silver ores brought from the interior, in order to ascertain their value. The Society has recently received a detailed account of the expedition made by MM. Cambier and Marno from Zanzibar during the past winter. The journey lasted seven weeks, and was accomplished without loss of life. The chief object of this tour was to test the availability of the route by Mpwapwa for expeditions into the interior of Equatorial Africa. It was found to be well adapted even for waggons.

THE Italian Consul at Aden, who is now in Europe, is occupied with the formation of a society for the purpose of acquiring a portion of land and forming an Italian colony at Shoa. The object of the colony is to establish commercial relations between Italy and Central Africa. The African traveller, Carlo Piaggia, is now making the final preparations for a new journey to Equatorial Africa. This journey will be his fourth; formerly he has principally visited Abyssinia and Soudan.

MR. GORDON BENNETT'S polar expedition, to which we have already referred, is not to start, it would seem, till 1879, when, in June, it will probably leave San Francisco for the route by Behring's Straits. The *Pandora*, which will be re-christened the *Jeannette*, is being thoroughly refitted in Walker's yards on the Thames.

NOTES

THE Haarlem Society of Sciences resolved, some years ago, to award, biennially, a medal to the individual who, by his researches, discoveries, or inventions, during the previous twenty years, had, in the judgment of the society, distinguished himself in an exceptional manner in a particular branch of science. This year the medal was to be devoted to astronomy, and on the 18th inst. was awarded to Prof. Simon Newcomb. We believe the medal would have been awarded to Sir George Airy if the committee had felt themselves at liberty to embrace a period greater than twenty years past; but, according to the rules regulating the award, they are rigidly confined to the period stated.

MR. CARL BOCK, F.G.S., who has had considerable experience in the collection of shells and other specimens of natural history, is leaving England at the end of this week for Padang, in Sumatra, in order to explore and collect in the highlands of the interior of the island.

RECENT advices from Auckland state that Signor Beccari and Capt. D'Albertis, a nephew of the New Guinea explorer, were visiting the colony on their way to Europe, and that Signor Beccari had with him some 12,000 specimens of the flora and fauna of New Guinea.

IF Mr. Herbert Spencer remembers his Bible, an oft-quoted passage must have occurred to him on Sunday (appropriately)—"A prophet is not without honour save in his own country and among his own people." On that day, in Paris, where he has been spending a few days, a semi-public dinner was given to the English philosopher by a number of his admirers, headed by the well-known publisher M. Germer-Baillière. Mr. Spencer, in replying to the toast of his health—and he actually replied to a toast, and that too in a style not much out of the common—hinted that he was better known and better appreciated in France than in England, where, so far as we know, he never appeared either on a public or semi-public occasion. A decidedly social evening seems to have been passed by the assembled *savants*, Mr. Spencer concluding his genial and complimentary reply by drinking to the peculiarly French sentiment—Brotherhood (*à la fraternité*).

M. BARDOUX has written to the Meteorological Society of France asking them to organise the congress of meteorology. The committee of this association have written to the Association Scientifique of France, and other societies, requesting them to appoint a number of their members to serve on the committee of organisation. The aeronauts are not pleased because no invitation has been directed to any of the aeronautical societies of Paris.

NOTWITHSTANDING that M. Bardoux has published his decree on the meteorological organisation of France the great commission has held no sitting, and the list of presentations for the successor to M. Leverrier has not yet been deliberated upon. This singular delay is preventing the government from taking any step towards the realisation of the newly-established meteorological institution.

THE "Associations" are waking up once more and beginning to warn all whom it may concern to be ready for the great annual autumnal talk. So far as the British Association is concerned it has confined itself as yet to the usual preliminary advertisement and the private invitation circular intimating that this will be an Irish year—and no doubt Dublin will give a thoroughly Irish welcome—that the opening day is August 14, and that Dr. W. Spottiswoode is the President-Elect. Our American friends in this, as in some other things, are ahead of us, for already we have received the printed circular of arrangements for the twenty-seventh meeting of their Association, which is to be opened at St. Louis a week after our own, on August 21, with Prof. O. C. Marsh as President. Any English men of science who are likely to be in the States about the time of the meeting and who would like to be present, should write to Prof. F. W. Putnam, Salem, Mass., the Permanent Secretary, who, we are sure, will gladly give all necessary information.

IT is stated on good authority that the Water and Woods Department of the French Exhibition will be preserved as a permanent museum, and re-erected in the Bois de Boulogne after the close of the Exhibition. All the specimens of woods exhibited by foreign nations will be purchased if not presented by their respective departments, and added to the intended museum. All the parts of the central building have been constructed so that they can be utilised for railway stations, large markets, &c., and will be sold accordingly. It is stated that the greater part has been already disposed of. We see that parliament has also authorised the purchase of suitable apparatus for the Conservatoire des Arts et Métiers. Although intended originally only to last up to October it is pretty certain the Exhibition will not close before November, according to the universal wish expressed by exhibitors and public.

WITH the month of June will be commenced the visits to the Exhibition of the pupils of the several municipal schools of Paris, under the guidance of their teachers, with free tickets given by the Government in accordance with the votes of the Chamber of Deputies.

A SINGULAR blunder for a city that has established an official committee on lightning conductors has been committed by the architect of the Paris Exhibition; iron conductors have been placed on the central building which is in solid iron. This extraordinary error was also committed in 1867. It shows how little the principles enunciated by Franklin are understood even by scientific people.

GIFFARD's captive balloon is almost ready; the two steam-engines, 150 horse-power each, for working the monster cylinder, will be tried next week. The cylinder, which weighs 49,000 kilograms, has a length of 12 m. and a diameter of 175 ctm.; it will revolve with a velocity of 30 turns per minute. The exact weight of the rope is 1,950 kilos. for a length of 600 m. It has been made at Angers in less than a week, and will be tried within a few days. Next week the apparatus for manufacturing hydrogen gas at a rate of 1,000 cubic m. per hour will be completed. The varnishing of the monster balloon began on Monday; the preliminary ascents and police inspection will take place from June 10 to 20, and the balloon is expected to be opened on the 22nd. The expense will probably exceed £20,000.

THE portrait of Harvey, which we give this week, properly belongs to the previous volume, and ought to be bound along with it. Prof. Huxley's notice of Harvey will be found at p. 417 of that volume.

THE *Sydney Mail* of March 30, we learn from *The Colonies*, contains a letter from a Mr. Severn, dated Newcastle (New South Wales), March 24, in which he gives details of a singular discovery he has made, whereby deaf people can be made to hear by means of the telephone. After describing a very simple telephone which he constructed out of a tin pot, the closed end of which he opened and tied over it a piece of parchment, passing a fine string through the centre and making a knot inside, he says:—"Make a loop in the string some three feet long, put this loop over the forehead of the listener (the deaf man), cause him to place the palms of his hands flat and hard against the ears, let the loop pass over the hands, and now this listener will hear the smallest whisper, let him be deaf or not. This fact may appear extraordinary; it is, nevertheless, true that a deaf man may

thus be made to hear the voice, music," &c. A diagram is published in the *Mail*, showing the working of the telephone as described.

THE cultivation of the opium poppy (*Papaver somniferum*), which has hitherto been exclusively confined to the east, bids fair to become thoroughly established and remunerative in Eastern Africa. Seeds of the best kinds have been imported from Malwa into Mozambique, where 50,000 acres of uncultivated State land have been granted to a company, with a capital of 178,000*l*., for the purpose of cultivating and trading in opium. Besides the grant of land, the company also receives from the State "the exclusive right for twelve years to export opium free of duty through all the custom-houses of the province." It is satisfactory to learn that the poppy plants are thriving, and the fruits are reported to be larger than those produced in the best opium districts of India.

A RUSSIAN medical paper draws attention to *Sarracenia purpurea* as a remedy for gout, administered in the form of a powder in the proportion of one or two teaspoonfuls morning and evening.

THE thirty-second meeting of German philologists and pedagogues will take place at Gera at the end of September.

HERR FERDINAND NOLL, of Brandenburg, has presented to the International Postal Congress, now sitting at Paris, the drawings and descriptive plans of a decimal clock as well as two models of the clock itself. Its object, as its name implies is to introduce a division of time on the decimal system in accordance with that already in use for measures, weights, and moneys. Herr Noll therefore divides his dials into twenty hours and gives 100 minutes to the hour, each minute having fifty seconds, and each second fifty "tertien." Dr. Forster, the Director of the Berlin Observatory, gave a very favourable opinion of the invention when submitted to him two or three years since.

A FRENCH agricultural paper announces the discovery of an extremely simple and cheap means to protect houses from being struck by lightning. This consists merely in bundles of straw attached to sticks or broom-handles and placed on the roofs of houses in an upright position. The first trials of this simple apparatus were made at Tarbes (Hautes Pyrenées) by some intelligent agriculturists, and the results were so satisfactory that soon afterwards eighteen communes of the Tarbes district provided all their houses with these bundles of straw, and there have been no accidents from lightning since in the district.

THE Emperor of Austria has conferred the Cross of the Order of Francis Joseph upon the two well-known African travellers, Drs. Georg Schweinfurth and Gerhard Rohlfs.

THE large botanical library left by the late Prof. A. Braun, formerly director of the Berlin Botanical Gardens, is now being sold by Messrs. List and Francke, of Leipzig.

OUR readers will be glad to learn that Sir William Thomson and Prof. Tait have nearly completed for publication the first part of the new edition of their work on natural philosophy, which will be brought out very shortly by the Cambridge University Press.

NO less than twelve separate subterranean shocks are reported from Ancona as having occurred between May 9 and 12.

THE Government of Uruguay intends to construct a railway which will unite Uruguay with the Province of Rio Grande do Sul, in which many thousands of colonists are settled. The line is to begin on the right bank of the Quarahim River, and is to extend as far as the town of Uruguayana. On the Quarahim River this railway will join the line in course of construction between Salto and Santa Rosa, which is already finished and in use as far as Jacuhi (some 300 miles), and which in turn corresponds with the line between Salto and Fray Bentos, where the great Saladeros (slaughter-houses) of the "Liebig Company" are situated, at which over 1,000 head of cattle are killed daily to make the well-known "Liebig Extract of Meat."

MR. J. M. WILSON, of Rugby, has in the press a treatise on geometry written to correspond with the Syllabus of the Geometrical Association. The work will be published by Messrs. Macmillan and Co.

MESSRS. MACMILLAN AND CO. are preparing for publication a treatise on the nature and origin of coal and the extent of the supply in this country, written by the Professors of the Yorkshire College of Science, Leeds. The authors propose to sketch out the state of the country at the time when coal was coming into being and the processes by which it was formed; next to deal with the present, and give an account of the methods of working coal and some of the uses to which it is now being put; lastly, to endeavour to forecast the future and speak of the probable duration of our coal supply. The work will be edited by Prof. T. E. Thorpe, F.R.S. In it Prof. Rücker will treat the subject from the physicist's point of view; Prof. Miall will discuss its natural history; Prof. Green will take the geology of the question; Prof. Thorpe the chemistry; and Prof. Marshall will write on the economics of coal.

IN a recent paper in *L'Aeronaute*, Col. Laussedat gives the results of experiments made by a Commission appointed by the French Minister of War. For Captive Balloons it is absolutely necessary to employ the best silk and cordage, which, with the least weight, offers the greatest guarantee of durability. After much research a special varnish has been found which renders the aërostat impermeable to gas. Instead of numerous ropes held by men as in former military ballooning, a single cable has been adopted to work by a simple but secure capstan. Capt. Renard has discovered a rapid and economical new process of manufacturing hydrogen. For Postal Balloons Capt. Renard also has devised a secure and easily-worked valve. A liquid instead of a solid ballast has been resolved on, and a fluid is being sought which will not congeal in the low temperatures of the upper atmosphere. The valve and the ballast may work automatically and maintain the balloon at any given height. Among the methods of stopping the balloon experimented on are the javelin anchor of Meusnier and a sort of iron arrow devised by Capt. De la Haye. For Directable Balloons the principles which guided Dupuy de Lôme have, for the most part, been adopted by the Commission. That experimenter found that with an engine of eight horse-power turning a screw he could deviate from the direction of the wind by a considerable angle, with ordinary winds, and even travel, with reference to the earth, in all directions which it would be wished to follow. The Commission, however, instead of placing the screw in the car, at a great distance from the point of application of resistance of the air, have constructed the balloon so that the screw may work in the very centre of the aërostat.

THOSE familiar with the treasures of the Bibliothèque Nationale in Paris will appreciate the importance of a law lately laid before the French Chamber by the Minister of Public Instruction, providing for the demolition of all buildings adjoining the library, in order to insure its complete protection from danger by fire. The great building will in the future be entirely surrounded by an open space laid out in gardens and walks.

THE Paris Jardin d'Acclimatation has just received from the Seychelles Islands three of the largest tortoises known. The heaviest weighs 187 kilogrammes and is $1\frac{1}{8}$ metre in diameter.

AT the meeting of the Institution of Surveyors on the 13th May, Mr. R. W. Peregrine Birch read an important paper on the use of sewage by farmers. Mr. Birch has collected a considerable quantity of statistics on this unsavoury but important subject, from which the following conclusions are

drawn:—1st. That there are upwards of 100 owners and occupiers of land in Great Britain who use sewage for the sake alone of what they can get out of it by agricultural means. 2nd. That of this number more than sixty are tenant farmers who continue to use it although they have, annually at least, the option of ceasing to do so. 3rd. That of the latter number about five-sixths, and of the total number about three-fourths, actually pay money for the use of the sewage, either in the form of out-fall rent, unquestionable increase of land rent, or the price of occasional dressings. Nearly 4,000 acres of sewage land have been referred to, and these are in the hands of more than a hundred distinct occupiers. These occupiers may be divided into three classes:—Those who have to cleanse a certain quantity of sewage on a certain area of land; those who may take, or leave alone, as much of a town's sewage as they please; and those who may take, or leave alone, what sewage can be spared by others having a prior right. The first class occupies 1,670 acres of sewaged land, and deals with the sewage of twenty distinct sanitary districts, or a population of about 200,000 on as many as twenty-one different farms. Mr. Birch's paper will be published as a pamphlet by Messrs. Spon.

AMONG the novelties in the German book trade for May, we notice the following scientific works:—"Teleologie und Darwinismus," Dr. Kalischer (Berlin); "Gedanken über die Teleologie in der Natur," v. Bärenbach (Berlin); "Reisebriefe aus Kordofan und Dar-Fur," Dr. F. Pfund (Hamburg); "Die allgemeinsten chemischen Formeln," Prof. C. Willgerodt (Heidelberg); "Der Sternhaufen χ Persei, beobachtet in der Leipziger Sternwarte von 1867–70," H. C. Vogel (Leipzig); "Die Verbreitung der Atmosphäre," M. Thiesen (Berlin); "Aus der Physik des Luftmeers," G. Münter (Herford); "Praxis der Naturgeschichte botanische, zoologische, und Akklimatisationsgärten, Aquarien, &c.," P. L. Martin (Weimar); "Atlas cœlestis eclipticus viii.," E. Heis (Cologne); "Die Fauna des Graptolithen-Gesteines," K. Haupt (Görlitz); "Bericht über die Beobachtung des Venus-Durchgangs vom 8ten December in Luxor," A. Auwers (Berlin); "Theorie der Wärme," translated from Prof. J. C. Maxwell by F. Neesen; "Das Nervensystem &c., der Medusen," O. and R. Hertwig (Leipzig); "Journal des Museums Godeffroy—A. Garret's Fische der Südsee" (Hamburg); "Fungi italici authographice delineati," P. A. Saccardo (Berlin). The three last are very expensive works.

THE additions to the Zoological Society's Gardens during the past week include three Common Rheas (*Rhea americana*) from South America, presented by Mr. Frank Parish; four Water Ouzels (*Cinclus aquaticus*), British, presented by Mr. R. J. L. Price; a Hairy Tapir (*Tapirus roulini*) from Columbia, two Great-Billed Rheas (*Rhea macrorhyncha*), two Sulphury Tyrants (*Pitangus sulphuratus*) from South America, received in exchange; two Chimpanzees (*Troglodytes niger*) from West Africa, deposited; two Bar-headed Geese (*Anser indicus*) from India, purchased; a Great Kangaroo (*Macropus giganteus*), two Wild Boars (*Sus scrofa*), two Wild Cats (*Felis catus*), born in the Gardens; two Geoffroy's Doves (*Peristera geoffroii*), seven Chilian Pintails (*Dafila spinicauda*), a Yellow-Legged Herring Gull (*Larus leucophæus*), bred in the Gardens.

THE FRENCH METEOROLOGICAL SERVICE

WE learn that M. Mascart has been appointed head of the meteorological bureau. He is professor in the Collége de France, his special subjects being light and electricity. He is author of a work in two volumes, on static electricity.

Last week we gave a brief sketch of the new organisation of the French meteorological service by the government, and this week we are able to publish a translation of the decree, from which it will be seen how much alive the French government is to the national importance of a complete meteorological service. How Article 2, referring to "Titular Meteorologists," "Adjoint Meteorologists," and "Assistant Meteorologists," must surprise our "Meteorological" Council! In France they actually insist upon meteorologists to do meteorological work and to advise upon meteorological matters.

Article 1.—The meteorological division of the Paris Observatory forms a distinct service, which takes the title of "Bureau Central Météorologique." This service comprises the study of the movements of the atmosphere, meteorological advertisements to the ports and to agriculture, the organisation of the meteorological observations, and of the regional or departmental commissions, the publication of their works, and the whole of the researches on meteorology or on climatology.

2. The meteorological service of France comprises titular meteorologists, *adjoint* meteorologists, and assistant meteorologists. The salary of the titular meteorologists varies from 3,000 to 10,000 francs. The *adjoint* meteorologists are divided into three classes, whose salaries vary from 2,500 to 5,000 francs. The assistant meteorologists are divided into two classes, whose salaries vary from 1,500 to 2,000 francs. This staff is distributed among the central bureau and the regional or departmental observatories, in proportion to the needs of these establishments.

3. The scientific staff of the central bureau comprises a titular meteorologist acting as director, two titular meteorologists placed under him, *adjoint* meteorologists, and assistant meteorologists. One of the *adjoint* or assistant meteorologists acts as secretary of the central bureau.

4. The director is charged with the general service of the establishment, the correspondence, the presentation to the minister of the proposed annual budget, the meteorological service, and a detailed account of the yearly expenses. He ought to secure the co-ordination and execution of the works which demand the concurrence of the different services placed under his orders, and see to the regularity of the publications. No order may be given without his authorisation.

5. The scientific works are divided as follows:—(1) Service of advertisements to the ports and to agriculture. (2) Service of the general movements of the atmosphere. (3) Service of climatology and of inspections. Each of the chiefs of the service remits monthly to the director a summary report on the progress of the works, and brings directly before the committee, instituted in the following article, the scientific questions of the service.

6. The heads of the service meet each month, on a fixed day, under the presidency of the director. This committee may hold extraordinary meetings at the instance of the director.

7. The titular meteorologists and the director are nominated by decree, on the proposition of the minister, and after the advice of the council, to be spoken of afterwards. The *adjoint* and assistant meteorologists are appointed by orders after advice of the same council.

8. The heads of the regional meteorological observatories are placed under the authority of the director of the central bureau. Each of these officials addresses to the central bureau, under cover of the minister, the observations and works of his establishment. He proposes to the council, through the director of the central bureau, the advancement of the meteorologists under his orders.

9. The meteorological observatories and stations of every order will be visited annually by the meteorologist of the central bureau charged with the service of climatology and inspections. They may also be visited by the director of the bureau or by a member of the council appointed for that purpose. In cases where the departments or towns contribute to the expenses of a meteorological observatory, the inspection will take place in company with the delegate of the general or municipal council interested.

10. There is established beside the central meteorological bureau, a council composed of (1) A representative of each of the Ministries of Agriculture and Commerce, of Public Works, of War, Marine, Foreign and Home Affairs and of the Administration of Telegraph Lines; (2) Two delegates from the Ministry of Public Instruction; (3) Two members of the Academy of Sciences; (4) The director of the central bureau. The heads of the special service of the bureau are admitted to the council, with a consultative voice for questions which interest them. The members of the council are appointed for three years, by decree, on the proposal of the Ministry of Public Instruction.

11. The council of the central bureau will meet once every quarter on a fixed day. It may hold extraordinary meetings at the instance of the minister. The council gives its advice in the budget proposed by the director, on the construction of buildings or instruments intended for regional meteorological observations, on the collective investigations carried on in the various establishments, on the nominations and promotions of the officials, &c.

12. The president, vice-president, and the secretary of the council are appointed annually by the minister on the proposal of the council.

13. The council holds a general meeting yearly at which may be present the heads of the central bureau and of the regional observatories, the delegates of the regional and departmental commissions, and three delegates of the French Meteorological Society.

A regulation deliberated in council and approved by the minister will determine the mode and number of the delegations.

This meeting will hear the report of the president and council on the work of the year, and, if there are any, the reports and memoirs of the heads of the observatories that receive subventions, and those of the delegates of the regional or departmental commissions. It will discuss the views submitted to it, and transmit them to the minister. The report of the president will be printed.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

A TOWN meeting was held at Liverpool on Friday, the Mayor presiding, with the view of establishing a college for higher education, so as to qualify for degrees in art, science, and other subjects at any of the universities.

LEYDEN.—The university shows an attendance at present of 823 students, divided among the faculties as follows:—Law, 487; theology, 41; medicine, 184; philology, 58; science, 53. The corps of professors numbers 47.

AGRAM.—This young university is attended at present by 348 students, of whom but four are from countries outside Austria.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, May 16.—"Experimental Researches on the Electric Discharge with the Chloride of Silver Battery. Part II. The Discharge in Exhausted Tubes." By Warren De La Rue, M.A., D.C.L., F.R.S., and Hugo W. Müller, Ph.D., F.R.S.

"Note on Legendre's Coefficients." By I. Todhunter, F.R.S.

May 23.—"Observations on Arctic Sea-water and Ice." By Dr. Moss.—The paper consists of physical and chemical observations made during the Expedition of 1875-76 on polar ice and sea-water, and is accompanied by a tabulated statement of chlorine and specific gravity estimations; the latter made by the method devised by Mr. Buchanan of the *Challenger*. The author remarks that the low specific gravity of the Polar Sea (1·02467) indicates that even the deepest samples obtained had already received the dilution characteristic of outflowing polar currents. This low specific gravity was maintained through the winter. The highest temperature observed in the deep stratum of dense warm water in Smith's Sound was below 32°, but since its specific gravity was above that of Atlantic water, the northward flowing current may have a slightly higher temperature at a greater depth. The disturbed proportion of sulphates to chlorides in polar waters is attributed to the littoral source of their dilution and to the difference in the behaviour of the sulphuric and chloric cryohydrates (rather than to absence of fucoidal plants or volcanic influence, as suggested by Forchhammer). A detailed description is given of a *névé*-like stratification in the polar ice, proving, in the author's opinion, that the stupendous floes met with by the recent, and many other expeditions, are due, not to progressive freezings of sea-water, or to the sliding up of thinner ice-fields, but to a perennial accumulation of polar precipitation. The stratification includes and overlies air-carried *débris* of crystalline rocks, chiefly quartz, augite, and magnetite.

The strata are often built upon a conglomerate formation (including salt-water Diatomaceæ) affording evidence of a lateral extension of the floating glacier (by the freezing together of fragments in fissures). The "blue-domed" floes belong to the outer zones of the polar ice-cap, where waste exceeds precipitation. Their undulating surfaces intersecting the horizontal stratification and pitted with the ice-dust left from the layers above, are the surface signs of the decay which finally restores polar precipitation to the ocean in the shape of the increased dilution of outflowing polar currents.

May 25.—"On the Equations of Circles." (Second Memoir.) By John Casey, LL.D., F.R.S., M.R.I.A., Professor of Mathematics in the Catholic University of Ireland.

"Contributions to the Anatomy of the Central Nervous System in Vertebrate Animals. Part I. Ichthyopsida. Section 1. Pisces. Subsection 1. Teleostei." By Alfred Sanders, M.R.C.S. Communicated by Prof. Huxley, Sec.R.S.

Zoological Society, May 7.—F. D. Godman, F.Z.S., in the chair.—Mr. T. J. Parker read some notes on the stridulating organ of *Palinurus vulgaris* which had first been described by Dr. K. Möbius, but on whose observations Mr. Parker offered several criticisms.—A communication was read from Dr. F. Buchanan White, entitled "Contributions to a Knowledge of the Hemipterous Fauna of St. Helena, and Speculations on its Origin." In the first part of his paper the author, after briefly noticing what was known with regard to the fauna and flora of that remote and interesting oceanic island, and mentioning the various theories that had been brought forward to account for their origin, discussed the difficulties of the animals, and argued that they had evidently been derived at a remote period from the Palæarctic region by way of Madeira, the Canaries, and the Cape de Verde Archipelago. In the second part of his communication Dr. F. B. White described the Hemiptera collected in St. Helena by the late Mr. T. V. Wollaston, during the recent visit of that lamented naturalist to the island. The collection included thirty species, of which five were probably introduced; one appeared to be indigenous, but seemed identical with a European species, and the remaining twenty-four were regarded by the author as new and peculiar to the island. Seven new genera and one new sub-genus were created for the reception of ten of the species, the rest, with one exception, being referred to European genera. Specimens and drawings of details were exhibited in illustration of the paper.—Mr. P. L. Sclater, F.R.S., read some further remarks on *Fuligula nationi*, a species of duck from Western Peru, of which he had lately received a nearly adult male from Prof. Nation, the discoverer of the species.—Mr. A. G. Butler, F.Z.S., read the descriptions of a small collection of lepidoptera made at Kingston, Jamaica, by Mr. James J. Bowry.—Mr. Edgar A. Smith, F.Z.S., read a paper containing the description of three new land shells from Jamaica and Borneo.—A communication was read from Mr. D. G. Elliot, F.Z.S., containing a memoir on the fruit pigeons of the genus *Ptilopus*. Mr. Elliot recognised seventy-one species of this genus.

Meteorological Society, May 17.—Mr. C. Greaves, F.G.S., president, in the chair.—A. H. J. Crespi, B.A., M.R.C.S., Rev. David Lamplugh, William Morris, M. Inst. C.E., James Muir, M. Inst. C.E., and Miss E. A. Dymond, were elected Fellows of the Society.—The following papers were read:—On the daily inequality of the barometer, by W. Rundell, F.M.S.—Meteorology of Mozufferpore, Tirhoot, for the year 1877, by C. N. Pearson, F.M.S.—Note on the great rainfall of April 10–11, as recorded at the Royal Observatory, Greenwich, by William Ellis, F.R.A.S.—Observations of Sea Temperature at slight depths, by Capt. W. F. Caborne, F.M.S.

Anthropological Institute, April 30th.—Major-General A. Lane Fox, F.R.S., vice-president, in the chair.—Mr. Francis Galton, F.R.S., read a paper on composite portraits, made by combining those of various persons into a single resultant figure (NATURE, p. 97).—The Director read a paper by Mr. C. Staniland Wake on the origin of the classificatory system of relationships used among primitive people. After criticising Mr. Morgan's explanation of the classificatory system as having originated in the practice of marriage among consanguinei, Mr. Wake proceeded to show that the social condition of the Polynesian peoples, who possessed the simplest form of that system was inconsistent with the origin assigned to it by Mr. Morgan. The

author of the paper then showed, by an examination of various phases of the classificatory system, especially the Australian, that, although kinship may for certain purposes have been originally traced through the mother, the regulations as to marriage were based also on the relationship of a father to his child, and that the ideas which gave rise to those regulations also originated the classificatory system.—Mr. A. L. Lewis described a rude stone monument, known as the "Devil's Arrows," near Boroughbridge, Yorkshire.

Geologists' Association, May 3.—Prof. J. Morris, F.G.S., president, in the chair.—On the coralline oolites, &c., of Yorkshire, by W. H. Hudleston, M.A., F.G.S. The beds between the Kimmeridge clay and L.C.G. may be summarised as follows:—

		Feet.
Supracoralline	Upper Calcareous Grit: "Throstler," Cement Stone, &c.	6–36
Zone of *A. plicatilis*	Coral Rag: Subzone of *Cidaris florigemma*	8–40
	Coralline, Oolite and Middle Grit.	30–80
Upper part of zone of *A. perarmatus* ..	Lower Limestones, including Lower Coral Rag, Passage-beds, &c. ...	20–120

The great mass of limestone in the zone of *A. perarmatus* is an exceptional feature as regards England: fauna Oxfordian, especially towards the base, but the highest shell-bed in some places may be classed with the succeeding zone. The middle grit, a sandbank of variable thickness, developed in the Tabular range, and passing upwards through a series of shelly suboolitic grits (*e.g.* Pickering *Trigonia*-beds) into the coralline oolite, the whole constituting the lower subdivision of the zone of *A. plicatilis*; absence of Brachiopoda. The topographical sections described, and the Howardian oolites compared with those of the Tabular range: lithology of the group.—The coral rag sometimes sharply separated from underlying oolites; sometimes, though more rarely, blended by coralliferous oolites, coral shell-beds, &c. Ammonites rare, but where found in the intercoralline pastes, a peculiar form of *A. plicatilis* (? *A. varicostatus*, Buckl.) prevails. Topographical sections described, and the strong contrast between the coralline oolite of Pickering and the coral rag of North Grimston pointed out. Observations on the silicification of the Rag, and the frequent formation of flints; presumed connection in certain cases, with abundance of sponge spicules, especially "globo-stellates." The supracoralline beds principally argillaceous limestones, and sandy shales, with a capping of upper calcareous grit; numerous ammonites of a lower Kimmeridge type, but other fossils few and badly preserved. A slight sketch of the stratigraphy of the region surrounding the Vale of Pickering, and a brief notice of the palæontology of the entire corallian series in Yorkshire, explanatory of the table of fossils, concluded the paper.

Institution of Civil Engineers, May 21.—Mr. W. H. Barlow, F.R.S., vice-president, in the chair.—The paper read was on the design generally of iron bridges of very large spans for railway traffic, by Mr. T. C. Clarke, M.Inst.C.E., of Philadelphia.

PARIS

Academy of Sciences, May 20.—M. Fizeau in the chair.—The following among other papers were read:—On the temperature of the air at the surface of the ground and in the ground to 36 m. depth, also the comparative temperature of turf-covered and bare ground, during 1877, by MM. Becquerel. The temperature was a little higher on an average in the turf-covered than in the bare ground, and in the former it never sank below zero. In the latter, at 0·05 m., it only once sank below the temperature of melting ice.—On the action of the nervous system on the sudoriferous glands, by M. Vulpian. These glands seem (like the salivary glands) to be subject to two antagonistic influences exerted by different nerve-fibres; the one class, which conduct the exciting influence, nearly all come directly from the bulbo-medullary centres; the other class, which conduct the moderating influence, also emanate from these centres, but indirectly, through the great sympathetic.—A letter from Prof. Hughes stated that by inserting an induction-coil in the circuit, in his arrangement, the Bell telephone could be heard over a large hall. His system was a very sensitive thermoscope, &c.—Application of the telephone on board the cruiser *La Desaix*, by M. Trève. This ship communicated very successfully with another in tow by means of the telephone, the circuit being completed by the sea and copper sheathing. Another application was, fitting a telephone to the head of divers' apparatus.—On alloys of gallium and aluminium, by M. Lecoq de Boisbaudran. One such alloy (containing a good deal of aluminium) is solid but brittle; it decomposes water, with rise of temperature, liberation of hydrogen, and formation of a brown powder, later resolved into white flocks of alumina; nearly all the gallium is liberated in the form of globules. Liquid or pasty alloys may also be formed, with much greater decomposing power.—Production of liquid and gaseous carbonised hydrogens by the action of pure water on a carburetted alloy of iron and manganese, by M. Cloez. Water alone, acting with heat on such an alloy, yields its oxygen to the metal, forming, first, protoxides, which afterwards, by action of air, pass to a higher degree of oxidation. A part of the hydrogen enters into the free state; the rest combines with the carbon to produce hydrocarbons similar to petroleum.—On an induction machine, by M. Gaiffe. To obtain a current as constant as possible he employs a bobbin of elliptical section instead of circular (Siemens); the change of polarity is thus effected gradually during the whole of a half-revolution.—On an allotropic modification of copper, by M. Schützenberger. This is got by electrolysis of a solution of about 10 per cent. of acetate of copper (previously boiled), with two Bunsen or three Daniell elements, the negative platinum plate being placed parallel to the larger positive copper electrode, and 3 to 4 ctm. from it. The allotropic copper is then deposited on the platinum, as brittle, rugous metallic plates, of bronze aspect. The specific gravity is 8·0 to 8·2; that of ordinary copper is 6·9. The moist plates quickly oxidise at the surface in air. Allotropic copper is changed to ordinary copper by heat, and prolonged contact with dilute sulphuric acid.—On a new synthetic method of formation of carbides of hydrogen, by M. Randolph. This is by causing a substance like fluoborethylene to act on oxygenated organic compounds capable of furnishing given carbides by dehydration.—On pelletierine, an alkaloid from the bark of the pomegranate tree, by M. Tanret. This volatile alkaloid is thought to explain the tænifuge properties of pomegranate bark in the fresh state.—On the distinction between luminous and chromatic sensations, by M. Charpentier. The luminous sensibility may change under certain conditions (rest in darkness and exposure to bright rays), while the sensibility to colours remains constant.—On the existence of reflex trembling, in the unparalysed member, in certain hemiplegic subjects, by M. Dejerine. This may be produced, *e.g.*, by flexion of the foot on the leg.—On the terminations of nerves in the sudoriparous glands of the cat's paw, by M. Coyne. The glandular *cul-de-sac* is brought into relation with the peripheric nerve system (1) by nerve tubes losing themselves in the limiting membrane; (2) by cells similar to multipolar nerve-cells situated outside the limiting membrane.—On the unity of forces in geology, by M. Hermite.

CONTENTS

THURSDAY, JUNE 6, 1878

MODERN NAVAL ARCHITECTURE

A Manual of Naval Architecture. For the Use of Officers of the Royal Navy, Officers of the Mercantile Marine, Shipbuilders, and Shipowners. By W. H. White, Assistant Constructor, Royal Navy, &c. (London: John Murray, 1877.)

NO one acquainted with our own and foreign navies can doubt that at the time of the establishment of the Royal Naval College at Greenwich Great Britain had been falling very much astern of other countries in the professional education of its naval officers. In the days when ships were all pretty much alike, and differed chiefly in forms and proportions, the greater progress of other navies in technical science was, perhaps, of no great moment; but in days like the present, when the sea teems with experimental ships flying the royal and mercantile flags of this country, the neglect of known principles, or the failure to discover others, may have the worst results. It was for this reason, among others, that the writer of these remarks long since joined those who pressed for the establishment of a Naval College worthy of the country and of the time, and it was doubtless for this reason also that the Government of the day—and more especially Mr. Goschen, then First Lord of the Admiralty—founded the great Naval Institution which now flourishes at Greenwich. It is there that Mr. White, the author of the work before us, has been engaged in expounding naval architecture to naval officers and other students, and it was in the performance of that important duty that he felt the need of such a work pressed upon him.

Mr. White is himself, in no small degree, the product of previous acts of wisdom on the part of Boards of Admiralty; and as those Boards very often come under just censure for their errors and omissions, it is a pleasure to be able sometimes to record their wise and enlightened doings. Mr. White was a distinguished student (and afterwards a Fellow) of that Royal School of Naval Architecture at South Kensington which Lord Hampton took so much pains in promoting, and which the Admiralty founded; having been elected to a studentship at a competitive examination in a Royal Dockyard School, where he had previously undergone a good and extensive grounding in elementary, and indeed in more than elementary, science. It is gratifying to know that Mr. White, like all the naval architects of high position at present in the Admiralty, thus represents a successful system of Government training, commencing in the Royal Dockyard Schools, and advancing through higher-class royal schools, to the most eminent spheres of professional influence.

The object of the present work is to supply to naval officers, and to such others as may need or desire it, a statement of the general principles which underlie the profession and practice of the naval architect. Without some clear and definite knowledge of these principles it is not possible for naval officers to apply modern vessels to their designed services with skill and success. The loss of the *Captain* and the foundering of the *Vanguard* afforded dramatic examples of the class of disasters which may be expected to result and must result from the imperfect handling of modern vessels; and if all the facts of the case could be known, the loss of the *Eurydice* would probably be brought more or less within the same category. In the case of the *Captain* notwithstanding the grievous well-known defects of her design, the laws which regulate a ship's stability and power to carry canvas if known to and applied by those in command of her, would have suggested the paramount necessity of shortening sail in the wind which capsized her; and the evidence taken after the sinking of the *Vanguard* points clearly to the fact that the prompt closing of certain water-tight doors, and other like measures would, in all probability, have saved the ship, or at least have given her ample time for reaching shallow water. As regards the *Eurydice* it is well-known that she was an experimental vessel, designed by a naval officer many years ago, and that some not very usual features tending to reduction of stability entered into her form. It is obvious that in all such cases—and indeed in the cases of all ships—a knowledge of the principles which enter into their construction and use should, as far as possible, be communicated to those upon whom is placed the responsibility of employing them under all the varying conditions of sea-service; and this not merely for the purpose of securing their safety, but also with a view to their efficient and economical employment. Only the few persons who have had special opportunities of observing the facts can imagine the extent to which the performances of ships in steaming, in sailing, and in other operations, depend upon the knowledge and skill of those who command and work them. It is not too much to say that, as regards vessels which in the main resemble each other, the differences in the officers who command them usually obliterate altogether the distinctive qualities of the vessels themselves, and the relative skill of their designers. To the readers o NATURE it is doubtless needless to dwell upon the desirability of the naval officer, whether of the royal or the mercantile marine, possessing the knowledge of ships, and of naval principles, which Mr. White's work is designed to convey.

In composing this work the author has shown himself most judicious in determining the limit beyond which it would not be well to carry theoretical investigation in addressing naval officers. In each of those chapters in which a temptation to over-indulgence in this respect would most be felt by a man highly trained in the theory of naval architecture, the self-restraint of the author is obvious, and deserves all praise. The book is readable throughout by all naval officers who have availed themselves with energy and spirit of the educational advantages which the royal navy now affords to every officer, and it presents to them, for the first time, a sound, well-selected, and trustworthy summary of naval science such as was beginning to be most strongly felt in the merchant no less than in the royal navy.

In his first chapter the author explains the buoyancy of ships, their subdivision into compartments, and the effect of admitting water into compartments variously placed, and discusses with clearness and with the latest information the vexed question of freeboard. The accuracy and

perspicuity with which this chapter is written are remarkable, and prove the fitness of the author for the work he undertook. Diagrams are given wherever they would serve to assist the reader in readily apprehending the subject. The second chapter is devoted to tonnage measurement, and, while mainly derived from an article contributed by the author to *Naval Science* (a periodical which is no longer published), it embodies all that naval officers need know upon the subject, including the measurement of yachts, and the regulations of the Suez Canal; these latter, we regret to say, are in some respects carried out at present in violation of the understandings which were come to with the British Government, and were announced as authoritative by our Ministers in Parliament. The third chapter is an admirable statement of the principles which regulate the statical stability of ships—a subject upon which Mr. White well deserves to be pronounced a high authority, and one who has worthily extended this branch of science.[1] This chapter, like some later ones, embodies information which the naval architect would do well to study, more especially as regards the effect upon stability of adding, shifting, and removing weights. At p. 63 the author remarks that the question of neutral or indifferent equilibrium has little practical interest in connection with ships, "for which stability and instability are alone important." However true this may have been a short time ago, it is no longer so, for the case of the *Inflexible* has invested the question of infinitesimal stability, and of neutral equilibrium, with a strong and melancholy interest, and an interest which must go on increasing if such ships are repeated. The investigations of the *Inflexible's* state led the writer of these lines to observe and consider a peculiar condition of stability which may, and doubtless will, arise in citadel ships like the *Inflexible*, with unarmoured ends large in proportion to the citadel, and which would add a curious case to the numerous curves of stability with which this chapter is illustrated. It is the case in which the citadel is so formed and proportioned, that the ship, with her ends penetrated, would have no stability in her upright position, but would acquire a small amount on inclining through a greater or less angle, and lose it again on being inclined still further. This case would exhibit itself in a curve of stability by a mere loop, of greater or less length and depth, and at greater or less distance from the origin, according to circumstances. A similar result would of course arise in any ordinary ship, the statical stability of which was *nil* in the upright position, but become positive at an inclination. We mention it here, however, because in the case of a citadel ship it reduces to an absurdity, and to worse than an absurdity, that reference to "range" of stability which has been much too frequent in recent discussions. In a future edition Mr. White would do well to extend his remarks upon this branch of the subject, as it has become one of great practical, and even of vital interest.

The following chapters, on the Oscillations of Ships in Still Water, on Deep-Sea Waves, the Oscillations of Ships among Waves, and Methods of Observing the Rolling and Pitching Motions of Ships, together form a most valuable treatise on a branch of naval science which is both in form and substance essentially modern, and full both of interest and of future promise. Although this part of the work has necessarily been composed chiefly by compilation, it is the result of much labour, and of a close study of a large number of essays and discussions which have appeared from time to time during the last eighteen years. In the first paper ever read (in 1860) at the Institution of Naval Architects, Dr. Woolley said—"One of the chief benefits to be looked for from the Institution which we are inaugurating to-day is a more systematic inquiry into the laws of nature on which the motions of a vessel at sea depend than has hitherto been attempted." These were prophetic words, for the field of labour to which scientific men were thus invited was very soon entered upon by Mr. Froude, who has most worthily and successfully laboured in it ever since. He has been joined by other labourers who have well and steadily advanced the good work, including several able French *savans* (notably M. Bertin, of Cherbourg, and M. Duhil de Benazé), and most recently Dr. Woolley himself, who a month since contributed to the same institution a most able analytical discussion of the constitution and properties of deep-sea waves. No summary of the recent striking developments of science respecting the constitution of sea waves, and the behaviour of ships among them, which can at all compare with that here given by Mr. White can anywhere be found.

The following chapter on the strains experienced by ships is an equally clear and comprehensive statement of another thoroughly modern branch of study. It is primarily based upon a paper published in the *Philosophical Transactions* by the present writer in 1871, in the calculations and construction of which Mr. White largely co-operated. The methods of investigation pursued were novel, and of so detailed a nature as to place the subject on a solid basis of fact, with the result of either setting aside or subverting most of the opinions—for they were but opinions—which had previously prevailed. The present author gives the substance of those labours, and adds to them the results of many others of more recent date, supplementing the general investigation with brief examinations of minor and local strains. The chapters on the structural strength of ships and materials for shipbuilding, although of less immediate value to naval officers than those that illustrate the behaviour of ships in motion, abound with facts which they will find of daily value afloat. In places the author is somewhat more historical and diffuse, perhaps, than is strictly consistent with the object of the work, but all that he records is valuable, and the narrative passages will doubtless add to its attractions, especially in the eyes of the younger officers. With respect to the important question of the "Resistance of Ships" (so designated, as usual, although "Resistance to Ships" would surely be more correct) the author has performed a like service to that rendered in the case of the recent discoveries respecting waves and the oscillations of ships. He has sketched and summarised the existing knowledge of the subject, and here as elsewhere has kept mere abstract considerations and theories well under the control of practical requirements. He has done justice to the recent labours of Mr. Froude in this sphere

[1] By a valuable paper "On the Calculations of the Stability of Ships," read at the Institution of Naval Architects in 1871, the joint production of Mr. White and Mr. W. John, now of Lloyd's Register Office, and by other contributions to the knowledge of this subject.

of investigation likewise, and has shown how advantageous his experiments with models have been. The present Controller and Constructors of the Navy deserve great praise for giving their steady support and appreciation to Mr. Froude in his experimental work.

The remaining chapters of Mr. White's book are on Propulsion by Sails and by Steam, and on the Steering of Ships. While well and clearly written throughout, and embodying all the settled science of these questions, these chapters appear to us to be less firmly based in some respects, and to present more controversial matter than the earlier parts of the work. We are unable, for example, to approve of the strong preference expressed for the compound engine over other types of marine engine. Basing his views upon the actual performances of three separate types of engine—the ordinary old type of jet-condenser engine, the surface-condenser type, and the compound type—the author goes on to claim for the last-named enormous advantages. In making his comparisons and drawing his inferences, however, he seems to us to generalise too freely, and to leave out of consideration many facts and circumstances which we are bound to consider before pronouncing in favour of the compound engine. Nor are we by any means alone in calling in question the pre-eminent merits of this form of engine when regarded in the light of general principles. Among others who have expressed similar views we may advert to Mr. Neil McDougall, himself an officer of the Admiralty (Steam Branch), who in 1875 published an essay on "The Relative Merits of Simple and Compound Engines as applied to Ships of War," and who arrived, after a long and patient investigation of the Admiralty and other records, at these conclusions, viz., "That there is no insuperable difficulty in the way of working simple engines at the same pressure as that in use at present with the compound engine at sea. Equal economy might, then, fairly be expected with the simple engine, specially fitted, as with the compound, under ordinary working conditions;" and, "all available evidence goes to show that it is impossible that the compound engine can be to any serious extent superior to the rival engine, at present pressures, in point of economy." It is worthy of note that the first prize, in a professional competition, was awarded to this essay, the judges being Prof. Cotterill, M.A., F.R.S.; Chief Inspector W. Eames, R.N., the Chief Engineer of H.M. Dockyard, Chatham; and the eminent engineer, J. Penn, F.R.S.

The rapid perusal which alone we have been able to give to this large volume of more than 600 pages has disclosed to us but few blemishes, and these, for the most part, of the slighter sort, and such as are chiefly due to brevity of treatment. We will refer to those only which we observe in the first chapter. The remark that the equality existing between the total weight of water displaced by a ship and her own weight "is equally true of wholly submerged vessels as of ships of ordinary form, having only a portion of their volume immersed," will create difficulty in the minds of some of the readers for whom the work is intended. The principle is doubtless true of vessels which just float, without any reserve of buoyancy above the water's surface, and which, therefore, are wholly submerged; but its obvious inaccuracy when applied to the *Vanguard*, the *Eurydice*, the *Grosser Kurfürst* and other ships which are very unfortunately, but nevertheless very certainly, "wholly submerged" points to the necessity for a modification of the language employed. Again, the statement that "the weight of the ship in tons multiplied by thirty-five gives the number of cubic feet in the volume of displacement" may puzzle some young readers of the work—and there should be many young readers of it among the naval cadets and others—who may fail to see that the number "thirty-five" is derived from the previous statement that 64 lbs. is the weight of a cubic foot of sea-water. It is impossible to make these elementary matters too clear for young sailor-officers. Such students will also require some assistance in understanding the paragraph in which the author explains the pressures acting on the bottom of a ship. Following the mathematical method of assuming the existence and action at every point of a pressure acting "perpendicularly to the bottom," and treating this as made up of three components, the author justly speaks of the vertical components only as affecting buoyancy, and no less justly dismisses the horizontal components, as they must, on each set, "obviously be exactly balanced amongst themselves." No youngster accustomed to the mathematical treatment of forces or pressures will find the slightest difficulty in all this; but we can well imagine less favoured sailors, of all ages, pausing at the statement that the water pressure can "at every point" be resolved into three such components, and searching in vain for horizontal pressures, for example, under the flat bottom of a ship. It is, we are well aware, impossible to avoid difficulties of this kind without great elaboration; but we hold it to be a primary necessity to avoid them to the utmost possible extent in a work like this, which has been expressly written for those very many persons, outside the naval architect's profession, who are "more or less intimately connected with shipping," and desire to get some knowledge of the subject. In one important respect we think the author's explanations of the sub-division of ships into compartments, and of the consequences of admitting water into them, much too brief, viz., that of the division of ships by middle-line bulkheads. This is dismissed with the remark that the advantages of such bulkheads are too obvious to need comment; but however obvious the advantages of the system may be, the effects of admitting water to the divisions so obtained should certainly have been investigated and set forth. In any case it would have constituted both an interesting and an instructive branch of the subject other parts of which the author has treated so fully and so well; but the recent improvements in the *Alexandra* and other twin-screw ships has made it also a subject of great importance. The introduction of the middle-line water-tight bulkhead wherever it could be applied is one of the most valuable of the many valuable improvements introduced by Mr. Barnaby and his staff since the first charge of our Admiralty Naval Construction became theirs, and officers who are "shipmates" of this system (to use a naval phrase) will be disappointed to find it so summarily dismissed from the author's and the reader's notice. The defect is the more obvious because of the singular clearness and fulness with which the general question of sub-division is expounded. The task of mentioning the blemishes and

defects of so excellent and comprehensive a work as this is too distasteful to be pursued through more than a single chapter.

It is impossible to conclude this notice of Mr. White's work, however—which, by the by, is well-indexed and turned out of hand by Mr. Murray in his usual style of efficiency and excellence—without reflecting upon the immense developments which naval architecture has undergone during the present century, is still undergoing, and has yet obviously to undergo. There is not a chapter in this volume of many chapters, which does not abound with illustrations and indications of improvement.

> "Men, my brothers, men the workers, ever reaping something new:
> That which they have done but earnest of the things that they shall do."

Take, for a single example, the material of which the naval constructor builds his ships. How recently is it that wood was in universal use; now far more than nine-tenths of the ships built are of iron. But already iron is being superseded by steel, and a few weeks ago Mr. Martell, the able Chief Surveyor of Lloyd's Registry, said at the Institution of Naval Architects, "The time has now come when it is said by many others besides the manufacturers that steel can be used with as much confidence as iron; and it is held that whilst the properties of mild steel are in every respect superior to those of iron, the cost—having regard to the reduced weight required—will warrant the shipowner, from a commercial point of view, in adopting the lighter material." The manufacture of this "mild steel" for ship-building purposes, and indeed for many other purposes, is, as Mr. White states, "mainly due to the efforts of Mr. Barnaby, Director of Naval Construction, who had previously conducted most of the experiments on steel made in the Royal Dockyards, and done much to develop the use of the material." By insisting upon the combination of increased strength with great ductility in the material, Mr. Barnaby directed the attention of manufacturers to the great importance of turning their energies into a new direction, and the result has been the production of a most excellent material for the purpose, which can now be obtained in any quantity from several firms. But even these most recent forms of steel seem destined speedily to be replaced by the fluid-pressed steel of Sir Joseph Whitworth, who, by the application of enormous force to the metal in a molten state, can solidify and make sound castings which contain whatever proportion of carbon may be desired. By pursuing this process through a thousand details, and many years of costly experiment, this distinguished man has given us a material which is as superior to the best mild steel produced otherwise as that is itself superior to the ordinary forms of ship-building iron, and the process promises to carry us to results of far greater importance still. It is perhaps one of the few reflections which should reconcile us to the persistence of mankind in the pursuit of the arts of war that the eager desire to improve guns and war-ships is continually conducting us to collateral and large improvements in the materials employed for the purposes of civilised life—for commercial ships profit no less than war-ships and guns by the improvements of men like Sir Joseph Whitworth, who has himself contributed immensely to the manufacturing arts, both directly and indirectly. The fields in which Mr. Froude is labouring are not less prolific than this, while the forms of vessels, and the propelling powers applied to them, are receiving continual improvement. In respect of naval architecture, at least, Tennyson is right; and that which we have done is but an "earnest" of the things sooner or later to be done.

E. J. REED

TROPICAL NATURE

Tropical Nature and other Essays. By Alfred R. Wallace. (London: Macmillan and Co., 1878.)

MR. WALLACE tells us that the luxuriance and beauty of tropical nature is a well-worn theme and that there is little new to say about it, and yet he thinks that none have as yet attempted to give a general view of the phenomena which are *essentially* tropical or to determine the causes and conditions of those phenomena. Indeed many very erroneous ideas are commonly entertained about the charms of the tropics and about the brilliant tints of its flowers, and birds, and insects.

In the first three chapters of this most interesting volume Mr. Wallace treats of the climate of the tropics, of its vegetation, and of its animal life. A fourth treats of the humming-birds as illustrating the luxuriance of tropical nature. The next two enter on the discussion of the nature and origin of the bright colours of animals and plants, showing how far and in what way these are dependent on the climate and physical conditions of the tropics. A seventh chapter contains an account of certain curious relations of colour to locality, which are almost exclusively manifested within the tropical zones, while the next and last chapter tries to explain the probable origin of many of the forms of life now characteristic of tropical regions.

Despite its being a well-worn theme and its want of novelty, Mr. Wallace has succeeded in writing a most interesting volume on the peculiarities of tropical life, and this chiefly from the results of his own long experience of nature in the eastern and western tropics of the equatorial zone, while his theory to account for the diverse colours, the special adornments, and the brilliant hues which distinguish certain male birds and insects—a theory quite opposed to that of Mr. Darwin's—cannot fail to attract the attention of all interested in this subject.

Mr. Wallace's account of his theory is perhaps the most important portion of his book; he finds, on close examination, that neither the general influence of solar light and heat, nor the special action of variously-tinted rays, are at all adequate causes for the many wondrous complexities of colours with which we are acquainted. He would therefore take another view, dividing the colours into groups, as they are protective to the creature, act as warning colours, or sexual colours, or typical colours, or simply as in floras, attractive colours.

Mr. Darwin's theory on this subject of colours was that all, or almost all, the colours of the higher forms of animal life were due to voluntary or conscious sexual selection, and that diversity of colour in the sexes is due at least, first of all, to the transmission of colour variations either to one sex only or to both sexes, the difference depending on some unknown law and not being due to

simply natural selection; but Mr. Wallace regards this view as erroneous, and to him the very frequent superiority of the male bird or insect in brightness of colour, even when the general coloration is the same in both sexes, seems to be due primarily to the greater vigour and activity and the higher vitality of the male. He reminds us that the colours of an animal usually fade during disease or weakness, while robust vigour and health add to their intensity. This intensity is most developed in the male during the breeding season. It is also very general in those cases in which the male is smaller than the female. This greater intensity of coloration in the male would be further developed by the combats of the males for the possession of the females. Increased vigour, acting thus on the epidermal system, would soon produce further distribution of colour, and even new tints and markings. Nay, even the remarkable display by so many male birds of their peculiar beauties of colour and plumage may be thus accounted for; for at the pairing season these birds are in a state of the greatest energy. Even unornamental birds, at such a season, flutter and spread out their wings and erect their head-crests or their tail-feathers; and there would be a progressive development of these ornaments in all dominant races, and if those portions of the plumage which were originally erected under the influence of anger or fear became largely-developed and brightly-coloured, the actual display under the influence of jealousy or sexual excitement would be quite intelligible; the males would soon find what plumes were most effective, and would endeavour to excel their rivals.

It will thus be seen that Mr. Wallace's theory of colour might almost be called a molecular one. The causes of colour are due to molecular or chemical changes of certain substances, and on the action on these of light, heat, and moisture. They can be produced or intensified by processes of development, and this as the surface bearing these colours is extended or diminished and as there is a surplus of vital energy; or they may be, as in plants, acted on by some, as yet, unknown local action dependent on the soil or on vegetation.

Doubtless this theory will give rise to much controversy; and in the course of this, no doubt, many important facts will be elucidated. Thus, Mr. Wallace reminds us that, in the case of those female birds with brighter plumage than the males, the females are larger, more pugnacious, and show more of vital energy.

One portion of tropical nature Mr. Wallace has overlooked in the volume—that which spreads its brilliant colouring over the white rocks that lie under the sea. Crowds of lovely forms are here; and they are worthy of a chronicle. E. PERCEVAL WRIGHT

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Extinct and Recent Irish Mammals

PROF. BOYD DAWKINS, in his interesting "Preliminary Treatise on the Relation of the Pleistocene Mammals to those now living in Europe," just published by the Palæontographical Society, places the Irish elk (*Cervus megaceros*) among the prehistoric mammals in consequence of its presence "*in the peat bogs* of England, Scotland, and Ireland;" indeed, in a former monograph on the British Pleistocene Mammals, by Messrs. Dawkins and Sandford, and published by the same Society, it is stated that "the *C. megaceros*, *C. tarandus*, *C. elaphus*, and *Bos longifrons*, have been found associated *in peat in Ireland*."

Now although remains of the red deer and short-horned ox are not uncommon in Irish turbaries, there is not a single authenticated instance of either the Irish elk or reindeer having been discovered *in peat*. This observation as regards the Irish elk was made by Prof. Owen long since (1846) in the "British Fossil Mammals," and a wide field of observation confirms my impression of its truth as far as Ireland is concerned. Moreover, there is no reliable evidence to show that man and the Irish elk, reindeer, mammoth, horse, and bear, were contemporaneous in this island.

With reference to the smaller Irish mammals referred to in Mr. Dawkins's treatise. On the authority of Wilde and others it is stated that both the *Martes foina* and *M. abietum* are natives. The former, at best a doubtful British species, has never been authenticated in Ireland, but the latter is not uncommon.

Again, neither the weazel (*M. vulgaris*) or polecat (*M. patorius*) have any claims to be included in the Irish fauna. As to *Felis catus* there is much doubt, the individuals being in all probability domesticated cats run wild.

In regard to the rodents given in Mr. Dawkins's list as Irish, neither the *Arvicola agrestis* nor the *Arvicola amphibius* have been identified; but on the other hand, the *house mouse* (*M. musculus*), reported absent, is unfortunately too plentiful in many districts.

The red deer is still a native of the mountains around the Killarney Lakes, and until recently a few lingered in the wilds of Connaught, but certainly it is not just now on the Tipperary Mountains, though the fallow deer does occur there. Of the shrews, none of which are given in Mr. Dawkins's list, the pigmy (*S. pygmæus*) is the only species hitherto identified in Ireland. I mention these facts, having lately bestowed much attention to the study of Irish mammals. A. LEITH ADAMS

Royal College of Science, Dublin, May 25

Hints to Workers with the Microscope

I AM now and have been for the last fortnight enjoying a treat which everyone who possesses a microscope, a slip of glass to lay on the stage, and a piece of thin microscopic glass with a little cotton wool, can enjoy for the price of 1*s*. Mr. Bolton, formerly of Stourbridge and now of 17, Ann Street, Birmingham, sends me weekly supplies of rotifers, and has just sent me *Rhinops vitræa* and *Hydatina senta* in great profusion. With ordinary *compressoria* and live boxes these are troublesome to see, as they are very lively rovers. To those who may not know the *Midland Naturalist* or the *Microscopic Transactions*, I recommend a particular method which I recently sent to those publications.—Take a plane glass slide, on it drop one or more of the rotifers in a drop of water about half-an-inch in diameter, and draw off the surplus water, if any, carefully with the empty pipette. Then fray out a very very small portion of cotton wool (I always use a watchmaker's glass in the eye to do all such operations) until it is much extended, and spread out and lay this on the drop. Upon that lay the thin microscopic glass, the thinner the better, and then set up the capillary attraction by gently touching it with a needle. Draw off any superfluous water from the edges with the pocket-handkerchief and you will have a little wilderness of wool in which the rotifer is restrained in its movements, protected from pressure, and within reach of very high powers. The amount of wool depends on the size of the rotifer. Hydatina requires more depth than rhinops. The same plan answers equally well for all roving animals. The poduridæ in particular when placed in deep glass cells are easily seen by this apparatus, and it saves many a weary and vexatious five minutes with the compressorium, which, even at the best, requires with living animals extraordinary patience. The rotifers are easily found and secured with the pipette and a watchmaker's glass in the eye after a very little practice. Mr. Bolton's studio is of the greatest value to naturalists and cannot be too well known, for to those who have not time to look for specimens it is a great privilege to be able to purchase them.

Fort Hall, Bridlington Quay, Yorks, May 25 F. A. BEDWELL

The Virial in Thermodynamics

In my letter on the Virial in NATURE, vol. xviii. p. 39, a line of the description of a force's "radiancy" (as it was there termed) with respect to a given point was accidentally omitted; and the definition should have been the product of the distance of its point of application from the given point or "focus," and the resolved part of the force in the direction of that distance, the last and most important member of which product was unmentioned by some unintentional oversight in the description. It would also be wrong, in the dynamical equation of the virial, the *vis viva* and the radiancy of momentum of a system to range the *vis viva* and virial together (as I did in the letter) in the class of physical agents, bound therefore by known laws of conservation, since either their joint or their separate effects in changing the system's total radiancy of momentum are easily seen, if we suppose one of them, for example, the *vis viva*, to act alone, to be totally unfettered, and therefore their actions to be of a measurable kind, but not subject, like that of natural agents, to any known laws of physical connection.

The rate of acceleration of a fourth part[1] of the triple sum formed of a system's moments of inertia round any three axes at right angles to each other is the rate of change of its total radiancy of momentum, and if the various parts of the system are all moving uniformly in straight lines, their joint *vis viva* measures the rate of this change; but it cannot be said to cause or produce it, since, by the laws of motion, the bodies *unassisted* or left to themselves will continue to produce, by their *vis viva*, the same rate of change, without connection in doing so, with any known physical agent, from whose class, accordingly, it is evident that both *vis viva* and the "virial," or the radiances of a system's forces as linked in an equation with acceleration of total moment of inertia, are formally excluded. The equation has very important applications: as when, on an average of a sensible time, the total moment of inertia remains unaltered, or when a system is apparently at rest: for example, in the case of immobility of a gravitating atmosphere in a state of equilibrium, under any possible assigned law of variation of temperature. But the idea of this state of immobility being a necessary one, which the *vis viva* and virial together of the ponderable mass is constrained to conserve—placing them together in a fixed and definite relationship to each other, or to any other agents of physical phenomena, subject to known laws of conservation—was evidently a totally mistaken and unreal one.

A. S. HERSCHEL

The Meteor of May 12

In NATURE (vol. xviii. p. 105) the statement occurs among the "Notes" that the brilliant bolide of May 12 was seen at Geneva, the local time being said to agree. May I call your attention to the fact that the difference between Greenwich and Geneva is 25 minutes (or 2 minutes more if Berne time is compared). Thus 9.45 is 9.20 Greenwich time, nearly half an hour after the meteor recorded by English observers. It is now a well recognised fact that large meteors come in *groups*, naturally raising the suggestion that such groups form part of a slowly disintegrating mass.

From records I have obtained from Scarborough, Leeds, and Bradford, combined with an excellent observation of the latter part here, and the notices contained in your number for May 16, I find the *probable* positions of beginning and end to be from 4 miles west of Northallerton to 5 miles west of Hawick, a distance of 94 miles, the angle of flight being 38° with the horizon, making an actual course of 108 miles described in about 9 seconds, giving the unusually slow rate of 12 miles per second. This, and one or two other points, make it possible that the course really extended further. But the end was in nearly all instances obscured by clouds and the observations in line with the meteor's course. An exact description of its course by your Edinburgh correspondent (especially as to whether it passed near the zenith) would make this certain.

May I venture to make one or two suggestions to your correspondents who favour you with notes on meteors? When, for any reason, celestial measurements cannot be given, *rough measurements* of the positions made either by holding a ruler in front of you, or, if light allows, by the minutes upon a watch face, which shall enable the actual height, and distances from a point of the compass to be determined, are by far the most valuable items, accompanied of course by exact time, date, and place. Thus a meteor might appear at a height of 15 inches on a base 27″ (an arm's length) and 12″ W. of north. Or, having placed 12 o'clock level, the hand at 10 minutes past might point to the place of disappearance, an angle of 7 minutes (42°) giving the distance E. of south. Prof. Herschel, of Newcastle, gives some capital hints in a letter published in the *Scotsman*, May 1, upon the March daylight meteor. Either he or Capt. G. L. Tupman (or I myself) would at any time be glad of observations, in which case a rough plan, indicating its position among the stars, would be of great value. The position of the meteor with reference to houses, trees, &c., the course across a window, if seen indoors (the observer's position and distance being also given and the points of the compass), and many similar items are very useful for after reference and may lead to very exact determinations.

J. EDMUND CLARK

20, Bootham, York, May 28

[1] The acceleration of the total moment instead of the fourth part of the total moment of inertia was wrongly written in the postscript of my letter as equal to the rate of change of momentum-radiancy. Actual energy and "virial," as defined by Clausius, are also half of the quantities here described as *vis viva*, and radiancy of a force.

"Divide et Impera"

Verily we have divided and subdivided, and as yet are but little nearer the "command" promised.

I am a subscriber to your able magazine, which is extensively read in South America, and beg to bring the following subject to the notice of your zoological readers:—

At this distance of 8,000 miles and at the outskirts of civilisation, books of reference are scarce, or, if existing, difficult ot access. In constructing some zoological tables I am constantly beset by the difficulty of discovering two, three, four, five, six, or more synonyms for the same species, or in the case of a supposed new species find afterwards that the same animal has been described under another name; the genera often differ! the families constantly vary, and even the higher classification is by no means constant.

Where is all this perpetual confusion to end? In the science being destroyed by excessive or faulty nomenclature? We want an Ariadne with her thread to lead us out of the maze, for such it is, especially to young zoologists like myself.

Is it too much to expect that an international zoological congress should be constituted with power to methodise and reduce to order this chaotic classification, and print and publish authorised lists of fauna? How are young naturalists to progress, constantly hindered as they are by wasting weary hours in seeking for that which should be patent at a glance?

Such a congress should, by unanimous consent of the chief zoological societies of Europe, fix immutably not only the superior classification, but also the generic and specific nomenclature; and in the event of new species being discovered, whilst conceding the right to the discoverer and describer to affix its title, this should in all cases be subject to the approval of the International Congress, which might sit permanently in the shape of one or two deputies.

It seems to me the science has already emerged from its swaddling clothes, and it is high time for our scientific authorities to give up that fatal habit of generating and clinging to their own superstitions, and fostering that intense jealousy so characteristic of them, which, leading to multiplicity of systems, leads only to distraction.

There may be aberrant forms yet undecided (there will be such, perhaps, to the end of time); borderlands to be limited; yet there is ample material to fix unalterably and universally the skeleton of that science, to fill in whose details there are multitudes of willing and skilled hands, ready to aid, in all parts of the world.

E. W. WHITE

Buenos Ayres, May 1

A Quadruple Rainbow

In the afternoon of Friday, the 24th ult., while proceeding by rail to Dublin, and before reaching Abbeyleix station, I observed the curious phenomenon of four rainbows forming a single bow—that is, without any dark space intervening between the colours. The four bows were all of the same, or nearly the same, breadth, but I cannot say whether all the colours were present in each.

The brighter colours—as the red and yellow—showed that the bows were arranged in the same order.

I called the attention of several other passengers to the novel spectacle.

A word in explanation of this strange appearance from some of your learned contributors would, I think, be interesting.

Model School, Waterford, June 1 HENRY P. DOWLING

Classes for Women at University College

IN view of the new charter enabling the University of London to confer degrees on women, and the increased demand for a higher education of women, the council of this college have determined to provide for them systematic instruction in regular college classes.

In most subjects the junior classes for women will be distinct from those attended by male students. The senior classes will more generally be open to both sexes, and these classes, which are already open to both, as fine arts, philosophy of mind, &c., will remain so.

Prospectuses embodying the results of this change will be ready by the 18th inst. TALFOURD ELY

University College, London

[OUR St. Petersburg correspondent, "C. S." must send us his name (in confidence), before we can publish his last communication.]

PROF. JOSEPH HENRY, LL.D.

PROF. HENRY was born December 17, 1797, at Albany, New York, where also much of his early life was passed. The year of his birth seems, however, uncertain, some authorities placing it in 1799, or even later. He had at first the advantages of only a common school education. A parish library supplied him with boyish reading, and his early tastes were in the direction of romance and the drama. He was nearly grown when the accidental possession of a copy of Robinson's "Mechanical Philosophy" turned his thoughts towards natural philosophy. After two years of work as a watchmaker, he came under the training of the Albany Academy, where he developed a degree of mathematical talent which, in 1826, led to his selection for the duties of instructor in mathematics in that institution. Prior to this, having had some experience in the field as a surveyor, he was associated with Amos Eaton in the Geological Survey along the line of the Erie Canal, projected and sustained by General Stephen van Rensselaer. Failing physical health led to his taking this step. He returned home with a robust constitution, which never failed him throughout his life.

While occupied with his duties as mathematical instructor in the academy—then in charge of Dr. T. Romeyn Beck—he commenced that line of investigation in electricity which resulted in the important discoveries that have made his name famous. He attended the lectures on chemistry of Dr. Beck, and assisted in the preparation of his experiments. At this time he devised and published an improved form of Wollaston's sliding-scale of chemical equivalents, in which hydrogen was adopted as the radix—a contrivance which is hardly known, even by name, to the present generation of chemists. Thus, while Prof. Henry's original contributions to science were chiefly physical, his first scientific work was in the department of chemistry. His work with Dr. Beck enabled him, after his removal to Princeton—where he became professor of natural philosophy in 1832,—to take up the duties of the chemist, Dr. John Torrey, when that well-known teacher was disabled for a time by ill health.

It was in the interval between 1828 and 1837 that the most important work of his life was accomplished in the line of strictly scientific research.

If we compare the poverty of his apparatus and the poverty of his means for research and publication with the importance of the results which he reached, we may accord him a place by the side of Faraday as an experimentalist. He became the sole discoverer of one of the most singular forms of electrical induction, and was among the first, perhaps the very first, to see clearly the laws which connect the transmission of electricity with the power of the battery employed. One of the problems to which he devoted himself was that of producing mechanical effects at a great distance by the aid of an electro-magnet and a conducting wire. The horse-shoe electro-magnet, formed by winding copper wire round a bar of iron bent into the form of a U, had been known before his time, and it was also known that by increasing the number of coils of wire greater force could be given to the magnet, if the latter were near the battery. But when it was removed to a distance the power was found to weaken at so rapid a rate that the idea of using the electro-magnet for telegraphic purposes seemed hopeless. Henry's experiments were directed toward determining the laws of electro-motive force from which this diminution of power resulted, and led to the discovery of a relation between the number of coils of wire round the electro-magnet and the construction of the battery to work it. He showed that the very same amount of acid and zinc arranged in one way would produce entirely different effects when arranged in another, and that by increasing the number of cells in the battery there was no limit to the distance at which its effects might be felt. It only remained for some one to invent an instrument by which these effects should be made to register in an intelligible manner, to complete the electro-magnetic telegraph, and this was done by Morse. Henry himself considered the work of an inventor as wholly distinct from that of a scientific investigator, and would not protect the application of his discoveries, nor even engage in the work of maturing such applications. He never sought to detract from Morse's merits as the inventor of the magneto-electric telegraph, but did on one occasion, under legal process, give a history of the subject which was not favourable to Morse's claim to the exclusive use of the electro-magnet for telegraphic purposes. Some feeling was thus excited; but Henry took no other part in the controversy than to ask an investigation of some charges against himself contained in an article of Morse's.

The results of these researches are chiefly recorded in the *Transactions* of the Albany Institute, the volumes of the *American Journal of Science and Arts* for the period, and the *Transactions* of the American Philosophical Society. His "Contributions to Electricity and Magnetism" were collected in a separate volume in 1839. The analysis of these important researches, and the discussion of the questions of priority connected with them, will be the duty of the academician to whom shall be assigned the preparation of a memoir or eulogy of the distinguished author.

The memoir in the *American Journal* gives a list of twenty-two memoirs and discoveries by Prof. Henry. To these papers should be added an important series of communications, made chiefly to the National Academy of Sciences during the past four or five years, upon the laws of acoustics as developed in the course of investigations conducted for the Light-House Service in order to determine the various conditions involved in the transmission of fog-signals. These investigations have been carried forward mainly in government vessels, and occupied Prof. Henry's close personal attention during many weeks of each season.

Besides these experimental additions to physical science, Prof. Henry is the author of thirty reports, between the years 1846 and 1876, giving an exposition of the annual operations of the Smithsonian Institution. He has also published a series of essays on meteorology in the Patent Office Reports, which, along with an exposition of established principles, contain many new suggestions, and, among others, the origin of the development of electricity, as exhibited in the thunderstorm.

In 1837 he visited Europe and made the acquaintance

of Faraday, Wheatstone, Bailey, and other eminent physicists, discussing with Wheatstone their projects for an electric telegraph. He returned to his lectures with the zest and vigour acquired by this exchange of views with men of like pursuits with himself, and held his place as the foremost of American scientific teachers until 1846, when he was called to an entirely different sphere of activity.

Ten years before Congress had accepted, by a solemn act, the curious bequest of James Smithson, made to the United States in trust, "to found at Washington an establishment for the increase and diffusion of knowledge among men." The will gave no indications whatever as to the details of the proposed establishment, and long consideration was therefore necessary before the Government could decide upon its organisation. It was not until 1846 that a definite plan of organisation was established by law. When this was done Prof. Henry was at once looked upon as pre-eminently the man to be the principal executive officer of the institution. He accepted the position with "reluctance, fear, and trembling," upon the urgent solicitation of Prof. Bache. From the beginning two different views of the proper direction in which the energies of the establishment should be devoted have been entertained. There was a scientific party, which held that the operations of the establishment should be confined strictly within the limits prescribed by the donor, and in the sense in which he himself, as a scientific investigator, would naturally have construed his own words—in fact, that it should be entirely an institution for scientific research and publication. Another party was desirous of giving it a larger scope and wider range, including literature and art as well as science. The new secretary, of course, sympathised entirely with the scientific party, who considered most of the objects proposed by the other party as foreign to the proper purpose of the institution, and the expenditure of money upon them as contrary to the expressed intention of the donor. The whole policy of Henry was directed towards diminishing, as far as possible, the expenditure of the Smithsonian fund upon the library, the building, the museum, and art-gallery, by having these several objects provided for in other ways. He got the library removed to the Capitol and deposited in the Library of Congress, and the art-gallery superseded by the Corcoran Gallery of Art. The impropriety of charging the Smithsonian fund with the support of the governmental collections was so obvious that Congress has for several years provided for the maintenance of the National Museum, as it has now become, in connection with the institution. He aimed at a complete separation of the museum from the institution; the Government leasing the building for the use of the former, while the latter should find more modest and appropriate but less expensive quarters. This project, however, he did not live to carry out.

Henry was, of course, the authority most frequently and regularly consulted by the Government on all questions which arose involving applications of science or of scientific principles. His greatest services to the Government were rendered as a member of the Light-House Board, a position which he held from the time the Board was organised. His principal duties were at first to inquire into the various methods of illumination, and especially to test the oils proposed for this purpose. Of late years he began to investigate the subject of fog-signals, which led to a very extended series of experimental researches on the causes which influence the propagation of sound through the air, and which sometimes render it inaudible at comparatively short distances. These experiments were mostly published in the annual reports of the Light-House Board.

The idea of using the telegraph for communicating the weather reports originated with Professor Henry, and was put in operation at the Institution at an early period of his connection with it.

It was while engaged in the discharge of certain experimental work on Staaten Island last December, connected with the photometric laboratory of the Light-House Board, that he experienced a partial paralysis, which yielded soon to treatment, but was doubtless the precursor of the nephritic attack to which he succumbed. In April he presided at the opening meeting of the session of the National Academy of Sciences, held in the rooms of the Secretary of the Smithsonian, and submitted an address to his associates, read by the Home Secretary, recounting with touching simplicity his recent decline of power, and expressing his desire to be relieved from the cares of the office of President. As a mark of affectionate respect, the Academy unanimously requested him to retain this post during his life—leaving the duties to be discharged by the Vice-President. It was on this occasion that the announcement was made to the Academy, by Prof. Henry, and, subsequently, in fuller details, by Prof. Fairman Rogers, the treasurer, of the creation of an endowment to be called "the Joseph Henry Fund." This fund consists of forty thousand dollars, securely invested, the income of which is for the support of Prof. Henry and that of his family, during the life of the latest survivor. Afterwards the fund is to be transferred, in trust, to the National Academy of Sciences, the income to be for ever devoted to scientific research. No more graceful and well-merited tribute of respect and affection was ever bestowed upon a man of science by the spontaneous offerings of personal friends and associates. Alas! that its honoured object should have remained so brief a time to enjoy the peace and satisfaction of this gracious endowment. If it is true that republics are ungrateful, it is pleasant to know that the absence of imperial and kingly patronage may be compensated by a sovereignty not less potent.

The whole course of Prof. Henry was marked by an elevation of character entirely in keeping with his intellectual force. Placed in a position where the temptation to lend the use of his name to commercial enterprises was incessant, he so studiously avoided every appearance of evil that the shadow of suspicion never rested upon him. His services to the Government in many capacities, especially in that of member of the Light-House Board, where his experiments saved it hundreds of thousands of dollars, were entirely gratuitous. His salary was paid from the Smithsonian bequest, and he never asked the Government for a dollar on account of his services. An elevated but genial humour, a delicate poetic taste, a memory replete with anecdote, a refined intellectual face, and an impressive bearing made him one of the most valued members of the intellectual society of Washington.

Prof. Henry leaves a wife and three unmarried daughters, who have been assiduous helpers in the scientific work of their father, making good to a degree the loss of an only son, whose death in early manhood was a sad disappointment of parental hopes and youthful promise.

Prof. Henry was buried May 16, in the Rock Creek Cemetery, near Georgetown, D.C. The President of the United States, the cabinet officers, diplomatic corps, and members of Congress and of the National Academy, were among the mourners.

Prof. Spencer F. Baird, long the Assistant-Secretary to the Smithsonian Institution, was, on May 17, unanimously elected by the Board of Regents as Prof. Henry's successor in the office of Secretary of that institution. No more acceptable appointment could have been made.

We express our indebtedness to Prof. Silliman, who has kindly forwarded us early sheets of an article on Prof. Henry to appear in the *American Journal of Science and Arts*. From this, together with an article in the New York *Nation*, we have gathered most of the above details of Prof. Henry's life and work.

MAJOR-GENERAL SIR ANDREW SCOTT WAUGH

FROM a paper in the *Royal Engineer Journal*, we obtain some interesting facts concerning the career of this able officer of the Indian Survey, whose death we announced at the time (vol. xvii. p. 350). Having been appointed in July, 1832, and retiring in 1861, his services in the department extended over a period of close upon thirty years.

Under Col. Everest, as his astronomical assistant, Waugh took part in the measurement of the great arc of the meridian extending from Cape Comorin, the most southern point of the peninsula of India, to the Deyrah Doon at the base of the Himalayas. In December, 1834, we find him with his Chief at the measurement of the northern base-line in the above valley, an operation that extended over a year. In the connection of this base with that near Sironj, about 450 miles to the south, Waugh took a large share of work, and also, in 1837, at the re-measurement of this Sironj base with the new bars that had been used in the Deyrah Doon. The wonderful accuracy secured on these grand operations may be estimated by the difference of length of the Deyrah base-line, as measured, and as deduced by triangulation from Sironj, being only 7·2 inches.

He shared with Everest the arduous observatory work, carried on simultaneously at the stations of Kaliana Kalianpur, and Dumargidda, by which the arc of amplitude was determined, and brought this important work to an end in 1841. Between 1834 and 1840 he was also conducting the Ranghir series in the North-West Provinces, and in 1842, the triangulation through the malarious Rohilkund Terai, which Everest acknowledged to be "as complete a specimen of rapidity combined with accuracy of execution as there is on record." Sir Andrew thus had the good fortune to be the immediate pupil of the great geodesist who placed the Indian Surveys on their present footing, for the whole system was then. elaborated and brought to a high pitch of excellency This, Waugh, on succeeding to the appointment of Surveyor-General and Superintendent of the Topographical Survey in 1843, made it his first object to keep up and improve. Sir George Everest's high opinion of the man who had served under him on so many important operations may be understood from the singularly strong terms which he used when recommending Waugh as his successor.

He began by carrying out the remaining series, seven in number, a total of 1,300 miles in length and embracing an area of some 28,000 square miles, originating from the Calcutta longitudinal series on the *gridiron system* projected by Sir G. Everest. The eastern side was formed by the Calcutta meridional series (begun in 1844 and finished in 1848), which terminated in another base-line near the foot of the Darjeeling hills.

One of the finest of surveying operations, commenced about this period of Sir A. Waugh's tenure of office, was the north-east Himalaya series, connecting the northern ends of all the before-mentioned meridional series. In these field-operations Waugh took a leading part. The line of country was along the base of the Himalayas (the Terai). These operations led to fixing the positions and the heights of some of the highest and grandest of the Himalayan peaks in Nipal and Sikkim; one of these, 29,002 feet above the sea, was named by Waugh Mount Everest, and was found to be the highest in the world; its name, one well-known to the natives, is Devidanga. Mr. Clements Markham, in his exhaustive memoir on the Indian Surveys, states that the dangers and difficulties in the execution of this work were air greater than have been encountered in the majority of the Indian campaigns.

On the South of India, the South Concan, the Madras coast series, the South Parisnath and South Maluncha series were also begun and finished, and several pages might be written of the dangers and difficulties the Survey Staff had to contend with. Of all the Indian Survey work that originated in Col. Waugh's tenure of office, on account of the general interest attaching to the country, its beauty, and its vastness, the survey of Kashmir was chief. This important and difficult survey, finally completed in 1864, was in full swing of work at the time Sir Andrew Waugh retired from the department, and we cannot do better than quote the lines the late Lord Canning was pleased to write privately to Sir Andrew Waugh in July, 1859, on being shown the first instalment of this work. Coming from so high and intelligent a source, they are a tribute not only to the Surveyor-General, but to the whole department. "I cannot resist telling you at once with how much satisfaction I have seen these papers. It is a real pleasure to turn from the troubles and anxieties with which India is still beset, and to find that a gigantic work of permanent peaceful usefulness, and one which will assuredly take the highest rank as a work of scientific labour and skill, has been steadily and rapidly progressing through all the turmoil of the last two years. I never saw a more perfect or artistic-like production of its kind than this map."

The other meridional series were also pushing forward. Jogi Tila by Jhelum and the Gurhagurh by Umritsur to join the Arumlia. Kattywar and Cutch must also be included. For a full and detailed account of these many difficult operations that comprise every kind of country and climate that India presents, we must refer the reader to the memoir before-mentioned.

Space will not allow us to enter into detail of all the important work done for the survey of India during Waugh's tenure of office, but it may be stated roundly that he advanced the triangulation by no less than 316,000 square miles, an area three times that of England, Wales, Scotland, and Ireland, and of this 94,000 were topographically surveyed. Col. Waugh retired from the service in 1861, receiving as usual the honorary rank of Major-General, and Her Majesty conferred on him the honour of knighthood in the same year. He had held the post of Surveyor-General for seventeen years, had maintained the high character of the survey, and was highly esteemed by the whole department. The results of the work during his incumbency are given in some thirteen different volumes and reports deposited in the India Office, parts of which originally complete appear to have been lost. In 1856 the Royal Geographical Society presented him with their gold medal, and in 1858 he was elected a member of the Royal Society. For some years past his health had been failing, and he suffered much, dying on February 21, 1877, at his residence in South Kensington.

THE HARVEY TERCENTENARY

ON Saturday evening the Royal College of Physicians commemorated the tercentenary of Harvey, the discoverer of the circulation of the blood, by a banquet in the library of their institution in Pall Mall. The president, Dr. Risdon Bennett, occupied the chair, and the company included the Marquis of Ripon, Viscount Cardwell, Mr. Gladstone, M.P., Mr. Lowe, M.P., Mr. Spencer Walpole, M.P., Baron Cleasby, Mr. Justice Denman, Prof. Huxley, Dr. Allen Thomson, Prof. Owen, Capt. Cameron, R.N., Dr. Carpenter, Mr. Benett-Stanford, M.P., Dr. Lyon Playfair, M.P., the President of the Royal College of Surgeons, the President of the College of Physicians, Ireland, Dr. Richardson, Sir W. Jenner, and Sir W. Gull

The Marquis of Ripon and Mr. Walpole, M.P., in responding to the toasts of the House of Lords and the House of Commons, respectively, paid high tributes

to the memory of Harvey and to the reputation of the College of Physicians, while Lord Cardwell spoke with much satisfaction of the legislation resulting from the investigations of the Commission on Vivisection. The speech of the evening, however, was undoubtedly that of Prof. Huxley, in responding to the toast of the evening, "The Memory of Harvey," proposed in happy terms by the President. Prof. Huxley replied as follows:—

Mr. President,—In attempting to fulfil the task you have imposed upon me, I am mindful that I address myself to an audience which is already familiar with William Harvey's claims to the honour which we are assembled to show him. For, within these walls, the memory of your illustrious Fellow and chief benefactor, is kept perennially green by the customary piety of the speaker of the annual oration which Harvey founded; and his merits have been placed before you, with exhaustive completeness, by a long succession of able and eloquent orators. Even if the time and place were fitted for a disquisition on these topics, I could not hope to be able to add to the facts already known, or to place them before you in a new light. And, happily, this is not my function; I have to act simply as your remembrancer, to play the part of the herald who announces the familiar titles of a monarch on a state occasion.

Harvey's titles are three—he was the discoverer of the circulation of the blood; he wrote the "Exercitatio de Motu Cordis et Sanguinis"; he formulated anew the theory of epigenesis, and thereby founded the modern doctrine of development.

His first and, in general estimation, his greatest title to our honour has been challenged; but only to the confusion of the challengers. A century ago, your Fellow, Dr. Lawrence, in the excellent memoir prefaced to the College edition of Harvey's works, met the arguments of those who had, up to that time, attempted to dim his fame, with a solid refutation, which has never been answered and to my mind remains unanswerable. In our own day, Dr. Willis has stated the facts of the case, and deduced the inevitable conclusion, with no less force and cogency. And, having taken some pains to get at the truth of the matter myself, I may state my clear conviction that Harvey stands almost alone among great scientific discoverers, not so much that, as Hobbes said, he lived to see the doctrine he propounded received into the body of universally accepted truth, but because that doctrine was both absolutely original, and absolutely new. I have yet to meet with a single particle of evidence to show that, before Harvey declared the fact that the blood is in constant circular motion, there was so much as a suspicion on the part of any of his predecessors or contemporaries that such is the case. Neither in Galen, nor in Servetus, nor in Realdus Columbus, nor in Cæsalpinus, is there a hint that a given portion of blood sent out from the left ventricle, passes through the body and the lungs, and returns to the place from whence it started; yet this is the essence of Harvey's discovery.

Hence when we hear of pompous inscriptions being put up in Spain to Michael Servetus, "the discoverer of the circulation," or in Italy to Cæsalpinus "the discoverer of the circulation;" it is well to recollect that churchyards have no monopoly of unhistorical inscriptions. Indeed, have we not ourselves, within easy walking distance, that famous monument, the subject of Pope's scathing but just lines—

> "And London's column, soaring to the skies
> Like a tall bully, lifts its head and lies."

Sir, I have no sympathy with Chauvinism of any kind, but, surely, of all kinds that is the worst which obtrudes pitiful national jealousies and rivalries into the realm of science. We will not shame ourselves by permitting the fact of Harvey's English birth to enter into the consideration of his claims as a discoverer; but those claims once established beyond dispute, it is, I hope, something nobler and better than mere national vanity which brings us together to celebrate his birth; to take an honest pride that such a man came of our English race; and as, I hope, to feel the deep responsibility which is laid upon us to have a care that the stock which in the same hundred years bourgeoned out in a Harvey and a Newton, shall not have its capacity for producing like growths in the present and in the future, starved by devotion to mere material interests, or stunted by ignorant outcries against scientific investigation.

The second title which I have claimed for Harvey is that of author of the "Exercitatio de Motu Cordis et Sanguinis." And that title is, happily, quite indisputable. But some may suppose that I have so far thrown myself into the spirit of my assumed office as to insert a superfluous appellation—a sort of "Defender of the Faith." However, this would be an error. Harvey might have discovered the circular course of the blood; he might have given sufficient evidence of his discovery; and yet he might have been quite incapable of writing that little essay of fifty pages which no physiologist of the present day can read without wonder and delight. For, not only is it a typical example of sound scientific method and of concise and clear statement; but, in addition to the evidence of the course of the blood through the body, it contains the first accurate analysis of the motions of the heart; the first clear conception of the mechanism of that organ as a pumping apparatus; the first application of quantitative considerations to a physiological problem; and the first deductive explanation of the phenomena of the pulse and of the uses of the valves of the veins. "Libellus aureus," Haller called it—and never was epithet more aptly bestowed.

Harvey's third title to honour is the authorship of the "Exercitationes de Generatione." In this treatise Harvey grapples with two of the most difficult problems of biology—the physiological problem of generation and the morphological problem of development. It was simply impossible that he should solve these problems, for they can be approached only through the microscope; and Harvey was dead before Hooke, Malpighi, Swammerdam, or Leeuwenhoek, the fathers of microscopy, began their work. He saw the circulation in shrimps "ope perspicillo" indeed—but the perspicillum was a mere handglass. Hence it is not wonderful that Harvey's theory of fecundation is altogether erroneous: and that he is no less mistaken respecting the nature of the parts of the embryo which first make their appearance and the mode of their formation.

Nevertheless, just as it is the fate of dulness to be blind to the significance of justly observed facts, so is it the rare privilege of men of the highest genius to discern the true light among the *ignes fatui* of error. They know the truth, as Falstaff discerned the true prince among his pot companions, by instinct. Explain the matter how we will, it is an indubitable fact that though Harvey's fundamental observations were either inadequate or erroneous, some of his most important general conclusions express the outcome of modern research.

For a whole century Harvey's successors, even though the illustrious Haller was among them, went wrong when Harvey was right; and though Caspar Wolff returned to Harvey's views and thereby laid the foundation of modern embryology, the definitive triumph of the doctrine of epigenesis is the result of labours which have been effected within the memory of living men.

Such appear to me to be the chief claims of Harvey to be held in everlasting honour among men of science. We know that they represent a mere fraction of what he did. But the violence of an unhappy time has robbed us of the rest. I should trespass unwarrantably on your time if I insisted on the applications of Harvey's discoveries to medicine and surgery in the presence of those whose daily avocations bear witness to them.

I have hitherto dwelt upon the claims to our honour of Harvey the philosopher; one word, in conclusion, concerning Harvey the man. There have been great men whose personality one would gladly forget: brilliant capacities besmirched with the stains of inordinate ambition, or vanity, or avarice; or soiled by worse vices; or men of one idea, unable to look beyond the circle of their own pursuits. But no such flaw as any of these defaces the fair fame of William Harvey. The most that tradition has to say against him is, that he was quick of temper and could say a sharp thing on occasion. I do not feel disposed to cast a stone against him on that ground; but rather, such being the case, to marvel at the astonishing, not only self-control, but sweetness, displayed in his two short controversial writings—the letters to Riolan; a man who really was nothing better than a tympanitic philistine, and who would have been all the better for a few sharp incisions.

Moreover, in such a temperament, while the love of appreciation is keen, the sense of wrong at unjust and wilful opposition is no less strong. But I do not recollect, in all Harvey's writings, an allusion to the magnitude of his own achievements or an angry word against his assailants.

Ready to welcome honour if it came, but quite able to be content without it; caring little for anything but liberty to follow in peace his search into the ways of the unfathomable cause of things—"sive Deus, sive Natura Naturans, sive Anima Mundi appelletur"[1]—one fancies this man of the true Stoic stamp would have summed up his eighty years of good and evil in the line of the poet, which was the favourite aphorism of his great contemporary, Descartes—

"Bene qui latuit bene vixit."

But he lived too well that the memory of his life should be allowed to fall into oblivion; and we may hope that recurring centennial anniversaries will find our successors still mindful of the root from whence their ever-widening knowledge has sprung.

After this Mr. Lowe replied in his usual racy style to the toast of the Universities, naturally having a little fling at the aspirations of Owens College and other recent institutions. Mr. Lowe remarked that anything like competition among the persons who conferred degrees and honours must be productive of evil. The result of such a system had been a kind of Dutch auction of degrees and honours, there being in some quarters a desire to secure as many students as possible by lowering the standard of qualification; but he was happy to think that that evil was about to be remedied, and that they were approaching a time when they would obtain what not only the medical profession, but every individual in this country had a right to demand, namely, that no one should be allowed to heave the lead into the depths of his fellow creature's physical constitution without possessing a certain proved degree of skill. That had been the dream of all sound medical reformers for a long time. It had hitherto remained only a dream, but as he had indicated, it was about to be realised, and he was bound to say that, as far as he understood the question, it was about to be so mainly through the noble and disinterested conduct of the universities, who, instead of displaying selfishness, had expressed their readiness to surrender the privilege they now enjoyed of admitting persons to the medical profession, and to hand over this duty to a certain body possessing the power of fixing a standard of qualification below which no person whatever should be admitted to practise.

Mr. Gladstone, in responding to the toast of "General Science and Literature," said—Great as had been their profession in former times, every one must feel that it was growing greater, wider, more solid from year to year and from generation to generation. He did not speak now of literary culture; for although he felt that literature had stood in a very important relation to the medical profession of late years, still literature was necessarily fluctuating, and had been so in all periods of the world. They had gone through a great literary age, as other races had done before, and they could hardly expect the succeeding generation to maintain the same literary level. But as regarded science the case was very different. Nothing here seemed to be required but that patient labour which it was in the power of all men to bestow, together with those large opportunities for observation which we all enjoyed in some degree if we would but use them, and which medical men perhaps enjoyed in a greater degree than any other class of men. As society was developed, as civilisation became more elaborate, as the wants of men, as the enjoyments of men, and as, perhaps, also the dangers of men multiplied, and as the connection of body and mind, which was daily under their eyes, became revealed, they would find their way more and more into the very innermost chambers, so to speak, of human nature. As science progressed their responsibilities would increase, but he was sure they would never be wanting in that capacity and zeal which had ever distinguished them, and that in proportion as their influence over human welfare and human happiness increased, they would obtain that respect and gratitude which, amid their imperfections, mankind were ever ready to extend to their benefactors.

[1] "Exercitationes de Generatione," Ex. 50.

OUR ASTRONOMICAL COLUMN

THE TRANSIT OF MERCURY.—Unfavourable weather appears to have very generally interfered with observations of the first contacts in the transit of May 6, in this country, and in France a similar adverse state of atmospheric conditions also prevailed. At Antwerp, Christiania, Göttingen, Josephstadt (Vienna), Kiel, and San Fernando (Cadiz), the contacts were observed and the results have been mostly published in the *Astronomische Nachrichten*. In two cases only is there any distinction made between what has been called geometrical contact, when Mercury appears perfectly round and his outer limb in coincidence with the sun's limb, and the instant when a fine filament of light is perceptible (or a connecting ligament is broken) which more correctly distinguishes the true internal contact. Thus at Kiel the time was noted when the planet appeared round and when the narrow luminous thread (*deutlicher Lichtfaden*) appeared. But the most complete observations of the first contacts hitherto printed are those made at the Observatory of San Fernando, near Cadiz, which are detailed in a circular issued on May 8, by Señor Cecilio Pujazon, the director of the establishment. Amongst the observers were Señores Garrido and La Flor, who had also experience in the case of the transit in November, 1868, at the same observatory, and with the same or very similar instruments, achromatics by Troughton and Simms of 80 mm. aperture. Three of the observers distinguish between what is termed "first internal contact" and separation of the limbs (*desprendimiento de los limbos*), the mean interval noted between the two phases being 18 seconds.

At Palermo the contacts were noted both with the spectroscope and on the ordinary telescopic method. Prof. Tacchini communicated the particulars to the Paris Academy of Sciences on May 20, at the same time stating that he had been informed of the ill-success attending the observation of the transit at Naples, Florence, Venice, Gallarate (Baron Dembowski's observatory), Genoa, and Modena, on account of unfavourable skies.

In the United States the phenomenon appears to have excited a very unusual degree of interest, occasioned, no doubt, by the instructions for observing it widely-circulated by the authorities of the Naval Observatory,

Washington, and the presence in the country of a special expedition composed of French astronomers. Judging from the accounts published in the New York papers on May 7, observations were more or less successful in many astronomical institutions, both the first and last contacts being generally well observed, and numerous photographs obtained during the passage of the planet across the sun's disc. At Ogden, Utah, where the French astronomers were located, the clouds prevented more than imperfect observations of the first contacts; but those at egress were satisfactory. Up to one o'clock only three photographs were obtained, but subsequently as many as seventy-five were secured, and the results, as a whole, were considered satisfactory. At the observatory of Dr. Draper, Hastings, on the Hudson, a number of observers, including Prof. Holden, of Washington, availed themselves of the admirable instrumental resources, and the weather being for the most part advantageous, very good results attended their efforts: of eighteen negatives taken by Dr. Draper several were particularly perfect. In addition to observations at the U.S. Naval Observatory Prof. Newcomb and assistants made satisfactory ones at the office of the *American Ephemeris* in Washington, noting the first internal contact at 10h. 7m. 43s. A.M., according to the *New York Times*, and the second internal contact at 5h. 53m. 50s. P.M.

The following differences between the calculated and observed times of first internal contact have been obtained by comparison with Leverrier's elements, with Newcomb's value of the solar parallax; the Greenwich mean time for the centre of the earth resulting from a calculation of somewhat greater refinement than that previously introduced in this column being 3h. 16m. 12·5s.

Place of Observation.	Observed G.M.T. reduced to earth's centre.	Error of Calculation.	
	h. m. s.	s.	
Antwerp	3 15 46·0	+ 26·5	Two observers.
Christiania	— 41·2	+ 31·3	"Apparent internal contact."
,,	— 52·9	+ 19·6	"True internal contact."
Göttingen	— 34·8	+ 37·7	Prof. Klinkerfues.
,,	— 47·7	+ 24·8	Boeddicker and Heidorn.
Josephstadt	— 48·5	+ 24·0	Three observers.
Kiel	— 38·6	+ 33·9	Planet round.
,,	— 53·3	+ 19·2	"Deutlicher Lichtfaden."
Palermo	— 55·9	+ 16·6	Spectroscope.
,,	— 46·1	+ 26·4	Ordinary telescopic method.
San Fernando	— 49·1	+ 23·4	Geometrical contact.
,,	3 16 11·7	+ 0·8	Separation of limbs.
Washington	3 15 58·4	+ 14·1	Prof. Newcomb and assistants.

The Greenwich mean time of second internal contact similarly calculated is 10h. 43m. 57·3s., which, compared with Prof. Newcomb's observations at Washington, shows a difference of + 19·6s. Other observations of the second internal contact given in the New York journals are either provisionally reduced or apparently affected by typographical errors or errors of transmission.

THE ZODIACAL LIGHT AND SUN-SPOT FREQUENCY.—In a letter addressed to Gruithuisen in February, 1839, published by the latter in his *Astronomisches Jahrbuch* for 1840, Olbers remarks, "My grandson, Wilhelm Focke, Doctor of Law, who with attachment and zeal often contemplates and scrutinises the starry heavens, asserts that the zodiacal light has been observed in January and February with quite exceptional brightness;" which, Gruithuisen observes in a note, is "a new confirmation of Cassini's observation that the zodiacal light is much more brilliant when numerous and large sun-spots are present, and diminishes in brightness when the spots are few. My observations show that during January and February the sun has exhibited unusually large and numerous spots," and he adds, "viel Licht und fast immer eine grosse negative Refraction." This refers to Cassini's concluding statement in his memoir entitled "Découverte de la lumière céleste qui parait dans le Zodiaque." "It is a remarkable circumstance that since the end of the year 1688, when this light began to grow fainter, spots have no longer appeared in the sun, while in the preceding years they were very frequent, which seems to support in some manner the conjecture that this light may arise from the same emanations as the spots and *faculæ* of the sun." In a previous part of the memoir Cassini, endeavouring to assign a possible cause for the appearance of the zodiacal light, remarks that the observations of that century had made known that the sun is not only the source of light, but also of "une matière propre à terminer, à détourner, et à réflechir ses rayons;" and that "cette matière ne coule pas toujours de la même manière, mais qu'elle a des vicissitudes sans règle, selon lesquelles nous voyons en certain temps dans son disque des facules, qui sont plus claires que le reste de la surface, et des taches obscures qui ne sont point pénétrées par sa lumière." And he goes on to say that if the matter which is the subject of this light is of the same nature as that which forms the *faculæ* and spots on the sun, it should be liable to the same changes and irregularities. However inadequate or incorrect is the explanation of the spots and faculæ given by Cassini, his conjecture that the brightness of the zodiacal light varies with the number and magnitude of the solar spots is worthy of note, though we do not remember to have seen any allusion to it in our popular astronomical treatises.

THE INTERNATIONAL GEOLOGICAL CONGRESS

THE time of the opening of this Congress in Paris has been finally fixed by the local committee for the 29th August, and the Congress will remain in session about two weeks. Further details as to organization and place of meeting will soon be made public. Meanwhile, it is announced that from the 20th August to the 15th September, the library and reading-rooms of the Geological Society of France, No. 7, rue des Grands-Augustins, Paris, will be at the service of members of the Congress. As before, it is requested that all those who desire to take part therein will make it known to the general secretary, Dr. Ed. Jannetaz, at the above address, where, also, the subscription of twelve francs, required for each member, may be sent to Dr. Bioche, treasurer. Ladies are admitted to the Congress.

The local committee add to the above announcements:—There is reason to believe that the numerous collections of geology and palæontology, minerals, rocks, fossils, maps, sections, plans, models in relief, &c., to be found in the *Exposition Universelle*, will realise the expectations expressed in the circular of the International Committee, of an International Geological Exhibition. All exhibitors of such collections are requested to send, as above, such lists as will enable the secretary-general, Dr. Jannetaz, to prepare a special catalogue of them for the use of the Congress.

T. STERRY HUNT,
Secretary of the International Committee

A KINEMATICAL THEOREM

TAKE a plane, and, for clearness of idea, consider it as fixed horizontally. On this fixed plane lay another, and throughout the subsequent movement let the surfaces of the two planes always remain in contact. Now let the upper plane, starting from any position, be moved about in any manner whatever, making any number (N) of rotations, the points on it describing curves of any desired degree of complexity on the lower plane; and let it finally settle down again into its initial position, the curves described by the points on it being, in consequence, closed curves. Take the upper plane, and let us investigate the position on it of those points which have described curves of any given area (A) on the fixed plane.

However complex the curves described by them may be, the points will be found to form a circle on the upper plane; and if we give to A different values, the corresponding circles will be found to be all concentric. Further, if we call the circle corresponding to the value $A = 0$ the *zero-circle*, the area of the curves described by the points on any other circle of the system equals N times the ring inclosed between that circle and the zero-circle. It is remarkable that such a singular point as the centre of the circles should exist.

In the special case in which $N=0$, *i.e.*, where there has been only an oscillatory movement of the upper plane and no complete rotation, the system of concentric circles is replaced by a system of parallel straight lines, the area of the curves described by the points on any straight line of the system being proportional to the distance of that line from the zero-line.

It should, perhaps, be pointed out that the area of a figure 8 is zero, as the two halves are of opposite signs; also that when a point reciprocates on a curve the area inclosed by it in its path is zero. For example: if we take the interesting case of a circle rolling inside another of twice its diameter, every point on its circumference reciprocates on a straight line, and consequently the circumference is the zero-circle.

This theorem was suggested to me by reading a paper by Mr. C. Leudesdorf in the *Messenger of Mathematics*, where I have already enunciated it. It seems, however, to be one which, from its somewhat startling simplicity, may interest a larger class of readers than a purely mathematical one.

The proof is simple. Let P, P' be two points on the moving plane, and let A, A' be the areas described by them. Let $PP' = r$, and let the total movement of P' perpendicular to $PP' = n$.

$$\text{Then } A - A' = nr + N\pi r^2.$$

If we take P' as origin and the position of $P'P$ in which n is a maximum and equal to n' as initial line, $n' = n\cos\theta$. Thus $A - A' = n'\cos\theta \cdot r + N\pi r^2$, the equation to a family of concentric circles. Transforming to the centre, we have $$A = N\pi(r^2 - a^2),$$ where a is the radius of the zero-circle.

A. B. KEMPE

OLD MAPS OF AFRICA

MR. STANLEY, in the paper which he read at the Geographical Society on Monday, spoke of Africa being brought to light after an oblivion of 6,000 years. Notwithstanding the somewhat confused phraseology, Mr. Stanley's meaning is clear enough: Central Africa, with its great lakes and rivers, is now known, he means to say, for the first time. But recent investigation seems to show that the oblivion of Africa must be counted by hundreds and not thousands of years; that, in fact, it is only within two or three centuries that a knowledge of Central Africa has been allowed to lapse. A more rigorous search may show that between the fourteenth and the seventeenth centuries the great features that have been placed on modern maps within the past few years were discovered and recorded on the maps of the time.

We have recently referred, on more than one occasion, to two very curious globes that have been brought to light, one in the National Library in Paris, and the other in the Library of Lyons. On the Lyons globe, the date of which is 1701, the Congo is made to issue from a great lake, and wind its way westwards to the Atlantic, in a direction to some extent coincident with that recently discovered by Mr. Stanley. As a sort of preparation for the work of the great traveller, so soon to be issued, some account of the *data* on which these maps may have been constructed, may not be uninteresting. Our information is based on an article in *La Nature*, and on a report by a commission of the Lyons Geographical Society, appointed to investigate the value and origin of the Lyons globe.

The discovery made at Lyons is, in reality, no surprise to those who know the history of geographical exploration. Not only in the seventeenth century, does the Zaire-Congo appear on most of the maps with the direction definitely assigned to it by Stanley, but nearly all old documents, from the fifteenth century—and the date should be noted—make the great river issue from a considerable mass of water far in the interior of the African continent.

Already, in the year 1500, the famous *mappemonde* of Juan de la Cosa, the pilot of Christopher Columbus, gives the same indications; the picturesque *mappemonde* known as that of Henry II., repeats them with some variations, as also the master-work of Mercator (1569), the founder of modern geography. All the old geographers, or nearly all, repeat the same data:—Forlani (1562), Castaldi (1564), Sanuto (1588), Hondius (1607), Nicolas Picart (1644), Blœu (1569), Sanson, &c. Therefore there need be no surprise to find on a globe of the eighteenth century information which for more than 200 years previously had been registered on the map of Africa.

Whence, however, came this knowledge which our fathers had of certain regions in Central and Equatorial Africa? The reply is simple: from the Portuguese, who, since the fifteenth century, undertook not only extensive maritime voyages, but several times crossed Africa from west to east and from east to west. It is even very possible that they discovered the sources of the Nile, the great equatorial lakes; thus, in the midst of the simplicity and incoherence of their tracings we find, in their old parchments, the great lines of African geography almost as science now represents them. Most of these Portuguese, with the exception of some missionaries, were but poorly educated; they travelled much oftener as traders than as experienced explorers; nevertheless, we have almost the certainty that before the year 1500 they had furnished very precise information on the centre of Africa. In nearly all these maps, and in that of Lyons, the Congo flows in a nearly straight line from Lake Zaire or Zembre to the Atlantic; it bends only a very little to the north, and does not pass the equator, as we now know it does.

As a sort of exception, there has been found among the riches of the National Library at Paris, a Spanish globe of copper (without date, but probably between 1530 and 1540), which is not content with presenting the same data, but which reproduces, with wonderful closeness, the course of the Congo as discovered by Stanley. The river issues from a lake, flows towards the north, describes a large curve well to the north of the equator, then turns west-south-west to the Atlantic. This is indeed a summary of the last journey of the intrepid American correspondent. Fig. 1 gives a perfectly accurate idea of a portion of this valuable globe.

From all this it must not be concluded that Stanley has discovered nothing new. These discoveries of the ancient travellers, if genuine discoveries they were, seem to have ben forgotten as soon as they were recorded; and although the maps referred to above have been known for generations, no one ever seems to have taken them as trustworthy guides to the lines of African exploration. Indeed, it is only now that Stanley has made a discovery never to be forgotten that these old maps have come to have a real interest, for we suspect that till now geographers regarded the tracings as having their basis in the cartographers' imaginations. The glory of being really the first discoverers of the two Nyanzas, Nyassa, Tanganyika, Bangweolo, and the course of the Congo cannot be taken away from Speke and Baker and Burton and Livingstone and Stanley; or if so it must be by some ancient Arab or possibly Egyptian, many, many centuries ago, for there can be no doubt that long before Europe

awakened to modern geographical enterprise, these great features of Central Africa were known; Herodotus had an inkling of them, and Ptolemy all but located the central lakes. These modern explorers deserve the glory of first discoverers as much as Columbus deserves that of discoverer of America.

Without then detracting from the originality of the work of modern explorers, it is evident that from the fifteenth century onwards some travellers whose names have fallen into oblivion but who may have been companions of Diego Cam and Martin Behaim, ventured into the heart of Africa; followed certain arteries of communication and discovered the course of the Congo; geography kept possession of these discoveries for two centuries and gave them as articles of faith; besides, in the sixteenth and seventeenth centuries many Portuguese, Capuchins or simple traders, entered anew the interior, published the same facts, sometimes with corrections and additions.

Father Ricioli, a Jesuit and very intelligent man, furnished the Fathers Placide, of St. Amien, and Crespinien, two laborious monks, with documents to prepare the Lyons globe, in 1701. The actual constructor of the globe seems to have been the celebrated Lyons mechanician, Henri Marchand, in religion Père Gregoire, a Franciscan, with the help of the Venetian Contarini, a pupil of Nolin, belonging to the Flemish cartographic system. Evidently this was the last word of science. Fig. 3 is a much reduced copy of the facsimile made from the globe by M. Deloncle, the reporter of the commission we have alluded to.

How came it that just about the same time, about 1700, one of the princes of modern geography, Guillaume Delisle, was so badly inspired as to reconstruct an alto-

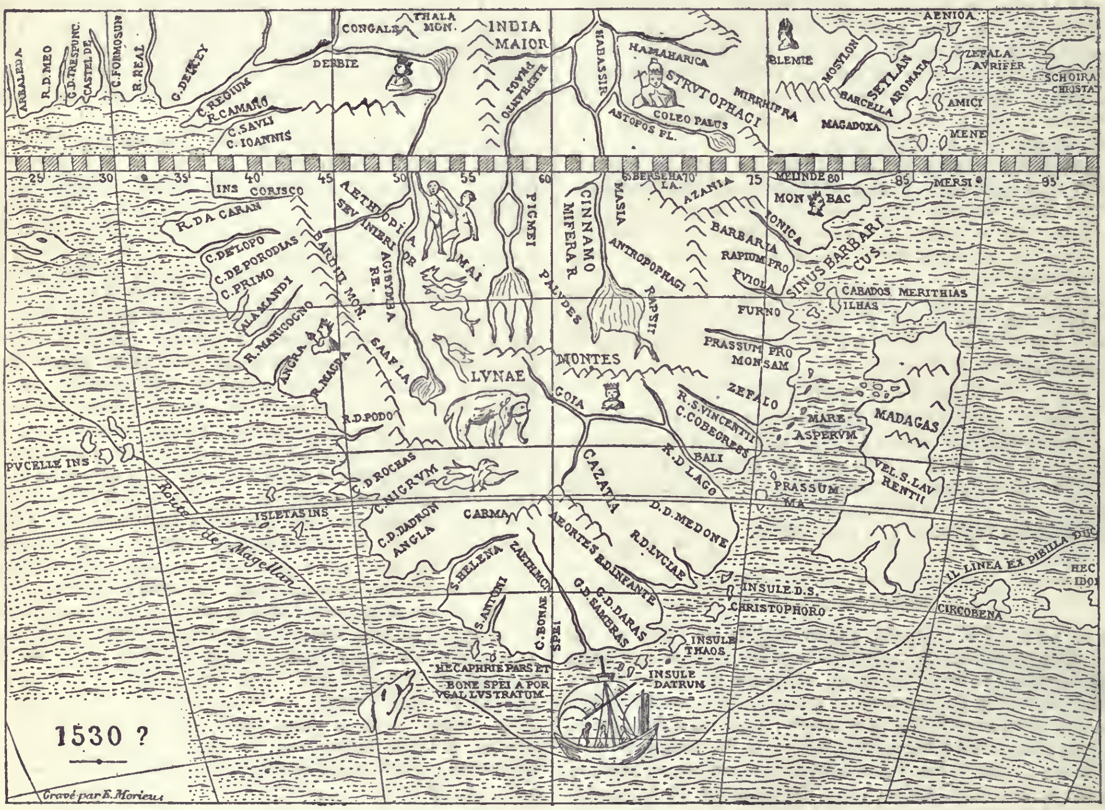


FIG. 1.—Portion of a Spanish Globe of 1530-40, found in the National Library, Paris.

gether New Africa in which he accumulated heresy on heresy? The Central lakes, the immense reservoirs of the Nile, disappear at one stroke of the pen; as to the Congo it is no longer connected with the lakes of the interior, although it is allowed to retain a little of its semicircular direction. The error accredited by a celebrated geographer like Delisle made way. The old map of Africa was demolished stone by stone, so to speak. In short the work was so well done that, after having piled nonsense upon nonsense, for the sake of peace, all was expunged; after having believed in tribes with dog-heads, placed a few anthropophagi everywhere, and confounded countries situated a thousand miles from each other, they ended by making a *tabula rasa* and leaving a white space where formerly were rightly placed the great lakes and sources of the Nile. Yet a few years and here was geography doubting, denying, and ridiculing the follies of our predecessors. The students of geography of the period of 1840-50 were too much on their guard to commit the colossal blunder at that period of making the Nile issue from the lakes to the south of the equator. "So far as concerns a part of Africa," to quote M. R. Cortambert, in the article in *La Nature*, "the past has been resuscitated: 'old things have become new.' That which was laughed at yesterday is taken seriously to-day. Then, my friends, these good ancestors of the fifteenth and sixteenth centuries, who counted among them Columbus, Gama, Magellan, and many other conquerors of the world, have not, perhaps, left altogether to their descendants of the nineteenth century the glory of inventing geography."

From the report of the Lyons Commission we learn that the following works were probably accessible to the Flemish map-makers, and later to the constructors of the

Lyons globe:—1. The Geography of Ptolemy; 2. The "Portuguese Asia" of De Barros (1552); 3. The "Description of the Congo," by Pigafetta, according to Lopez (1592); 4. The "Historical Description of Ethiopia" of Dom Francesco Alvarez" (1558); 5. The "Africa" of Leo Africanus (1556); 6. And the old maps and portulans.

Among these old maps and portulans, those which appear at this period to have had a certain influence are:—1. The Medicean Portulan (1351); 2. The Catalan Atlas (1375); 3. The Map of Mecia de Viledestes (1413); 4. The Map of Johannes Leardus (1448); 5. The *Mappemonde* of Fra Mauro; 6. The Ambrosean Map (1480); 7. The *Mappemonde* of Juan de la Cosa (1500); 8. The Map of Diego Ribera; 9. The Spanish *Mappemonde* of the National Library, Paris (Fig. 3) (1540); 10. The Maps of Ramusio, of Pigafetta, and of Hugues Linschoten.

In the detailed report which the Lyons Commission will communicate to the Society will be shown to what extent each of the above documents contributed to the establishment of the Flemish maps, on which probably the Lyons globe was more immediately based. The same Report will contain an investigation into the travels known or unpublished which, from the tenth century, have contributed to the progress of the African geography of the Middle Ages and of the Renaissance. This investigation will include the following:—1. The Arab voyages and Compendia; 2. The voyages of the mendicant Spanish friar of the fourteenth century; 3. The expedition of eight dominicans of Montpellier to the sources of the Nile (1317–1350), unpublished; 4. The travels of

FIG. 2. FIG. 3.

FIG. 2.—Map of Stanley's Recent Journey Across Africa. FIG. 3.—Portion of a Globe of 1701, belonging to the Lyons Library, from a copy by M. Deloncle.

the brothers Vivaldi, thirteenth century; 5. The expedition of the Catalan Ferrer in 1346, unpublished; 6. The voyages of Diego Cam; 7. The itineraries of the early *pombeiros*; 8. The "Eastern Ethiopia" of Joan dos Santos; 9. The travels of Barbosa; 10. The exploration of the Dutch Jan van Herder, in the country of the Akkas, unpublished; 11. The Derrotero desde Lisbõa Al Cabo de Bueno Esperanza y India Oriental, anonymous and unpublished; 12. The description of the Congo, by Martinus Abarca de Boléa et Castro (1601), unpublished; 13. The "Universal Book of the Navigations of the World" (1590?), Spanish, unpublished; 14; The Travels of the Belgian Pierre Fardé from Algiers to the Congo (1686), unpublished; 15. The "Travels of Manoël Godinho" (1663); 16. The Letters of Father Mariano, the Jesuit, on Kaffraria, &c., &c.

The work undertaken by the Lyons Geographical Society is creditable to them in the highest degree and will result in a valuable addition being made to historical geography. Their work, as the Commission rightly maintain, is to some extent international, and deserves the countenance and assistance of geographers all the world over.

COSMIC METEOROLOGY[1]

II.

INFLUENCE of the Moon on the Earth's Magnetism.—There is a fact in connection with the moon's influence on our earth for which an explanation is necessary, and M. Faye has proposed for this end a hypothesis in advance. He had already pointed out Dr. Lloyd's investigation which showed that the diurnal magnetic variations could

[1] Continued from p. 128.

not be explained by the hypothesis that the sun acts as a magnet. But, it is said, "May the moon not acquire induced magnetism under the action of the earth, perpetually variable according to the relative position of the two bodies? If we consider the enormous magnetic power of the earth, that Gauss finds equal to that of 464 trillions[1] of magnets weighing a pound each, and if we remark besides that the distance of the moon to the earth does not exceed thirty times the length of this gigantic magnet, we may give an affirmative answer to the question proposed. But then the magnetism induced in the moon should in its turn exercise a small action upon the proper magnetism of the earth in the period of a lunar month. The observations alone can decide this provided they are of great precision."

M. Faye then cites the results obtained from the Toronto observations by Gen. Sir E. Sabine, that for the magnetic declination showing a range of 0′·64; and he adds, "All these effects are of double period; they show two maxima and two minima in the course of the lunar month of 29½ days, which proves that they are due to an induced or reflex action, not to a direct action of the moon herself." I shall put my remarks on this subject under three heads.

1. Is such a result possible for the moon's synodical revolution? Let us commence with full moon at the winter solstice; near this epoch the moon is in the plane perpendicular to the ecliptic passing through the earth's magnetic axis and the sun. The north pole of the terrestrial magnet is then presented to the moon in such a way as to produce the maximum of induction; when the moon is near her third quarter the two terrestrial magnetic poles will be equidistant from the moon and the inducing action will be a minimum; there will be a second maximum near new moon when the south pole is most presented to our satellite and a second minimum near the first quarter. If now we follow the earth in her revolution to the vernal equinox, we shall find all this changed. At full moon our satellite is then equidistant from the two terrestrial poles, and the inducing action is a minimum; it is a maximum, on the contrary, near the first and third quarters. The consequence will be that if any inducing action existed it would have the same value at all ages of the moon in the mean of observations made during a series of years, such as were employed by Sabine for the variations in question. Such a result, however, as has been imagined by M. Faye might be possible if, instead of the synodical, we employ the tropical revolution of the moon, which occupies nearly 27·3 days.

2. We may inquire, then, if the moon as a permanent or induced magnet can produce any magnetic variations appreciable by our instruments? In the first place, Mr. Stoney has shown that if the moon were as magnetic bulk for bulk as our earth, her whole action in deflecting a freely-suspended needle in our latitudes, could not exceed one-tenth of a second of arc (0″·1).[2] In order to consider the question of the variable magnetism induced in the moon by our earth, let us suppose her inductive capacity equal to that of cast-iron. From Barlow's experiments at Woolwich with iron balls I find that the magnetism induced in an iron ball of one foot diameter is about 2·0, in English units, which is nearly twice the magnetic force given by Gauss for the same volume of our earth. Barlow found the induced moments of different balls to vary as their volumes, and assuming that the induced magnetism varies inversely as the cube of the distance of the inducing and induced bodies, we find at the moon's distance (60 terrestrial radii) the induced magnetism at the maximum, under the most favourable condition, could not be more than $\frac{2}{60^3} = \frac{1}{108,000}$ of that supposed in the first case, that is when as magnetic as the earth. Her whole action on a magnetic needle here, then, due to the earth's induction, could not exceed one millionth of a second of arc. It is advantageous to get rid of hypotheses which are so completely insufficient, and we may put aside for the future any consideration of the moon's action by her own permanent magnetism, or by a variable magnetism induced in her by the earth.

3. M. Faye has also misunderstood the facts which he wished to explain. The results obtained by Sabine have reference to a variation which occurs in 24¾ *hours*, the lunar *day*, and not the lunar month of 29½ days. The laws of the lunar diurnal variations were obtained first by Kreil for the magnetic declination, and by myself for the magnetic force and inclination. This action of the moon is, however, so very different from what is generally supposed, and from what was concluded from the first investigation on the subject, that it is of the greatest importance, in relation to the whole question of cosmic meteorology, I should state some of the more marked facts which have been deduced from eleven years' hourly observations on the magnetic equator. I shall limit myself at present to the lunar actions on the *direction* of the horizontal magnetic needle.

The moon, in a lunar day of 24·7 hours, produces a variation in the earth's magnetism, such that the magnetic needle makes *two* complete and nearly equal oscillations from an easterly to a westerly position in the interval in question. This is the general *mean* law. We have seen, in considering the law of the solar diurnal variations that, near the magnetic equator, the law becomes reversed when the sun passes from the one hemisphere to the other, so that when the sun is north, the movement of the needle is like that in high north latitudes, and when south, like that in high south latitudes. If, then, the moon acts in the same way as the sun, we should expect a similar phenomenon for the lunar diurnal variation when the moon crosses the equator. This is not the fact. The law differs little for the position of the moon north and south of the equator.

There *is*, however, an inversion of the lunar diurnal oscillations; thus, in the months of December and January the north end of a magnetic needle is farthest *east* when the moon is on the upper and lower meridians, and farthest west near moon-rise and moon-set; whereas in the months of June and July the reverse is the case, the north end of the needle being farthest *west* when the moon is on the meridian (upper and lower) and farthest east when she is on the horizon. It followed from this, as for the solar diurnal law, that the oscillations should be in opposite directions at the same time in the higher latitudes of the two hemispheres, as has been found to be the case.

It is not then when the moon crosses the equator but near the times when the *sun* does so, that the moon's action is reversed.

The dependence of the lunar action on the position of the sun becomes more evident as the investigation becomes more detailed. When we determine the mean law for each month of the year, we find that the north end of the needle moves equally far east and equally far west at each of the two oscillations in the lunar day; this is not found to be the case for different positions of the moon relatively to the sun. Thus in the quarter lunations including full moon, in the months of December and January, the greatest *west-east-west* oscillation of the needle occurs when the moon is on the *lower* meridian; not when the moon, but when the *sun*, is shining on the place of the needle. The oscillation from moon-rise to moon-set, that is to say, while the moon is above the horizon, is little more than one-third of the oscillation for the half day when she is below the horizon; the two westerly extreme positions when the moon is on the horizon are nearly the same.

[1] M. Faye uses the word trillions, but the trillions are English, not French, the latter being a very different number.

[2] *Phil. Mag.*, vol. xxii. p 294.

Similar results are obtained for the other quarter lunations. In all cases that oscillation is the greatest of the two for which the sun is above the horizon, whether the moon be above it or not.

There are still some remarkable facts connected with this variation at the magnetic equator. Limiting our examination of them always to December and January, we find, if we determine the oscillations due to the moon for the day when she is in conjunction and for each of the six following days, that in the first three days of the seven the oscillation is *west-east-west* during *the day*, that is, from sunrise to sunset; and in the last three days it is *east-west-east*. In the middle day of the seven the lunar action is almost null; the oscillation of the needle is very small, as we might expect, since on that day the change at sunrise from a *west-east* to an *east-west* motion takes place. The lunar hours of the maximum and minimum extremes thus oscillate about two hours on each side of the mean, depending on the position of the moon at *sunrise*.

The action of the moon, then, is dependent on the sun's position relatively to the equator (or the earth's position in its orbit), and on the position of the moon relatively to sunrise and sunset. But there is no relation between the laws and amplitudes of the solar and lunar diurnal oscillations. In the months from which I have taken my illustrations, the solar diurnal variation is a single oscillation: that for the moon, however taken, for single days, for quarter or for whole lunations, is always double. Through the combination of all the varying modes in which this oscillation is produced from day to day, *the mean for a lunation* is a regular double oscillation. The amplitude of this mean oscillation is three times as great in January as in June or July; whereas the amplitude of the mean solar diurnal variation is a half greater in June or July than in January.

I shall add another fact, one of the greatest importance in connection with this subject. We have seen that the lunar diurnal variation changes in the relative *amplitudes* of the two oscillations from day to day; the consequence of this is that when the means for a whole lunation, or even a quarter lunation, are taken, the mean amplitude is much less than that which is shown by each day separately. Thus I have found that the range of the mean lunar diurnal oscillation for the lunation December 16, 1858, to January 15, 1859, at Trevandrum, was 1′·25, while the ranges of the mean oscillations for the quarter lunations varied from 1′·70 to 2′·70, these quarter lunations giving exactly the same laws as have been deduced from eleven years observations for the same lunar epochs.

In order to understand the value of these results we must compare them with the ranges of the solar diurnal oscillations for the same months; those for December, 1858, and January, 1859, were 2′·20 and 2′·24 respectively. And as on some days the lunar diurnal variation has amounted to nearly 5′·0 (which is equivalent to 12′ in England with the smaller directive force), it appears that the lunar action is sometimes greater than the solar action at the magnetic equator.

As long as the lunar diurnal action was considered to be of the minute character first discovered, it was always possible for the supporters of the heat thesis to suspect that some small unknown heat action was in question. Such an idea is no longer possible. The lunar is sometimes greater than the solar diurnal action; and the former is dependent for its magnitude on the light and heat vibrations *due to the sun* shining on the place of the magnetic needle.[1]

If the solar light and heat vibrations can increase the magnetic action, there can be no difficulty in believing that these vibrations may in their turn suffer some modification of intensity. It would be difficult to measure small variations of the sun's light with sufficient accuracy as yet, though Mr. Willoughby Smith has suggested a selenium photometer for this end; we can, however, measure the variations of temperature, and the fact that the direct heating action of the moon is inappreciable is no longer sufficient to disprove the results of Mädler, Kreil, Park Harrison, and Balfour Stewart. We have in fact a mode of lunar action with which M. Faye was unacquainted and could not take into account. The whole basis of his argument is therefore destroyed.

The view now given opens up a wide field of inquiry, and cosmic meteorology appears under another aspect. I hope to be able at another time to present other facts which seem to relate to magnetical and meteorological phenomena.

JOHN ALLAN BROUN

[1] Mr. Willoughby Smith's experiments show that the light vibrations of the ether in selenium diminish in a very marked manner the electrical resistance of the crystal; and it does not seem improbable that the increase of the lunar magnetic oscillation in sunlight may be due to some similar action.

THE NUTRITION OF DROSERA ROTUNDIFOLIA

DURING the summer of 1877 I began an experiment, the results of which were given in a paper read before the Linnean Society, January 17, 1878. A number of Drosera Plants were freely supplied with meat, while another set were kept without animal food. At the end of the season the two sets were compared in various ways with the object of deciding whether or not carnivorous plants profit by an animal diet. In the abstract of my paper published in NATURE (vol. xvii. p. 222), it may be seen how numerous were the advantages gained by the fed plants.

The further results of the experiment are not without interest.

The plants on which I worked were cultivated in six soup-plates, and after all the flower stems had been cut, the plants in three of the plates were removed from the moss in which they grew, and were counted and weighed. The plants in the other plates were left undisturbed with the object of comparing the new plants which should spring up from the winter buds of the two sets in the following year. They were removed to the hothouse in the course of the autumn, in order that they might rapidly send up the next year's leaves. By the middle of January, 1878, it became quite clear that far more leaves were springing up from the winter buds of the plants which had been fed than from the others. Both sets of plants were now kept without food, and on April 3 they were removed from the plates, and carefully counted, dried, and weighed. The following numbers give the result of the examination:—

	Actual numbers and weights.		Proportion between two first columns.	
	Not fed.	Fed.	Not fed.	Fed.
Number of plants	89	105	100	118 0
Total weight	grams. ·206	grams. ·518	100	251·6
Average weight per plant	·0023	·0049	100	213·0

It will be seen that there is only comparatively a small difference (18 per cent.) between the number of not-fed and fed plants. Numerous minute offsets were found among both sets, and were all counted as separate plants. But, judging either by the total or average weights, no doubt can be entertained of the great advantage gained by the fed plants. It is a striking fact that, in spite of the far larger yield of flower stalks, seeds, &c., produced during the previous summer by the fed plants, they were nevertheless enabled to lay by a far greater store of reserve-material than their not-fed competitors.

It is a curious coincidence that while I was at work on Drosera, an almost identical research was being conducted in Germany. The experiment of Drs. Kellermann and von Raumer were described before the Phys. Med. Society of Erlangen in July, 1877, and the final results were communicated by Rees, of Erlangen, to the *Botanische Zeitung*, April 5, 1878.

The research was evidently conducted with extreme care, and it is very satisfactory to me to find that my results agree (speaking generally) with those of Kellermann and von Raumer. The plants used in their experiments were fed on aphides, and do not seem to have thriven quite so heartily as mine did on a meat diet. This appears from the following figures:—

	Kellermann and von Raumer's results.	Mine.
Number of flower stems ...	100 : 152	100 : 165
Number of capsules	100 : 174	100 : 194
Weight of seeds	100 : 205	100 : 380

In testing the relative powers of the fed and not fed plants in laying by reserve-material in the winter buds, the Erlangen observers adopted a more accurate method than mine, namely, that of weighing the winter buds, instead of waiting until the new leaves had grown. They found that the weights of winter buds for the fed and not fed plants were as 173 : 100. FRANCIS DARWIN

PHYSICAL SCIENCE FOR ARTISTS[1]

V.

THE simple and forcible language employed by Prof. Stokes in the extract I gave in my last paper, should have made it quite clear that in nine cases out of ten, when bodies reflect light to us, they have really absorbed a part of it in the process, and that to this absorption of light bodies by their colours are chiefly to be ascribed.

Those bodies which give back to us light in the middle of the spectrum—light, in other words, containing green and yellow—are those which are most liable to change with different intensities of light. I shall endeavour, if I have space, to return to this point in the sequel, but I feel that my first duty, now that the phenomena of absorption have, I trust, been clearly explained, is to pass on to the application of this knowledge to the various colours of the sky.

Having, then, this idea of absorption, a very important consideration comes in: the absorption of a substance generally increases with its thickness, and when we deal with those substances that for a given thickness absorb either the red or the blue, we often find that when the thickness is considerably increased the absorption spreads over the whole spectrum from the blue or the red end respectively. This can be shown graphically as follows:—

Blue Absorption.

V I B G Y O R } No absorption.
V I B G Y O R
V I B G Y O R
V I B G Y O R
V I B G Y O R
V I B G Y O R
V I B G Y O R

} Effect of increasing thicknesses.

[1] Continued from p. 126.

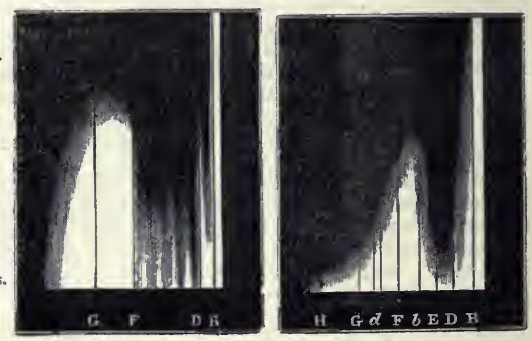
Here, then, we have an absorption beginning at the blue end and gradually closing everything except the red. I may remark *en passant* that we here have the physics of sunrise and sunset colours; similarly we might begin at the red end and then we should get

V I B G Y O **R**
V I B G Y **O R**
V I B G **Y O R**
V I B **G Y O R**, and so on.

These effects may be experimentally observed by either using different thicknesses of the absorbing materials or by putting them into a V-shaped vessel, and observing the change which takes place when the light passes through the greatest and least thicknesses of the absorbing material. It is of importance also for the artist to observe the effect of the residual light independently of the spectral phenomena. For instance: if we take a chlorine tube of such a length that it begins to cut off the

Greatest thickness.

Least thickness.



Potassium permanganate. Chromic chloride.

FIG. 1.—Showing phenomena of absorption produced by great and small thicknesses of the same substance in a wedge-shaped cell.

blue the residual light will be a delicate green; a tube twice the length will give us a colour in which the rich golden yellow predominates.

Although we have been compelled to leave out several steps in the argument, we are in a position now to approach the cause of the various colours of the sky.

Let us assume that our complex atmosphere—complex because it consists of a mixture of two pure gases and aqueous vapour—absorbs the light which passes through it, and that the absorptive effect depends upon the thickness of the atmosphere through which the light has to pass before it reaches the eye.

Now there are many grounds for supposing that the

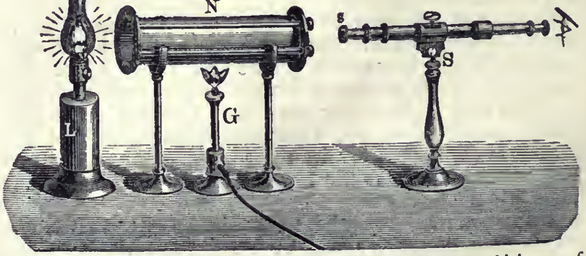


FIG. 2.—Arrangement for observing the absorption of a great thickness of gas or liquid. L, lamp; N, tube; S, spectroscope; G, Bunsen burner when used.

general absorptive effect both of the pure gases and of

the aqueous vapour is of the blue kind: that is to say, that the smallest thickness which has any visible effect will absorb in the blue. We also know that the absorptive effect of aqueous vapour is enormously greater than that of the pure gases.

I feel bound to show at once that this is no scientific abstraction, and it would be impossible to find two better examples to show exactly what I mean than those afforded by two of the pictures which I have chosen as texts—Mr. Vicat Cole's "Rosenlaui" and Mr. Peter Graham's "Wandering Shadows." Whether I am right or wrong about the molecular states of aqueous vapour, there is no doubt that the quantity of it varies considerably. The clouds in Mr. Graham's picture show us that the air is charged with it, for the simple reason that if it were not there would be no clouds to cast the shadows which he has so exquisitely caught. Look now at the dark hill in the distance; see how blue the air is between us and it—for it is true that it is the colour of the air, or rather of the aqueous vapour in the air, as Leonardo da Vinci first discovered, and not the colour of the hill which Mr. Graham here paints. We are in presence of aqueous vapour competent to be set in vibration by blue light, and because it vibrates in this way it appears blue.

What would have happened if the dark hill had not been there? If the stratum of aqueous vapour had had a background of bright sky, it would have absorbed the blue light of that sky. By virtue of the principles which I have stated, the sky would have appeared red in consequence of the abstraction of blue light. This, by the way.

Turn now to Mr. Vicat Cole's picture, and see the work of the vapour upon each receding buttress of rock on the left of the valley; the depth of atmosphere is rendered to perfection, but we do not get the blue that Mr. Graham gave us, for the reason that there is less aqueous vapour mixed up with the air.

Many an artist, I am sure, has noticed that at times there appears to be no atmosphere at all; all sense of distance is lost; buttresses such as those painted by Mr. Vicat Cole, although obviously, as may be gathered from the structure of the mountain, at different distances from the eye, seem yet to lie in the same vertical plane.

I saw this effect myself in its very strongest form last year at Cadenabbia. Looking eastwards from the hotel there, over the lake of Como, one sees Belaggio, the hills between Bellano and Lecco on the other side of what is called the Lecco leg of Como forming a magnificent background; these hills recede from the eye in a magnificent series of buttresses. Although some of these buttresses were three or four miles on the other side of Bellaggio, it was impossible to get rid of the feeling that lake, Bellaggio, background to the furthest buttress, was a painted canvas between us and the water. I called the attention of several friends to this wonderful sight; they saw it exactly as I did. The explanation is quite simple: although the permanent gases of the air were there, the aqueous vapour was not, at all events, in that form which by its action on light gives us what artists call atmosphere in a picture. To me this afforded the strongest possible proof of the statement I have already made that the absorption of the permanent gases of the air goes for nothing so far as art is concerned.

As I have already hinted, the molecular form of aqueous vapour with which we have most to do is one, the motions of which lie chiefly at the blue end of the spectrum; a small thickness of it cuts off the extreme blue, and as the thickness increases even the green may be dimmed by it.

In order to show how on such a point as this, art, representing an accurate study of natural phenomena, may help science, I will here give the result of some observations which my friend Dr. Schuster was good enough to make at my suggestion in the Himalayas and Tibet, with a view to test this very question.

Theory had led me to expect that with the enormous thickness of air available there, absorption at the red end of the spectrum by aqueous vapour would be seen as well as the absorption at the blue, which is so common with us. Seeing the sun a vivid green through the steam of the little paddle-boat on Windermere first led me to inquire into the possibility of aqueous vapour following the same law as that which I think we may now accept in the cases of the vapours of metals. As in these experiments with vapours, absorption of the red end alone was seen, as well as absorption at the blue end alone, the assumption that these two absorptions existed in aqueous vapour at once accounted for the green sun, which, I may remark *en passant*, I caught again last year through a thin veil of mist at the extreme summit of the pass of the Simplon.

Here, then, are Dr. Schuster's observations made at Simla when the rainy season had just begun:—

June 27, 8 A.M.—B (one of the Fraunhofer lines at the red end of the spectrum), beautifully shaded. Light visible in the blue as far as 4040; most likely further; but the telescope cannot be moved to greater deviation.

9 A.M.—Space beyond B closes up, while in the blue the spectrum is visible, as before.

11.15 A.M.—The red closed up still more; the blue as clear as before.

6.30 P.M.—Sun very near horizon; spectrum seen from C to G. (This means that both ends of the spectrum are now absorbed.)

Dr. Schuster further states that he was at the same time struck by the fact that the peculiar redness of the clouds in the evening, which we observe so often in our climates, was only rarely seen, and, when seen, that the colour was rather yellow than red. He adds, "On making this remark to a friend competent to judge, and who, through a long sojourn in Simla, was enabled to form an opinion, I heard that the redness of the sky at sunset was often beautifully seen at the end of and after the rainy season."

So much for the observations at Simla. I now pass on to some observations made in Upper Tibet, where there is no rainy season. I give them in Dr. Schuster's own words:—"The observations all point to the remarkable clearness in the blue. As I have said, the hygroscopic state of atmosphere, as measured by the wet and dry bulb or barometric pressure, cannot alone account for all the phenomena. I find, for instance, that the presence of vegetation affects the atmospheric absorption in a remarkable degree. In the Kyan Chu plain, for instance, the plateau on which I observed the mirage described in NATURE, vol. xiii. p. 67, objects at ten miles distance look as sharp and distinct as those half a mile off; it is, in fact, impossible to judge of distance. Crossing the Tagalung Pass (18,000 feet), we descended from that plain into the Valley of the Indus. As soon as we reached vegetation, at a distance of only two marches from the above-mentioned plain, and at a height still above 12,000 feet, the whole aspect of the country is a different one. Distant mountains now take the lofty blue colour which gives such peculiar charm to the landscape. In the evenings, especially, you cannot help knowing that there is something between your eye and a distant object, which affects its colour and distinctness, and through it you get a standard for judging distances. Without vegetation, even at a lower height, as, for instance, in the Valley of the Bagha (Lahoul), you seem to look through a vacuum. In the upper part of the Valley of the Indus, of which I am now speaking, I have not seen that clearness in the atmosphere which I have invariably seen in Switzerland at a height of 3,000 feet. The strong radiating power of the sun, which stands much more vertical in India, is evidently the cause of this, for it can only be organic matter floating in the atmosphere which can produce such a

striking result; that the absence of any rain or deposit of any kind must not be left out of account is clear. The air in the side valleys of Cashmere, although rich in vegetation, is particularly transparent. Strange enough, the principal valley of Cashmere, *i.e.*, the valley of the Jehlum, is generally hazy, although there is a good deal of rain. I have seen the planet Mars look almost white; Jupiter and the other stars at that time had a bluish tint."

I have been anxious to give these extracts not only because they form a valuable contribution to science, but because we see here the student of science doing what an artist is generally supposed to do, namely, interesting himself in the colouring of natural objects, and I cannot omit pointing this remark with the statement of my belief that when the artist attacks these also from the scientific point of view as well as the artistic one, his eye will lose nothing of its keenness, and his interest in the glory of nature will be nothing the less.

Let us consider, then, the action of those molecules which absorb the blue light.

Now since these molecules absorb blue light we know that they will reflect blue light, and, practically speaking, nothing else. Here, then, we have the cause for the blue colour of the sky.

Those who are familiar with the brilliant researches of Dr. Tyndall on the action of light upon vapours will recollect that he also has arrived at a somewhat similar conclusion from a different line of reasoning and a different method of experimentation.

To return one moment to oxygen and nitrogen, the gaseous constituents in our atmosphere, I must here remark that we have no evidence that the pure gases in our air change their molecular constitution; but we know that the aqueous vapour does to an enormous extent, and there is one state to which at present no reference has been made. There is a condition of aqueous vapour which is competent to absorb white light without giving rise to any coloured phenomena; this is the form of which mist and clouds are built up; why they are so dazzlingly white in the sunshine; why we have a dark grey day absolutely devoid of colour when a pall of cloud hangs over the whole sky. In addition to this we know also not only that condition to which I have already referred, which absorbs in the blue, but certainly of one, and in all probability two which absorb in the red. One of these absorptions indicates that the form of vapour which produces it is of the most delicate kind, while that which gives us the continuous absorption in the red end is perhaps the last stage reached before clouds are formed. If this be so, the very complex nature of the true cause of sky colour will be obvious. We have three molecular colour-giving states to contend with, and the action of these will depend largely upon the thickness of aqueous vapour traversed by the sunlight. A diagram will at once explain how the action of these different thicknesses is brought about.

In the diagram, Fig. 3, we have a section of a part of the earth and its atmosphere, supposing the latter to be

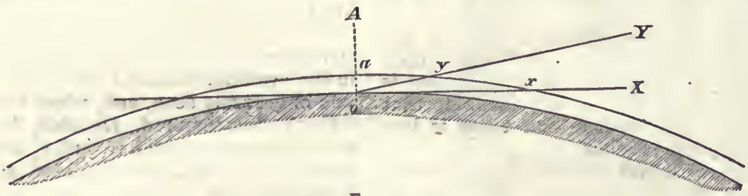


FIG. 3.

somewhere between forty and fifty miles thick. On the assumption that the aqueous vapour, which, as I have shown, is the effective absorber in the air, is equally distributed, let us see how the question of thickness of absorbing layers comes in. Take an observer at *o*, supposing him in the tropics, and that he sees the sun overhead at *A*; notice the distance *o a*, which represents the thickness of air traversed when the sun is overhead, and compare it with *o x*, when the sun is rising or setting as at *X*, and when, therefore, the greatest thickness is traversed, taking no account of refraction.

The whiteness of the sun at a high altitude and the redness of it when rising or setting is associated then with the fact that at these times the light traverses the least and greatest thickness of the atmosphere respectively; an intermediate height of the sun is represented at *Y*, and obviously the distance *o y* will vary for intermediate altitudes from *o a* to *o x*.

Now the thinner we make our atmosphere the greater will be the difference of the thicknesses *o a* and *o x*, and, as a matter of fact, the effective aqueous vapour lies very low down; so that the differences will be greater when we consider the aqueous vapour alone than when we consider the whole atmosphere. The thickness of the aqueous vapour, therefore, increasing from *a* to *x*, let us take the case of a perfectly clear sky at sunset; the white light reflected, as I have already shown, in conjunction with the blue, will find itself most absorbed in the line *o x*, least absorbed in the line *o a*. In the line *o x* we get everything absorbed but the red; we get, therefore, a red sky. A little higher everything is absorbed but the red, orange, and yellow; this will produce a rich golden colour above the red; higher still, the green and part of the blue is allowed to pass; in fact, only the extreme blue is absorbed; and, as I stated before, when I referred to the absorption of chlorine gas in a tube, the residual light will be green. Above the green we have the blue.

This is the order of the colours of *the sky;* the sun in consequence of its greater brilliancy can overcome this absorption until it has reached a very extreme limit, *sunset clouds lighted up by the sun, therefore, must put on the colour of the sun*, because the light which has reached our eye is red light, which has travelled to us *viâ* the cloud, *hence the green is limited to a band of sky*, between the gold and the blue, a green cloud is impossible, and it is on this ground that I ventured to criticise Mr. Ellis's picture, "The Last of the Wreck," 555. Mr. Ellis has painted green clouds; I am certain he never saw one in his life; for a similar reason I have objected to Mr. Oakes's picture, "The Dee Sands." Sky colour is begotten by a low sun.

I do not think that after what I have said it is necessary to point out how it comes that the blue clouds which Mr. Thornburn has chosen to paint are also impossible; a cloud can only be of a colour which is got from the sun directly or indirectly. Now a blue sun is possible, but clouds illuminated by a blue sun are impossible in a picture, because for the sun to be blue there must be nothing but a thick veil of mist.

I have drawn another diagram, which, although it looks rather complicated, may, I think, be rendered clear by a short description. The object I have had in view has been to show how the colours of the sky may be complicated after sunset. I believe in three pictures of sunrises or sunsets out of four, the phenomena presented have really been observed after sunset, in fact, in most pictures of sunset, the sun is a little too slow, we get sunset colour too soon.

In Fig. 4, oX representing the direction of sunset or "sunrise," my object is to show that a cloud high up, say at x, when the sun has set so long as to be at S^3 instead of at X can really receive light from the sun, and the distance xs''' added to ox will represent the total amount of atmospheric absorption undergone by the light. It is

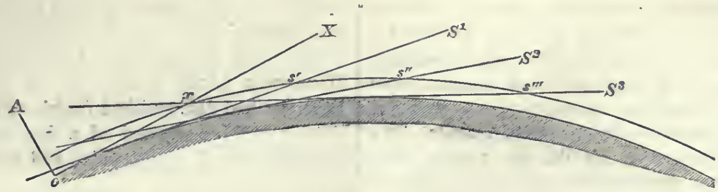


FIG. 4.

under these conditions, too, that, in consequence of the reduced illumination of the background, the sky puts on its most beautiful green, which I think is partly a physiological effect due to the molecular constitution of the retinas of our eyes. Similarly, by drawing a line from S^1 we can see how a cloud absolutely in the zenith of the observer at o may have its colour transformed by a considerable atmospheric absorption *after sunset*.

J. NORMAN LOCKYER

THE MICROPHONE IN SURGERY

ON Tuesday at 3 P.M., before a crowded audience of students and medical men, Sir Henry Thompson gave a demonstration in the anatomical theatre, University College, on the microphone as applicable in operations for stone.

In old days, the lecturer said, patients used to be sent away when they came to the doctor "because their case was not ripe.." The risk involved in the operation of cutting for stone was so serious that a surgeon seldom liked to undertake it except under compulsion, and when cutting had to be resorted to it was not much more difficult to remove a large than a small calculus. In the newer operation of stone-crushing it was better, of course, to have the stone to be crushed as small as possible, and it was essential to deal with the smallest fragments to which the operation reduced it. It was often said, indeed, in objection to lithotrity that to leave even the smallest fragment as a nucleus was to render further treatment necessary and, in time, inevitable. However that might be, it was clearly important to be able to deal with the smallest calculus in the bladder.

Before going further, Sir Henry Thompson emphatically stated that in his belief the present methods of lithotrity are quite sufficient in the hands of any surgeon of fair practice in the operation to enable him to deal successfully at all events with almost every case. He compared the use of the new instrument which he was going to describe to that of the endoscope for the urethra, which, however satisfactory on paper, had not been found important in practice, or, better perhaps, to that of the higher powers of the microscope, which were not necessary nor perhaps even advantageous in ordinary work, but which were a valuable resource in questions of unusual difficulty.

The apparatus consisted of the ordinary feeble battery with wires, connected with two telephones running to different parts of the room, and applied to the ears of the listeners. The ordinary Sound used in operations for crushing the stone was attached by a wire to the circuit of the battery. Near the handle a piece of carbon, such as is used by Prof. Hughes, was carefully balanced and attached by a delicate spring to the battery circuit. When the end of the sound strikes against the smallest piece of calculus the acoustic wave is transmitted along the steel of the instrument to the carbon, where it is transformed into electric vibrations, which are multiplied through the telephone, so that the noise becomes loud and unmistakable. But Sir Henry Thompson pointed out that in practice many things might interfere with the advantageous use of the instrument. The carbon arrangement on the sound must not be too delicate—not such, for instance, as could make us hear the walk of the fly like the tramp of an elephant—else the mere friction of the instrument on the walls of the bladder would produce a noise quite capable of being confounded with that caused by the presence of calculus. The battery must not be too strong, else mere accidental friction of the wires or the noises of the room would produce a distinct sound in the telephone. But when care was taken there would be no difficulty in detecting the noise. An ordinary calculus was put in a bladder in a basin of water, and the listeners could distinctly hear the different noises produced by the point of the sound rasping against the walls of the bladder or striking the stone. A sharp stroke of the former was sometimes not quite unlike the latter. But with the microphone properly adjusted, and the battery not too strong, it was not easy by trial to detect the presence of even a minute fragment of unremoved calculus in the bladder. The carbon needed only to be fitted to the probe, of course, to detect bullets or fragments of bone. But while it was quite possible for a skilful surgeon to make himself absolutely certain by means of the microphone of what he was previously only morally convinced of, Sir Henry Thompson did not appear to anticipate any very remarkable results, at least in ordinary practice, from the use of the instrument.

NOTES

WE are happy to state that a Commission appointed by the French Chamber of Deputies has reported favourably on the erection of a large observatory at Meudon, on the site of the Château which has been in ruins since the Franco-German war. The credit given is 690,000 francs, which will be paid in two instalments, 345,000 in 1878, and 345,000 in 1879. A large part of that sum, 390,000 francs, is destined for the construction of a large refractor, 250,000 francs are for the buildings, and 50,000 francs for the salary of M. Janssen, his assistants, and petty expenses during two years. The credit will be voted very likely *nemine obstante*.

WE learn, with much satisfaction, that the Swedish Diet has granted the necessary funds to the Meteorological Observatory at Upsala, so well known for the high excellence of its work, and that it will commence its new course as a separate institution, distinct from the Astronomical Observatory, on January 1, 1879.

ON Thursday, May 30, Dr. Gladstone, F.R.S., P.C.S. gave a *soirée* to the Fellows of the Chemical Society at Burlington House. Amongst the numerous objects of interest were the following:—A magnificent collection of immediate principles from the brain exhibited by Dr. Thudichum, who also demon-

strated the absorption spectrum of a new colouring matter, derived from eggshells, the bands being identical with those of cruentin, obtained from the blood. In the library were specimens of artificial corundum and emerald, made by Feil and Frémy; a large Cape diamond, exhibited by Prof. Tennant; a collection of precious stones from Hunt and Roskell, including a fine pink topaz, cat's eye, and a large crystal of garnet; some interesting apparatus of Faraday, amongst which was his rheostat; a collection of alkaloids from opium, aconite, &c., by Dr. Wright; a splendid case, by W. H. Perkin, illustrating the colouring matters from aniline, anthracene, &c.; a specimen of artificial alizarin and preparations of natural and artificial salicylic acids, the latter of which the exhibitors, Messrs. Hopkin and Williams, have succeeded in obtaining in crystals exactly resembling those of the natural product. Minerals containing liquid carbonic acid were shown by W. N. Hartley, who also demonstrated the effect of heat on the liquid inclosed in the cavities: crystals from Owens College, including a large, almost perfect octohedron of chrome alum; various interesting products, &c., were exhibited by Prof. Odling, Prof. Frankland, Dr. Russell, Dr. Armstrong, Dr. Witt, Dr. Schorlemmer, Dr. Hugo Müller, and M. M. P. Muir. In the room adjoining the lecture-room were some candles which had been acted upon by sea-water for 173 years, a large collection of meteoric stones, an interesting series of photographs of invisible fluorescent bodies, &c., exhibited by the President; a splendid photograph of the solar spectrum, shown by Mr. Rutherfurd; the spectrum of bismuth was shown by Mr. Browning; dichroic crystals of nickel and cobalt salts, by J. M. Thomson; photographs illustrating his recent researches in solar chemistry, by J. N. Lockyer; an enormous cut cairngorm, weighing 51 oz.; an opal cameo, and various minerals, by Bryce Wright, &c. In the lecture and preparation rooms were the microphone, exhibited by Prof. Hughes, which attracted considerable interest; Mr. W. De la Rue showed some phosphorescent tubes which, after a momentary exposure to some burning magnesium, flashed back all the colours of the spectrum; Byrne's pneumatic battery, and the copper zinc couple were shown in action; Messrs. Murray and Heath exhibited, under the microscope, some pretty crystals of gold, silver, &c.; Sir Joseph Whitworth and Dr. Siemens showed specimens of steel; in the same room Dr. Guthrie exhibited the formation of cryohydrates. Many other objects of interest were exhibited, but it would be impossible to enumerate all. The *soirée* was most successful, and althou gh the attendance was numerous, the arrangements were so good that at no time were the rooms inconveniently crowded.

THE publication of a work on the algæ of North-America, to consist of the plants themselves properly put up and labelled, was commenced a year ago by the three eminent American algologists, Dr. W. G. Farlow, of Cambridge, Prof. D. C. Eaton, of Yale, and Dr. C. L. Anderson, of California. A second fasciculus has lately appeared, and maintains the high character of the first.

THE death is announced of the venerable Baron von Ettingshausen, in his eighty-second year. We hope to give some notice of his life next week.

M. MASCART, the new director of the French Central Meteorological Bureau, took possession of the *Bulletin International* on June 1, without making any alteration in the nature of its contents or its periodicity. On the preceding day he visited for the first time the meteorological division of the observatory, and warned the officials to prepare for being removed from the establishment at an early date, their rooms being wanted for enlarging the astronomical service. Thus the principle of separation will be carried into effect very shortly. We are glad, however, that M. Mascart stated that the services of M. Leverrier's assistants had been quite appreciated by the Government. None of them will lose their present situations, and an increase of their present salaries is contemplated. The head of the Warning Service for Agriculture and Marine is M. Fron, a former pupil of the Normal School, and the sub-director, M. Moureau, a former teacher in the Mutual Schools, whom M. Leverrier had distinguished for his activity and ingenuity. The present staff is composed of a few subordinates acting merely as computers, an autographist, and a telegraphist.

WE notice the death, on May 14, in Dresden, of Prof. Wilhelm Friedrich Georg Behn, an able anatomist and zoologist. He was born in Kiel in 1808, and, after the completion of his scientific studies, entered as private docent in the Kiel University, where he received the chair of zoology in 1852. After the annexation of Schleswig-Holstein to Prussia he exchanged his professorship for one in Dresden. Here he was elected, in 1869, to the presidency of the Leopoldina-Carolina Akademie der Naturforscher, a position which he occupied up to his death. The academy, although the oldest in Germany—being founded in 1652—was then nearly on the point of dissolution. Prof. Behn's energetic efforts succeeded, however, in resuscitating it, and rendering it once more the centre of Saxon scientific life.

AT a general meeting of the Royal Irish Academy on Monday last week Cunningham gold medals were awarded to Dr. Aquila Smith for his inquiries into Irish numismatics; Dr. Allman, F.R.S., for his researches into the natural history of the Hydrozoa; and Dr. Casey for his important mathematical discoveries.

THE Dutch Society of Sciences held its 126th general meeting on May 18. It was at this meeting that the Huygens Medal was awarded to Prof. Newcomb, who, along with Sir George Airy, Dr. Auwers of Berlin, Prof. Du Bois Reymond of Berlin, M. V. Duruy of Paris, and Dr. C. F. W. Ludwig of Leipzig, were elected foreign members. For a paper on the question "What are the meteorological and magnetic phenomena which there are sufficient reasons for believing are connected with solar spots," Prof. Fritz of Zurich Polytechnic, was awarded a prize of 150 florins. The proposer of the question, Dr. Buys Ballot, was awarded a silver medal. A number of subject for prizes were proposed for competition in 1879 and 1880, the most important of which we hope to give next week.

IN the June number of the *American Journal of Science and Art* Prof. Marsh announces one of the most interesting discoveries yet made in the Palæontology of the Rocky Mountains, which have lately produced so many novelties. This is the right lower jaw of a small opossum, of the family Didelphydæ, for which he proposes the name *Dryolestes priscus*, from the Upper Jurassic series, in which no mammalian remains have previously been found in America.

THE first annual meeting of the Midland Union of Natural History Societies on May 27, at Birmingham, was, we are glad to say, a great success. After a luncheon given by the president, Mr. Tonks, a meeting was held at 3 P.M. in the Midland Institute, which was largely attended, as was also the brilliant *conversazione* in the evening in the town-hall. An excursion to Dudley on Tuesday, attended by about 400 members and their friends, brought to a close a pleasant and profitable meeting.

THE annual meeting of the Sanitary Institute was held on Friday last, Dr. B. W. Richardson, president, in the chair. It was shown that the Institute had already done good work, and exercised a decided influence in relation to sanitary matters. Dr. Richardson was re-elected president.

THE Tay Bridge, at Dundee, was opened on Friday, in presence of a large and distinguished company.

TWO Reports come to us from Scotland—one on the Glasgow Industrial Museum, and the other on the Dundee Free Library. From the former we are glad to see that, under the energetic curator, Mr. James Paton, F.L.S., the Glasgow Museum is gradually becoming worthy of the second city of the kingdom. Many important additions are being made to the well-arranged museum, with which, we see, have been incorporated the Corporation Galleries of Art. We trust the successors of "Bailie Nicol Jarvie" and his contemporary councillors will exercise a wise liberality and speedily raise their museum to the position it ought to occupy. From the Report of Mr. Maclauchlan we are pleased to see that scientific works are in considerable demand among the busy people of enterprising Dundee. The interesting museum, also, is gradually becoming possessed of that complete collection of the Arctic fauna which strangers naturally look for in the museum of the chief seat of the whaling trade.

IN the *Annalen der Hydrographie* we notice the account of a group of three islands discovered by Capt. Caller in 1877 on the north-west coast of Australia. These islands, which in their highest point do not rise more than thirty feet above the level of the sea, are covered with a thick deposit of guano, containing an unusual amount of ammonia and phosphates. On account of their nearness to the continent these valuable deposits will probably play an important part in the agricultural development of Australia.

ON October 4 last we gave an account of the post-mortem examination of a white whale (*Beluga*) that died after a few days' residence in a tank at the Westminster Aquarium. Mr. Farini then commissioned Zack Coup to obtain three more and bring them over from Labrador. They were packed each in a separate box lined with sea-weed, and four men were engaged to relieve one another in throwing water over the heads of the animals during the entire voyage. On Tuesday, May 27, they arrived at Liverpool, when one specimen was sent to Blackpool, one to Manchester, and one was brought under the personal care of Mr. Farini and Mr. Carrington, the naturalist of the Aquarium, to Westminster. This London specimen is 13ft. 6in. long, and arrived in apparently good condition. On Friday it was found requisite to "sling" it in order to remove an eel that had become entangled in its right flipper, when its quick sight in trying to avoid the sling was noticed with interest. The legs of a man sitting on the edge of the tank it carefully avoided, but it did not seem to mind the presence of those standing round. After the whale had been in the tank four days an indication of malaise and apparently of some accident having occurred attracted the careful attention of those who had charge of it. It was then ascertained the specimen was a female, and was for a while a subject of interest to physiologists especially.

A NEW improvement in the microscope is reported from Germany. Herr I. von Lenhossék has constructed an apparatus which permits no less than sixty microscopical preparations being observed in immediate succession, without the trouble of changing the slides and readjustment of the object-glass. Its construction is similar in principle to that of the well-known revolving stereoscopes, and the inventor has given the new apparatus the name of "polymicroscope."

UPON the occasion of unveiling the statue of Giordano Bruno, which will take place at Rome on February 19, 1879, a new edition of his works will be published. They are being reprinted at the expense of the Italian government.

THE Vienna Academy of Sciences held its annual public session on May 29, in the presence of representatives of the Court and Government. After the announcement of the various prizes and reports on the progress in the several sections of the Academy, Prof. Hann delivered an address on the "Problems of Modern Meteorology."

AN Ethnographical Exhibition, organised by the Anthropological Society of Paris in an annexe to the Trocadero, was opened on May 31. M. Teisserenc de Borg, Minister for Commerce and Agriculture was present on behalf of M. Bardoux, the Minister of Public Instruction, and declared the exhibition open. The addresses were delivered by MM. Quatrefages, Henri Martin, the president of the Society, and Dr. Broca. This exhibition is an extension of the Provisional Museum established for some months at the Palais de l'Industrie, in the Champs Elysées.

WE noticed at the time that M. Jules Simon, when French Minister for Public Instruction, had opened in the buildings of the Ministry, a provisional Pedagogical Museum, but a change having supervened in the Cabinet the scheme was dropped. It will, we learn now, be revived by M. Bardoux, who has asked special credit for that purpose from the Chamber of Deputies.

THE electric-light display in the Paris streets and thoroughfares is becoming one of the attractions of Paris. Eight electric lamps have been placed in the Place de l'Opéra, twenty-four others in the Opera Avenue, and eight more on the Place du Théâtre Français. Six lamps were lighted for the first time on June 1 on the part of the Palais Bourbon facing the Place de la Concorde. We should notice also the private illumination of the Grands Magasins du Louvre, about seventy lamps; Belle Jardinière, eight; Concert de l'Orangerie des Tuileries, twenty; and the Hippodrome, thirty-two. This last illumination, being in a closed building, cannot be viewed from the streets. All these illuminations are made by the Jablochkow candles. An electric lamp has been placed also on the top of the Trocadero Palace.

WE notice that the list of jurors for the Paris Exhibition has been gazetted.

THE last number of the *Journal* of the Society of Arts contains a valuable paper, recently read by Mr. J. M. Thomson, F.C.S., before the Society, on the Position of Chemistry in a System of Technical Education, as illustrated by some of its Applications. We are glad to see the Society turning its attention to a subject of such great importance.

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mr. J. Farmer; a Geoffroy's Cat (*Felis geoffroii*) from Uruguay, presented by Mr. Ronald Bridgett; a Brazilian Caracara (*Polyborus brasiliensis*) from South America, presented by Miss Amslie; a Tamandua Anteater (*Tamandua tetradactyla*) from South America, deposited; a White-eared Bulbul (*Pycnonotus leucotis*) from North-west India, received in exchange; four Temminck's Tragopans (*Ceriornis temminckii*), bred, a Yellow-footed Rock Kangaroo (*Petrogale xanthopus*), an Axis Deer (*Cervus axis*), born in the Gardens.

THE REDE LECTURE[1]

WHEN, about two years ago, news came from the other side of the Atlantic that a method had been invented of transmitting, by means of electricity, the articulate sounds of the human voice, so as to be heard hundreds of miles away from the speaker, those of us who had reason to believe that the report had some foundation in fact, began to exercise our imaginations in picturing some triumph of constructive skill—something as far surpassing Sir William Thomson's Siphon Recorder in delicacy and intricacy as that is beyond a common bell-pull. When

[1] Given at Cambridge by Prof. Clerk-Maxwell, F.R.S., May 24, 1878 Subject—"The Telephone."

at last this little instrument appeared, consisting, as it does, of parts, everyone of which is familiar to us, and capable of being put together by an amateur, the disappointment arising from its humble appearance was only partially relieved on finding that it was really able to talk.

But perhaps the telephone, though simple in respect of its material and construction, may involve some recondite physical principle, the study of which might worthily occupy an hour's time of an academic audience: I can only say that I have not yet met anyone acquainted with the first elements of electricity who has experienced the slightest difficulty in understanding the physical process involved in the action of the telephone. I may even go further, and say that I have never seen a printed article on the subject, even in the columns of a newspaper, which showed a sufficient amount of misapprehension to make it worth preserving—a proof that among scientific subjects the telephone possesses a very exceptional degree of lucidity.

However, if the telephone has something to say for itself, it would seem hardly necessary for me to take up your time with any tedious introduction. It is unfortunate, however, that up to the present time the telephone has kept all his more perfect utterances to be whispered into the privileged ear of a single listener. When he is older, he may get more accustomed to public speaking, but if we force him, in his present immature state, to exert his voice beyond what is good for him, it may sound rather too like the pot quarrelling with the kettle, and may call for the criticism with which Mr. Tennyson's Princess complimented the disguised Prince on his "Song of the Swallow:"—

> "Not for thee, she said,
> O Bulbul, any rose of Gulistan
> Shall burst her veil: marsh divers rather, maid,
> Shall croak thee sister, or the meadow crake
> Grate her harsh kindred in the grass."

Is it for this, then, that we are to forsake the luncheons and lawn tennis and all the engrossing studies of the May Term, and to assemble in this solemn hall, where the very air seems thick with the accumulation of unsolved problems, or else redolent of the graces of innumerable congregations?

It is not by concentrating our minds on any problem, however important, but rather by encouraging them to expand, that we shall best fulfil the intention of Sir Robert Rede when he founded this lecture.

It would be as useless as it would be tedious to try to explain the various parts of this small instrument to persons in every part of the Senate House. I shall, therefore, consider the telephone as a material symbol of the widely separated departments of human knowledge, the cultivation of which has led, by as many converging paths, to the invention of this instrument by Professor Graham Bell.

For whatever may be said about the importance of aiming at depth rather than width in our studies, and however strong the demand of the present age may be for specialists, there will always be work, not only for those who build up particular sciences and write monographs on them, but for those who open up such communications between the different groups of builders as will facilitate a healthy interaction between them. And in a university we are especially bound to recognise not only the unity of science itself, but the communion of the workers in science. We are too apt to suppose that we are congregated here merely to be within reach of certain appliances of study, such as museums and laboratories, libraries and lecturers, so that each of us may study what he prefers. I suppose that when the bees crowd round the flowers it is for the sake of the honey that they do so, never thinking that it is the dust which they are carrying from flower to flower which is to render possible a more splendid array of flowers, and a busier crowd of bees, in the years to come. We cannot, therefore, do better than improve the shining hour in helping forward the cross-fertilization of the sciences.

Before we go further, I wish to express my obligation to Mr. Garnett for the able assistance he has given me. He has not only collected the apparatus before you, but constructed some of it himself. But for him, I might have given you some second-hand information about telephones. He has made it possible for you to hear something yourselves. I have also to thank Mr. Gower, who has brought his telephone harp, and Mr. Middleton, who has contributed several instruments of his own invention.

We shall begin with the telephone in its most obvious aspect, as an instrument depending on certain physical principles.

The apparatus consists of two instruments, the transmitter and the receiver, doubly connected by a circuit capable of conducting electricity. The speaker talks to the transmitter at one end of the line, and at the other end of the line the listener puts his ear to the receiver, and hears what the speaker says.

The process in its two extreme stages is so exactly similar to the old-fashioned method of speaking and hearing that no preparatory practice is required on the part of either operator.

We must not, however, fall into the error of confounding the principle of the electric telephone with that of other contrivances for increasing the distance at which a conversation may be carried on. In all these the principle is the same as in the ordinary transmission of sound through the air. The different portions of matter which intervene between the speaker and the hearer take part, in succession, in a certain mechanical process. Each receives a certain motion from the portion behind it and communicates a precisely similar motion to the portion in front of it, in doing which it gives out all the energy it received, and is again reduced to rest.

The medium which takes part in this process may be the open air, or air confined in a long tube, or some other medium such as a brick wall, as when we hear what goes on in the next house, or a long wooden rod, or a metal wire, or even a stretched string. In all these it is by the actual motion of the successive portions of the medium that the message is transmitted.

In the electric telephone there is also a medium extending from the one instrument to the other. It is a copper wire, or rather two wires forming a closed circuit. But it is not by any motion of the copper that the message is transmitted. The copper remains at rest, but a variable electric current flows to and fro in the circuit.

It is this which distinguishes the electric telephone from the ordinary speaking tube, and from the transmission of vibrations along wooden rods by which Sir Charles Wheatstone used to cause musical instruments to sound in a mysterious manner without any visible performer.

On the other hand, we have to distinguish the principle of the articulating telephone from that of a great number of electrical contrivances which produce visible or audible signals at a distance. Most of these depend on the alternate transmission and interruption of an electric current. In some part of the circuit a piece of apparatus is introduced corresponding to this instrument which is called a key. Whenever two pieces of metal, called the contact pieces, touch each other, the current flows from the one to the other, and so round the circuit. Whenever the contact pieces are separated the current is interrupted, and the effects of this alternation of current and no current may be made to produce signals at any other part of the circuit.

In the Morse system of signalling, currents of longer and of shorter duration are called dashes and dots respectively, and by combinations of these the symbols of letters are formed. The rate at which these little currents succeed one another depends on the rate at which the operator can work the key, and may be increased by mechanical methods till the receiving clerk can no longer distinguish the symbols.

But the capability of the telegraph wire for transmitting signals is by no means exhausted; as the rapidity of the succession is increased, the ear ceases to distinguish them as separate signals, but begins to recognise the impression it receives as that of a musical tone, the pitch of which depends on the number of currents in a second.

Tuning forks driven by electricity were used by Helmholtz in his researches on the vowel sounds, and the periodically intermittent current which they furnish is recognised as a most valuable agent in physical and physiological research. The tuning forks are of the most massive construction, and the succession of currents goes on with the most inflexible regularity, so that whenever we have occasion to follow the march of a process which takes place in a short time, such as the vibration of a violin string or the twitch of a living muscle, the tuning fork becomes our appropriate timepiece.

Apparatus of this kind, however, the merit of which is its regularity, is quite incapable of adapting itself to the transmission of variable tones such as those of a melody.

The first successful attempt to transmit variable tones by electricity was made by Philip Reis, a teacher in a school at Friedrichsdorf, near Homburg. On October 21, 1861, Reis showed his instrument, which he called a telephone, to the Physical Society of Frankfort on the Main. He succeeded in transmitting melodies which were distinctly heard throughout the room.

The transmitter of Reis's telephone is essentially a make and

break key of so delicate a construction that the sound-waves in the air are able to work it.

The air vibrations set in motion a stretched membrane like a drumhead, with a piece of platinum fastened to it. This piece of platinum, when vibrating, strikes against another piece of platinum, and so completes the circuit every time contact is made.

At every point of the circuit there is thus a series of currents corresponding in number to the vibrations of the drumhead, and by causing these to pass through the coil of an electromagnet, the armature of the electromagnet is attracted every time the current passes, and if the armature is attached to a resonator of any kind, the succession of tugs will set it in vibration, and cause it to emit a sound, the pitch of which is the same as that of the note sung into the transmitter at the other end of the line.

[Mr. Gower here played the "March of the Men of Harlech" on the telephone harp placed in the Geological Museum. The instrument consists of a set of steel reeds worked by percussion, which make and break contact on the battery circuit, of which the primary wire of an induction coil forms part. The receivers are worked by the secondary current. There were four receivers, one of them Prof. Bell's original one, placed in different parts of the Senate-house.]

If the pitch of a sound were the only quality which we are able to distinguish, the problem of telephony would have received its complete solution in the instrument of Reis. But the human ear is so constructed, and we ourselves are so trained by continual practice, that we recognise distinctions in sound of a far more subtle character than that of pitch; and these finer distinctions have become so much more important for the purposes of human intercourse than the musical distinction of pitch, that many persons can detect the slightest variation in the pronunciation of a word who are comparatively indifferent to the variations of a melody.

Now, the telephone of Prof. Graham Bell is an articulating telephone, which can transmit not only melodies sung to it, but ordinary speech, and that so faithfully that we can often recognise the speaker by his voice as heard through the telephone. How is this effected? It is manifest that if by any means we can cause the tinned plate of the receiving instrument to vibrate in precisely the same manner as that of the transmitter, the impression on the ear will be exactly the same as if it had been placed at the back of the plate of the transmitter, and the words will be heard as if spoken at the other side of a tinned plate.

But this implies an exact correspondence, not only in the number of vibrations, but in the type of each vibration.

Now, if the electrical part of the process consisted merely of alternations between current and no current, the receiving instrument could never elicit from it the semblance of articulate speech. If the alternations were sufficiently regular, they would produce a sound of a recognisable pitch, which would be very rough music if the pitch were low, but might be less unendurable if the pitch were high; still, at the best, it would be like playing a violin with a saw instead of a bow.

What we want is not a sudden starting and stopping of the current, but a continuous rise and fall of the current, corresponding in every gradation and inflexion to the motion of the air agitated by the voice of the speaker.

Prof. Graham Bell has recounted the many unsuccessful attempts which he made to produce undulatory currents instead of mere intermittent ones. He had, of course, to give up altogether the method of making and breaking contact. Every method involving impact of any kind, whether between electric contact pieces or between the sounding parts of the instrument, introduces discontinuity of motion, and therefore precludes a faithful reproduction of speech.

In the ultimate form which the telephone in his hands assumed, the electric current is not merely regulated but actually generated by the aërial vibrations themselves.

The electric principle involved in Bell's telephone is that of the induction of electric currents discovered by Faraday in 1831. Faraday's own statement of this principle has been before the scientific world for nearly half a century, but has never been improved upon.

Consider first a conducting circuit, that is to say, a wire which after any number of convolutions returns into itself. Round such a circuit an electric current may flow, and will flow if there is an electromotive force to drive it.

Consider next a line of magnetic force, such a line as you see here made visible by sprinkling iron filings on a sheet of paraffin paper. This line, as Faraday also first showed, is a line returning into itself, or, as the mathematicians would say, it is a closed curve.

Now, if there are two closed curves in space, they must either embrace one another so as to be linked together, or they must not embrace each other.

If the line of force as well as the circuit were made of wire, and if it embraced the copper circuit, it would be impossible to unlink them without cutting one or other of the wires. But the line of force is more like one of Milton's spirits, which cannot

> "In their liquid texture mortal wound
> Receive, no more than can the fluid air."

Now, if the copper circuit or the lines of force move relatively to each other, then in general some of the lines of force which originally embraced the circuit will cease to embrace it, or else some of those which did not embrace it will become linked with it.

For every line of force which ceases to embrace the circuit there is a certain amount of positive electromotive force, which, if unopposed, will generate a current in the positive direction, and for every new line which embraces the circuit there is a negative electromotive force, causing a negative current.

In Bell's telephone the circuit forms a coil round a small core of soft iron fastened to the end of a steel magnet. Now lines of magnetic force pass more freely through iron than through any other substance. They will go out of their way in order to pass through iron instead of air. Hence a large proportion of the lines of force belonging to the magnet pass through the iron core, and, therefore, through the coil, even though there is no iron beyond the core, so that they have to complete their circuit through air.

But if another piece of soft iron is placed near the end of the core it will afford greater facilities for lines which have passed through the core to complete their circuit, and so the lines belonging to the magnet will crowd still closer together to take advantage of an easy passage through the core and the iron beyond it. If then the iron is moved nearer to the core, there will be an increase in the number of such lines, and, therefore, a negative current in the circuit. If it is moved away there will be a diminution in the number of lines, and a positive current in the circuit. This principle was employed by Page in the construction of one of the earliest magneto-electric machines, but it was reserved for Prof. Bell to discover that the vibrations of a tinned iron plate, set in motion by the voice, would produce such currents in the circuit as to set in motion a similar tinned plate at the other end of the line.

It will help us to appreciate the fertility of that germ of science which Faraday first detected and developed if we recollect that year after year he had employed the powerful batteries and magnets and delicate galvanometers of the Royal Institution to obtain evidence of what he all along hoped to discover—the production of a current in one circuit by a current in another, but all without success, till at last he detected the induced current as a transient phenomenon, to be observed only at the instant of making or breaking the primary circuit.

In less than half a century, and by the aid of no second Faraday, but in the course of the ordinary growth of scientific principles, this germ, so barely caught by Faraday, has developed on the one hand into the powerful currents which maintain the illumination of the lighthouses on our coasts; and on the other, into these currents of the telephone which produce an audible effect, though the engine that drives them is itself driven by the tremors of a child's voice.

Prof. Tait has recently measured the absolute strength of these telephone currents. He produced them by means of a tuning fork vibrating in front of the coil of the transmitter. Before the transmitted note ceased to be audible at the other end of the line he measured by means of a microscope the amplitude of the vibrations of the fork.

He then placed a very delicate galvanometer in the circuit and found what deflexion was produced by a measured motion of the fork.

Finally he measured the deflection of the galvanometer produced by a small electromotive force of known magnitude. He thus found that the telephone currents produced an audible effect when reversed 500 times a second, though their strength was no greater than what a Grove's cell would send through a million megohms, about a thousand million times less than the currents used in ordinary telegraphic work.

One great beauty of Prof. Bell's invention is that the instruments at the two ends of the line are precisely alike. When the tin plate of the transmitter approaches the core of its bobbin it produces a current in the circuit, which has also to circulate round the bobbin of the receiver, and thus the core of the receiver is rendered more or less magnetic, and attracts its tin plate with greater or smaller force. Thus the tin plate of the receiver reproduces on a smaller scale, but with perfect fidelity, every motion of the tin plate of the transmitter.

This perfect symmetry of the whole apparatus—the wire in the middle, the two telephones at the end of the wire, and the two gossips at the ends of the telephones—may be very fascinating to a mere mathematician, but it would not satisfy an evolutionist of the Spencerian type, who would consider anything with both ends alike to be an organism of a very low type, which must have its functions differentiated before any satisfactory integration can take place.

Accordingly, many attempts have been made, by differentiating the function of the transmitter from that of the receiver, to overcome the principal limitation to the power of the telephone. As long as the human voice is the sole motive power of the apparatus it is manifest that what is heard at one end must be fainter than what is spoken at the other. But if the vibration set up by the voice is used no longer as the source of energy, but merely as a means of modulating the strength of a current produced by a voltaic battery, then there will be no necessary limitation of the intensity of the resulting sound, so that what is whispered to the transmitter may be proclaimed *ore rotundo* by the receiver.

A result of this kind has already been obtained by Mr. Edison by means of a transmitter in which the sound vibrations produce a varying pressure on a piece of carbon, which forms part of the electric circuit. The greater the pressure, the smaller is the resistance due to the insertion of the carbon, and therefore the greater is the current in the circuit.

I have not yet seen Mr. Edison's transmitter, but the microphone of Prof. Hughes is an application of carbon and other substances to the construction of a transmitter, which modulates the intensity of a battery current in more or less complete accordance with the sound-vibrations it receives. The energy of the sound produced is no longer limited by that of the original sound. All that the original sound does is to draw supplies of energy from the battery, so that a very feeble sound may give rise to a considerable effect. Thus, when a fly walks over the table of the microphone the sound of his tramp may be heard miles off.

Indeed, the microphone seems to open up several new lines of research. We shall have London physicians performing stethoscopic auscultations on patients in all parts of the kingdom. The Entomological Society have recently been much interested by Mr. Wood-Mason's discovery of a stridulating apparatus in scorpions. Perhaps ere long a microphone, placed in a nest of tropical scorpions, may be connected up to a receiver in the apartments of the society, so as to give the members and their musical friends an opportunity of deciding whether the musical taste of the scorpion resembles that of the nightingale or that of the cat.

I have said that the telephone is an instance of the benefit to be derived from the cross-fertilization of the sciences. Now this is an operation which cannot be performed by merely collecting treatises on the different sciences, and binding them up into an encyclopædia. Science exists only in the mind, and the union of the sciences can take place only in a living person.

Now, Prof. Graham Bell, the inventor of the telephone, is not an electrician who has found out how to make a tin plate speak, but a speaker, who, to gain his private ends, has become an electrician. He is the son of a very remarkable man, Alexander Melville Bell, author of a book called "Visible Speech," and of other works relating to pronunciation. In fact, his whole life has been employed in teaching people to speak. He brought the art to such perfection that, though a Scotchman, he taught himself in six months to speak English, and I regret extremely that when I had the opportunity in Edinburgh I did not take lessons from him. Mr. Melville Bell has made a complete analysis and classification of all the sounds capable of being uttered by the human voice, from the Zulu clicks to coughing and sneezing; and he has embodied his results in a system of symbols, the elements of which are not taken from any existing alphabet, but are founded on the different configurations of the organs of speech.

The capacities of this new mode of representing speech have been put to the test by Mr. Alexander J. Ellis, author of "The Essentials of Phonetics," a gentleman who has studied the whole theory of speech acoustically, philologically, and historically. He describes the result in a letter to *The Reader* :—

"The mode of procedure was as follows :—Mr. Bell sent his two sons, who were to read the writing, out of the room—it is interesting to know that the elder, who read all the words in this case, had only had five weeks' instruction in the use of the alphabet—and I dictated slowly and distinctly the sounds which I wished to be written. They consisted of a few words in Latin, pronounced first as at Eton, then as in Italy, and then according to some theoretical notions of how the Latins might have uttered them. Then came some English provincialisms and affected pronunciations, the words 'how odd' being given in several distinct ways. Suddenly German provincialisms were introduced; then discriminations of sounds often confused. Some Arabic, some Cockney English, with an introduced Arabic guttural, some mispronounced Spanish, and a variety of shades of vowels and diphthongs.

"The result was perfectly satisfactory—that is, Mr. Bell wrote down my queer and purposely exaggerated pronunciations and mispronunciations, and delicate distinctions, in such a manner that his sons, not having heard them, so uttered them as to surprise me by the extremely correct echo of my own voice. . . . Accent, tone, drawl, brevity, indistinctness were all reproduced with surprising accuracy. Being on the watch, I could, as it were, trace the alphabet in the lips of the readers. I think, then, that Mr. Bell is justified in the somewhat bold title which he has assumed for his mode of writing—'Visible speech.' I only hope that for the advantage of linguists, such an alphabet may soon be made accessible, and that, for the intercourse of nations, it may be adopted generally, at least for extra-European nations, as for the Chinese dialect and the several extremely diverse Indian languages, where such an alphabet would rapidly become a great social and political engine."

The inventor of the telephone was thus prepared, by early training in the practical analysis of the elements of speech, to associate whatever scientific knowledge he might afterwards acquire with those elementary sensations and actions, which each of us must learn from himself, because they lie too deep within us to be described to others. This training was put to a very severe test when, at the request of the Boston Board of Education, Prof. Graham Bell conducted a series of experiments with his father's system in the Boston School for the Deaf and Dumb. I cannot conceive a nobler application of the scientific analysis of speech, than that by which it enables those to whom all sound is

> "expunged and rased
> And wisdom at one entrance quite shut out"

not only to speak themselves, but to read by sight what other people are saying. The successful result of the experiments at Boston is not only the most valuable testimonial to the father's system of visible speech, but an honour which the inventor of the telephone may well consider as the highest he has attained.

An independent method of research into the process of speech was employed by Wheatstone, Willis, and Kempelen, the aim of which was to imitate the sounds of the human voice by means of artificial apparatus. This apparatus was in some cases modelled so as to represent as nearly as possible the form as well as the functions of the organs of speech, but it was found that an equally good imitation of the vocal sounds could be obtained from apparatus the form of which had no resemblance to the natural organs.

Several isolated facts of considerable importance were established by this method, but the whole theory of speaking and hearing has been so profoundly modified by Helmholtz and Donders, that much of what was advanced before their time has come to possess only an historical interest.

Among all the recent steps in the progress of science, I know none of which the truly scientific or science-producing consequences are likely to be so influential as the rise of a school of physiologists, who investigate the conditions of our sensations by producing on the external senses impressions, the physical conditions of which can be measured with precision, and then recording the verdict of consciousness as to the similarity or difference of the resulting sensations.

Prof. Helmholtz, in his recent address as Rector of the University of Berlin, lays great stress on that personal interaction between living minds, which I have already spoken of as essen-

tial to the life of a University. "I appreciate," he says, "at its full value this last advantage, when, looking back, I recall my student days, and the impression made upon us by a man like Johannes Müller, the physiologist. When one finds himself in contact with a man of the first order, the entire scale of one's intellectual conceptions is modified for life; contact with such a man is perhaps the most interesting thing life may have to offer."

Now, the form in which Johannes Müller stated what we may regard as the germ which fertilized the physiology of the senses is this, that the difference in the sensations due to different senses does not depend upon the actions which excite them, but upon the various nervous arrangements which receive them.

To accept this statement out of a book, as a matter of dead faith, may not be difficult to an easy-going student; but when caught like a contagion, as Helmholtz caught it, from the lips of the living teacher, it has become the guiding principle of a life of research.

No man has done more than Helmholtz to open up paths of communication between isolated departments of human knowledge; and one of these, lying in a more attractive region than that of elementary psychology, might be explored under exceptionally favourable conditions, by some of the fresh minds now coming up to Cambridge.

Helmholtz, by a series of daring strides, has effected a passage for himself over that untrodden wild between acoustics and music—that Serbonian bog where whole armies of scientific musicians and musical men of science have sunk without filling it up.

We may not be able even yet to plant our feet in his tracks and follow him right across. That would require the seven league boots of the German colossus; but to help us in Cambridge we have the Board of Musical Studies, vindicating for music its ancient place in a liberal education. On the physical side we have Lord Rayleigh laying the foundation deep and strong in his "Theory of Sound." On the æsthetic side we have the University Musical Society doing the practical work, and in the space between, those conferences of Mr. Sedley Taylor, where the wail of the siren draws musician and mathematician together down into the depths of their sensational being, and where the gorgeous hues of the phoneidoscope are seen to seethe and twine and coil like the

"Dragon boughs and elvish emblemings"

on the gates of that city where

"an ye heard a music, like enow
They are building still, seeing the city is built
To music, therefore never built at all,
And therefore built for ever."

The special educational value of this combined study of music and acoustics is that more than almost any other study it involves a continual appeal to what we must observe for ourselves.

The facts are things which must be felt; they cannot be learned from any description of them.

All this has been said more than two hundred years ago by one of our own prophets—William Harvey, of Gonville and Caius College. "For whosoever they be that read authors, and do not by the aid of their own senses, abstract true representations of the things themselves (comprehended in the author's expressions) they do not resent true ideas, but deceitful idols and phantasms, by which they frame to themselves certain shadows and chimæras, and all their theory and contemplation (which they call science) represents nothing but waking men's dreams and sick men's phrensies."

Prof. Maxwell was assisted in his practical demonstrations by Mr. Garnett, of St. John's College.

SOCIETIES AND ACADEMIES

London

Physical Society, April 13.—Prof. R. B. Clifton, vice-president, in the chair.—The following candidates were elected Members of the Society:—W. Campbell, R. W. F. Harrison, Rev. T. N. Hutchinson, M.A., B. W. Richardson, M.B., F.R.S.—The Secretary read a paper by Messrs. J. Nixon and A. W. Heaviside, describing their experiments on the mechanical transmission of speech through wires or other substances, to which Mr. Preece had referred at a previous meeting of the Society. After describing a number of experiments in which metallic discs soldered on to the ends of the conducting wires were employed, they went on to enumerate the more successful experiments in which wooden discs were mainly employed. The first actual transmission of speech was effected by placing the belly of a violin against the receiving end of the wire, when every syllable spoken was distinctly audible. Very good results were obtained by employing mouth-and-ear pieces, formed as in a telephone, the disc being replaced by thin wooden discs, six inches in diameter, and a No. 4 wire was found to be most satisfactory. On suspending a length of this wire in such a manner that it had no rigid attachments, it was ascertained that 120 yards is the limit through which a conversation can be carried on.—Capt. Abney, F.R.S., described the method he adopted for photographing the least refrangible end of the spectrum. He pointed out that it is impossible, with the ordinary sensitive salts employed in the usual way, to photograph further than the Fraunhofer line E, though by a preliminary exposure to light of a Daguerrotype plate, Draper was able to photograph beyond the extreme limit of visibility in the red end of the spectrum. This method, however gave what is known as a reversed picture, the lights and shades being transposed, besides requiring a lengthened exposure. It enabled Becquerel to photograph the spectrum in its natural colours, and later St. Victor obtained coloured images of coloured cloths. The object of Capt. Abney had been to obtain unreversed pictures of this portion of the spectrum; in other words, to obtain a compound that would be similarly sensitive to the red and the blue components of white light. Such a compound he had at last obtained by what he termed *weighting* silver bromide with resin, and now he obtains it by causing the molecules of silver bromide to weight themselves. He showed an ordinary bromide of silver plate, and the colour of the transmitted light was of a ruddy tint, showing absorption of the blue rays; another film was shown containing weighted bromide of silver, which transmitted blue light and absorbed the red. Photographic plates prepared with the latter compound he showed were sensitive to the red and ultra-red waves of light, and he threw on the screen photographs of the spectrum from the line C to a wave-length of 10,000, the ultra-red showing remarkable groupings of lines. He further showed that by friction the blue film was changed to the red, and in that state was not sensitive to the lower part of the spectrum. These photographs were taken by means of a diffraction grating, and Capt. Abney demonstrated Fraunhofer's method of separating the various orders of spectra produced by it. He then explained that recently he had elucidated the reason of the reversal of Draper's pictures by the least refrangible end of the spectrum. He finds that it is accelerated by exposing the plates in weak oxidising solutions, such as those of hydroxyl, bichromate of potash, permanganate of potash, and nitric acid, or exposure to ozone. The red rays, in other words, seemed to oxidise the photographic image, and to render it incapable of development.—Mr. H. Bauermathen exhibited some paper models illustrative of the disposition of the planes of symmetry in crystals. These included octants of the sphere with inclosed cube and octahedron faces pointed into their corresponding hexakis-octohedral faces, a cubic skeleton built up from nine planes of symmetry with a removable outer shell, and a system of axial planes of an unsymmetrical mineral inclosing a solid nucleus contained between three parallel pairs of planes. They were constructed for the purpose of showing popularly the difference between planes of symmetry and other diametral planes by laying upon them a small mirror or plate of mica, when in the first case the inclosed nucleus gave a symmetrical image corresponding in position to the plane immediately behind the mirror, but in the second a broken image is produced.—Dr. Guthrie exhibited the arrangement of apparatus he had employed, in conjunction with his brother, to ascertain the effect of heat on the transpiration of gases. The main difficulty connected with the research was the securing of an absolutely constant pressure on the air operated upon. This was secured by inserting into the neck of the vessel which served as an air-chamber a tube turned up at its inner end and terminating externally by a small funnel; as the tube was kept constantly full of water, the funnel overflowing, a pressure represented by the difference between the heights of these levels was maintained. After passing through a series of drying tubes the air traversed the (U-shaped) capillary tube in a beaker containing water of known temperature, and was finally received in an inverted tube contained in an overflowing dish of water. Among other results it was found that the resistance of a tube is the same as that of its several portions,

and if t be the time occupied, T the absolute temperature, p_1 p_2 the pressures, and α and β constants, they find that—

$$t = \alpha T\left(T + \frac{\beta}{p_1 - p_2}\right).$$

Chemical Society, May 16.—Dr. Gladstone, president, in the chair.—The following papers were read :—On the detection and estimation of free mineral acids in various commercial products, by Peter Spence and A. Esilmann. The method is based on the fact that peracetate of iron even in dilute solutions has a distinct yellow colour, not perceptibly altered by acetic acid or solutions of persulphates, but instantly bleached by free sulphuric, hydrochloric, and nitric acids. The solution is made by dissolving ten parts of iron alum and eight parts of crystallised acetate of soda in 1,000 parts of 8 per cent. solution of acetic acid (25 per cent.).—The action of hypochlorites on urea, by H. G. H. Fenton. The author has found that when urea is acted on by a hypochlorite in the cold, in the presence of a caustic alkali, only half the nitrogen is evolved. From various experiments it was proved that the nitrogen remains behind as a cyanate.—On the behaviour of metallic solutions with filter paper and on the detection of cadmium, by T. Bayley. The author has investigated the action which takes place when drops of metallic solutions are placed on filter paper, the extent to which the solutions spread being tested by sulphuretted hydrogen. In some cases the solution seemed to concentrate itself in the middle, in others round the edge of the spot. Dilution, temperature, and the kind of filter paper used, have an important influence on this phenomenon. The salts of silver, lead, &c., when moderately concentrated, give a wide water ring containing no metal, while the salts of copper, nickel, cobalt, and especially cadmium, must be much more dilute to present the same appearance. This property of cadmium to spread itself over the whole drop is so marked that it affords an elegant means of detecting it in the presence of metals whose sulphides are black.—On essential oil of sage, by S. Siguira and M. M. P. Muir. The oil consists mainly of two terpenes, one boiling at 152-156° the toher 162-167°, an oxidised liquid and a camphor.—A small quantity of absolutely pure sage oil has been examined, and consists mainly of a terpene boiling at 264-270°, of a dark emerald green colour.—On the action of bromine upon sulphur, by J. B. Hannay. The author has investigated the evidence as to the existence of any compounds of these two elements by boiling points, the spectrum of the vapour, specific gravity, and vapour tension. He concludes that the action of any quantity of bromine or any quantity of sulphur is an action on the whole mass and not in multiple proportion, but that if at low temperatures the compound containing one atom of sulphur to two of bromine meets a body with which it can form a molecular combination, *e.g.*, arsenic, it assumes the crystalline form in conjunction with such a body.—On the determination of high boiling-points, by T. Carnelly and W. C. Williams. The authors have determined the boiling-points of various substances by observing whether or not certain salts fuse when exposed to the vapour of the boiling substance. The melting-points of the salts have been determined by Carnelly. The salts are contained in capillary tubes.—On high melting-points, Part IV., by T. Carnelly, D.Sc. The author has perfected his (specific heat) method of determining melting-points, and eliminated two sources of error. In the present paper he gives the melting-points of over one hundred substances. He promises a paper embodying theoretical results deduced from the above observations.

PARIS

Academy of Sciences, May 27.—M. Fizeau in the chair.—The following among other papers were read :—On the production and constitution of chromised steels, by M. Boussingault. This memoir gives experiments proving that chromium, without the presence of iron, does not communicate to pure iron the properties of steel; analyses of cast chromium steel; experiments on the temper, and resistance to shock and traction, of chromised steel; mode of preparation of it and ferrochrome, &c.—On the action of anæsthetics on the respiratory centre and cardiac ganglions, by M. Vulpian. In chloralised dogs faradisation of the upper cephalic segments of the cut vagi stops the respiratory movements just as in dogs not anæsthetised; but whereas, in the latter, the respiration in general easily and spontaneously commences again, spite of the electrisation being continued, it is not so with the former, and the animals die unless electrisation be stopped and artificial respiration be produced, aided, it may be, by energetic faradisation of the trunk. The heart, too, may finally stop in such a case. M. Vulpian thinks this explains certain accidents in clinic anæsthesia.—On the origin of the excito-sudoral nerve-fibres in the sciatic nerve of the cat by M. Vulpian. Those in the abdominal cord of the great sympathetic come from the spinal cord, chiefly by the first and second lumbar nerves; but there are others, and more, which come directly from the spinal cord by the roots of the sciatic nerve. An analogy with the nerves of the salivary glands is indicated.—M. de Lesseps gave details of the pacific conquests, made in the name of the Khedive of Egypt, by Gen. Gordon, and quoted from an official Egyptian report on Capt. Burton's recent important discoveries in Arabia.—Transparent hydrated silica and hydrophane opal, obtained by action of oxalic acid on alkaline silicates, by M. Monier. The experiment should be made with 500 to 600 grammes of silicate of 35° or 40° B; the oxalic acid is diluted to only four degrees. Letting the acid act six months at ordinary temperature, a transparent silicious layer was obtained, which, after heating to expel hygrometric water, took the milky colour and the hardness of opal. It becomes translucid again in water.—On the cost of establishment of lightning-conductors, by M. Melsens. He proves that his system of numerous free conductors and multiple earth-connections is generally less expensive than the construction of the ordinary lightning-conductors.—On a disorder, not hitherto described, of wines of the south of France called *vins tournés*, by M. Gautier. This appears after warm and rainy autumns. The wine becomes troubled, its surface irisated; the colouring matter passes from red to violet-blue, and is precipitated, the supernatant liquor being yellowish-brown, and having a baked odour and an acidulated and slightly bitter taste. These changes are worked by a parasite which appears in filamentous form in the deposit.—On the production of the luminous sensation, by M. Charpentier. Where we find less red substance in the retina, we observe a less luminous sensibility, and wherever the red appears in excess this sensibility is exaggerated. It is concluded that the luminous sensibility, defined as the simple and original reaction of the visual apparatus to all luminous excitations of whatever nature, is in relation to the degree of photo-chemical action exercised on the red of the retina by all the luminous rays.—On the physiological properties of conine, by MM. Bochefontaine and Tiriakian. Conine pure, or bromhydrate of conine, is not a very formidable poison, and not to be compared with hydrocyanic acid (as has been supposed). 65 centigr. of pure conine introduced under the skin of a dog weighing 7 kil. odd killed it in a little over twelve hours; 50 centigr. sufficed for a similar dog when introduced into the stomach. The chlorhydrate and bromhydrate are always more active than the pure conine. M. Mourrut has separated from the conine furnished as pure in shops a resinoid matter, which, like curare, paralyses the motor nerves.—*Rôle* of auxiliary acids in etherification; thermal experiments, by M. Berthelot.—On some peculiarities presented in the arrangement of fire-damp in pits and old works, by M. Coquillion.

CONTENTS PAGE

ERRATA.—In Prof. Lankester's review of Balfour's "Elasmobranch Fishes," vol. xviii. p. 114, 2nd column, line 22 from top, for *homogeneous* read *homogenetic*. In Dr. Siemens' letter on the microphone, p. 129 1st column, lines 25 and 28 from top, for *corpuscular bodies* read *corpuscular matter*.

THURSDAY, JUNE 13, 1878

ETHNOLOGY OF NORTH-WEST AMERICA

United States Geographical and Geological Survey of the Rocky Mountain Region; Tribes of the Extreme North-West. By W. H. Dall. *Tribes of Western Washington and North-Western Oregon.* By Geo. Gibbs. (Washington, 1877.)

WE have already had occasion to draw attention to the extremely good work which is being done by the United States Geographical and Geological Survey, as well as to the liberal construction put upon the duties attached to it. The volume now issued is another striking illustration of both facts, and makes us wish that other Governments would follow the example of that of America. For the first time we have full and accurate details regarding the Eskimaux and other tribes of what was once Russian America and of the adjoining territory. Not only has all the available literature on the subject been consulted, but the errors and deficiencies of former writers have been corrected and supplemented by personal and patient observation. A wholly new light has been cast on the ethnology of these remote regions, and both the ethnologist and the philologist will obtain fresh materials of the utmost value. The scanty and often inaccurate information we have hitherto possessed has now been replaced by the full and careful statements of scientifically trained observers.

Mr. J. W. Powell was the geologist in charge of the expedition, and it is the materials collected by him and his assistants that have been thrown into shape and edited in the present volume by Mr. W. H. Dall and the late Mr. George Gibbs. Mr. Dall's elaborate articles on the tribes of the extreme North-West acquire additional importance from the fact that they represent in large measure the fruits of his own researches. Since 1865 he has visited nearly the whole of the north-west coast, besides a good deal of the interior, and his statements have consequently all the advantage of being the results of personal knowledge. His first article describes the various tribes of Alaska and the adjacent territory, comprising the Innuit or Eskimaux, and their off-branch, the Aleuts, and the Indian tribes belonging to the Tinneh and T'linket groups. Mr. Dall seems to exclude the Chukchis from the Innuit family; to use his own words, they are "totally distinct in language and race from the nomadic 'reindeer-people' with whom they trade." Their trade-jargon, by the way, is a *lingua franca* composed of words or corruptions of words belonging to both. However, nothing is more remarkable than the differences in manners and condition between many of the tribes described by Mr. Dall, who must, nevertheless, be of the same origin, and this fact shows how rapidly and widely savage tribes will come to differ from one another, even when living in close proximity. Mr. Dall's second article is a very interesting one on his exploration of the numerous kitchen-middens of the Aleutian Islands. Usually the middens consist of three layers, the lower most being composed of echinus and similar shells, the second of fishbones, and the third of mammalian and other remains, the ruins of villages of a more recent period often crowning the whole deposit. The lowermost layer must go back to a remote date, and as Mr. Dall points out, would have required an immense number of years to form. The savages to whom it bears witness, must have been very low in the scale of humanity. They have left no traces of fire, weapons, or implements; indeed, had they possessed any, they would hardly have been content to subsist on sea-eggs. It is only towards the top of the layer that net-sinkers of a very rude pattern have been met with; but these may have worked their way through from the layer above, though the possibility does not seem to have suggested itself to Mr. Dall. With the fish-bone layer stone tools come into use; as Mr. Dall observes, "fish, when raw, is a substance which cannot be conveniently dismembered by teeth and nails." But it is not till we come to the mammalian epoch that the weapons show a decided improvement in form with attempts at ornamentation, though from the first the types are remarkably like those still used by the Eskimaux. Lamps, also, first came into use during this period, and no doubt the improvement in the tools was largely due to the lengthening of the working day by the introduction of artificial light.

Mr. Dall's third article is on the Origin of the Innuit. He differs from Mr. C. R. Markham in thinking that the Innuit emigration goes back to a vast antiquity, and agrees with Dr. Rink in holding that the Innuit have not come from northern Asia, but been pushed northward from the interior of America itself. Like the walrus they can be shown to have once ranged as far south as New Jersey. He admits, however, that green patches similar to those that mark the Aleutian kitchen-middens, have been observed by whalers on the shores of Wrangell Land, and he is certainly wrong in stating that "Linguistically, no ultimate distinction can be drawn between the American Innuit and the American Indian.". It is true that both groups of languages are polysynthetic, not agglutinative, as Mr. Dall affirms, but it is doubtful whether all the Indian languages even can be referred to a single source, and certainly the Indian and the Eskimaux cannot be. Nor, again, does Mr. Dall seem to be right in suggesting that the Arctic Highlanders, who have no means of navigation, represent the original condition of the Innuit tribes generally; they must rather be regarded as an instance of degradation and relapse. But he does good service in pointing out the untenability of the theory which would bring the first inhabitants of America from Asia, by way of the Aleutian Islands. Apart from the fact that Behring found no traces of inhabitants on the islands named after him, and that the echinus layer in the Aleutian kitchen middens pre-supposes a population without means for crossing the sea; "we find that a gap of 138 statute miles separates the Commander's Islands from Kamschatka, and another of 253 miles exist between the former and Attu. Here is one of the deepest gulfs known in any ocean, over which rolls a rough, foggy, and tempestuous sea." Three appendices are attached to Mr. Dall's part of the work: one on the natives of Alaska, another on the terms of relationship used by the Innuit, while the third gives comparative vocabularies of the tribes of the extreme North-West. The first appendix contains the outlines of two grammars, one belonging to the Sitka dialect of the T'linket Indians and the other to the Innuit Aleuts of Unalashka, both by

M. Furuhelm. The second will prove of considerable importance for Eskimaux philology. The most noticeable fact connected with Sitka grammar is that "there are only two cases, nominative and instrumental," and that the instrumental case of the pronouns is employed with active verbs, which means that no true verb exists in the language.

The second half of the volume is occupied by an exhaustive account of the tribes of Western Washington and North-Western Oregon, by Mr. George Gibbs. While the ethnology of these tribes has been treated minutely, their dialects have received the attention to be expected from so able a philologist, and lengthy tables of comparative vocabularies are followed by a complete Niskwalli-English and English-Niskwalli dictionary. Mr. Gibbs begins by saying that "in the western district of Washington Territory—that is to say, between the Cascade Mountains and the Pacific—there is found, compared with the extent of country occupied, an extraordinary diversity in the aboriginal tongues. Mr. Hale, the ethnologist, who accompanied Capt. Wilkes's expedition, recognised among them eight languages belonging to five distinct families, and to these are now to be added six other languages which escaped his observation. In addition there are several but partially intelligible, even to those speaking the same general language." It is the old story; the lower we descend the larger becomes the number of dialects and independent tongues which it is the part of civilisation to destroy and unify. The further back we trace the stream of human speech the greater is its diversity, the more manifold its forms.

Among the ethnological facts brought to light by Mr. Gibbs, may be mentioned the universal flattening of the skull, the use of the haikwa shell as a medium of exchange, and of armour composed of elk-skins or of thin pieces of hard wood. Scalping is unknown, as are also totemism and the division of the tribe into clans, while, on the other hand, "slavery is thoroughly interwoven with the social polity of the Indians of the coast." Earthworks are found in various parts of the district, though they never present the figures of animals, and the existing Indians have no traditions of their origin. But there are clear evidences that the present population of the country is a mixed one, and was probably preceded by a more civilised race. Thus the Makah differ from the Indians of the Sound "in features and habits as much as language." In fact, the Indians of North America differ among themselves, both physiologically and linguistically, no less than the natives of Europe, and to lump them together under a single name is as rude and unscientific a proceeding as that of the Greeks and Romans, with whom all other peoples were "barbarians." If the labours of Mr. Powell and his assistants do nothing more than impress this fact on the student of language and race, they will have effected a good and needful work.

A. H. SAYCE

CULLEY'S PRACTICAL TELEGRAPHY

A Handbook of Practical Telegraphy. By R. S. Culley. Seventh Edition. (Longmans, 1878.)

THIS well-known book has reached its seventh edition. It was first published in the year 1863, and 190 pages were sufficient to recount its practical instructions. Now 450 pages scarcely suffice to accomplish its purpose. The book reminds one of some old house that has been added to from time to time by different occupiers until it has lost all trace of plan or design. Valuable teachings of experience on one subject are found buried here and there in chapters devoted to other subjects. It is a pity that the author did not thoroughly revise and rewrite his book. It is more like some old housewife's recipe book, full of useful and valuable information, scattered indiscriminately about, than a methodical scientific manual of a grand practical art which has grown within the last few years with gigantic strides. It never pretended to be the result of scientific originality or profound research, but simply to be a practical book intended for practical men. Its great success is more a proof of its want than of its merit. Nevertheless it has merit, and that of no mean order.

Commencing with the sources of electricity and the laws of the current, of magnetism and electromagnetism, of induction and of atmospheric and cosmic electricity, it proceeds to describe the construction of a line of telegraph, both over-ground and under-ground. Modes of testing the various apparatus used, and the systems for signalling are fully described. Cable working and testing receive very exhaustive treatment. The automatic system of working—the child of Bain and the pupil of Wheatstone—receive full handling, and the recent developments of the duplex and quadruplex systems receive their fair share of description. The telephone is not neglected, but we must wait for an eighth edition for the later wonderful developments of Hughes and Edison.

In speaking of the history of the telegraphic system in this country in his meagre but pithy introduction, Mr. Culley says:—"No assistance whatever was granted by the Government, and it was only after several years of adversity that the undertaking became firmly established." Rather a strange remark from the pen of an officer of a company who owed its foundation to the support of the Government. The first contract of any magnitude ever made by the founders of the company was with the Government, who agreed to pay 1,500*l.* a year for twenty years, and 1,000*l.* a year for another twenty years, for telegraphic communication to Portsmouth; and it was this contract that enabled them to float their concern. However, it is an Englishman's happy privilege to abuse to his heart's content his own Government for what it does not do, and to ignore entirely what it does do, and we should be sorry to interfere with his prerogative in this respect; but it is curious to find a Government official making such a sweeping and erroneous statement as the above in a book accepted by his department as its text-book.

It is in the development of submarine telegraphy that England principally shines on the Continent and in America, and it is surprising to find our author omitting all mention of her great deeds in this field. English enterprise in this respect is most marked. English capital is invested in every sea, and English genius has surmounted every difficulty, whether natural, mechanical, or electrical. In 1876 the length of cable laid was 63,990 nautical miles, of which 59,547 were owned by private companies.

There is a great tendency to deny the existence

of English inventive genius. The over-shadowing influence of the recent sensational inventions of the telephone and phonograph have led even practica men to believe that inventive power had crossed the Atlantic, but no one who reads Mr. Culley's book can fail to learn how much has been done in England. Though duplex working was revived by Hearns, and quadruplex made practical by Edison, neither was invented in America. Cn the other hand, Hughes's beautiful type-printer was born in America, but it was developed in Europe, and its birthplace knows it not. Thomson's syphon recorder, Varley's double-current translator and condenser working, Bain and Wheatstone's automatic systems, fast-speed translators, and all the valuable systems and apparatus in use for testing have sprung from here, and are well described in this work. The Post Office telegraph system, in its technical department, is a credit to this country and a pattern to the world, and it possesses on its staff some of the most practical electricians of the day. Messrs. Preece, Lumsden, Marson, Gavey, and Kempe are well known everywhere, and though their labours are not acknowledged by Mr. Culley, it is well known that they have contributed materially to establishing the telegraphic system of the Post Office. It is especially in developing the automatic system and in establishing fast-speed translators that the Post Office officials have been so successful. A relay station in Anglesey has increased the rate of working between London and Dublin from 70 to 120 words per minute. Translating relays working at the rate of 120 words per minute are quite new in telegraphy. Mr. Culley has given scant justice to Mr. John Fuller for his new form of bichromate battery, a battery that is coming into very extensive employment for all purposes. It is a zinc-carbon couple, the exciting fluid being Poggendorff's mixture. Its peculiarity consists in the shape of the zinc, which is permanently inserted in a bath of mercury. Its electromotive force is double that of a Daniell's cell, its constancy wonderful, its economy great, and its cleanliness and freedom from smell all that can be desired.

This work is deservedly popular, not from its literary merit, but from the position of the author and from the great mass of very valuable practical information it possesses.

OUR BOOK SHELF

Manual of the Vertebrates of the Northern United States, Including the District East of the Mississipi River and North of North Carolina and Tennessee, Exclusive of Marine Species. By Prof. D. S. Jordan, M.D. Second edition, Revised and Enlarged. (Chicago: M^c^Clurg, 1878.)

THE object of this volume is to give collectors and students a ready means of identifying the families, genera, and species of the vertebrate animals of North America. Following the usage of botanists, the author has adopted the system of artificial keys to the classes, orders, families, genera, and species, while use has been freely made of every available source of information. The account of the mammals has been chiefly compiled from Prof. Baird's work, and Dr. Coues has given great assistance in the part relating to the birds; while in this edition the account of the fishes has been entirely re-written in order to include the results of recent investigations in that department. The fact that a work of this nature should in two years' time call for a second edition, is, indeed, a proof of the interest taken in natural science by the American people. This edition seems to fairly represent the present state of knowledge.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

The Phonograph and Vowel Theories

SEVERAL letters have appeared in NATURE bearing on the subject of the phonograph, and referring to our first communications upon the subject. We are glad to see that our statement as to the reversibility of consonants (NATURE, vol. xvii. p. 423) is generally accepted. We feel that as yet the phonograph does not speak with sufficient clearness to determine how perfect this reversibility is, and that the effect of many minute parts of articulate utterance cannot be heard with any certainty. Mr. Ellis, in his first communication, ranked the phonograph somewhat too low, but we are more than satisfied with the acknowledgment in his second letter (vol. xviii. p. 38). Mr. A. M. Mayer and Prof. Sylvanus Thompson both speak of the marks on the tinfoil as differing according to the distance of the mouth from the diaphragm. We do not observe any effect of this kind and see no theoretical reason for any alteration in the relative phases of the simple tones with a change of distance from the mouth. Mr. Mayer seems here to have fallen into an error. We find ample confirmation of Helmholtz's statement that the phase relation between two constituents is not appreciated by the ear. Each person usually, but not invariably, adheres to the same phase relation on one pitch, but different people pronouncing the same vowel with approximately the same constituents, combine these differently, which, as Mr. Mayer says, would make reading the marks on the tinfoil a very difficult matter.

With reference to the letter by Mr. C. R. Cross which appears in NATURE, vol. xviii. p. 93, we adhere with much confidence to the opinion that the five vowels, *a e i o u* (Italian), pronounced in succession, are by contrast at least thoroughly distinguishable when the instrument is run at various speeds, such as to reproduce the sounds at all the pitches within the compass of the average human voice. That no marked change is produced in the relative values of the vowels is confirmed by the fact that neither in public nor private exhibitions do the hearers of sentences alternately run slow and fast suggest that the vowels have changed with a change of speed. This alone would be a sufficient proof that *oh* does not change into *ee*, as we understand Mr. Cross to say, and there is no ground, according to Helmholtz's theory, for expecting that it would. To us the relative sounds of the vowels at various speeds seem at least as perfect as those obtained from Willis's well-known experiment, where a succession of vowels is suggested by contrast when the length of a resonating tube is altered.

We do not, however, think that our instrument speaks with sufficient distinctness to warrant our expressing an opinion as to the constancy of quality of any single vowel when the instrument is run at various speeds.

Some *oh*s remain apparently very constant, and at times we thought that other *oh*s became brighter or more like "awe."

Sometimes we thought *awe* became very like "ah." We should be glad to learn the impressions of any of your readers as to this point.

We venture, however, to remind any one trying the experiment that a low note followed by a high one suggests a change from *u* (Italian) to *i*. Thus if we whistle a low note and then the octave to it or a note near this, the ear is easily persuaded that the whistle resembles *u i*, but if now, beginning again on the note we just thought was *i*, we go up another octave, the new sequence again suggests *u i*, although the very note which was last taken to represent *i* now stands for *u*. If, therefore, we wish to judge what a sound really is we should not trust much to contrast, especially when a change of pitch is involved in the comparison.

We have now obtained and analysed a very large number of vowel curves, especially for the sounds *o* and *u*, and with your permission will send a selection of these for publication after the results of our investigation have been communicated to the Royal Society here. These curves show what the voice effects when singing at different pitches a vowel which remains of constant quality according to the appreciation of the speaker. The analysis of the curves gives the partial tones of which the vowel sounds are composed, and it becomes a matter of considerable interest to see how far these results confirm or contradict existing theories.

We therefore propose to give a short sketch of these theories, hoping that if in error, we may be at once corrected.

Prof. Willis showed (Cambridge *Phil. Soc. Trans.*, vol. iii.) that, by varying the length of a tube attached to a free reed, he was able to produce the sensation of a change of vowel sound. This sensation is not very definite, especially for the vowels *i* and *e*; but, when the length of the tube is changed rapidly, the ear accepts the suggestion of a change from *u* to *o*, *a*, *e*, and *i*. Prof. Willis concluded that the vowel quality was given in each case by the coexistence of the note proper to the tube with that of the reed, whether the former was or was not harmonic to the latter. One must bear in mind that Willis wrote before it was recognised that all musical sounds are compounded of harmonic partial tones, and also before the function of a resonator was understood. This we may call the absolute single-tone theory. Wheatstone pointed out that the tube used by Willis acted simply as a resonator.

F. C. Donders (*Donders' Archiv*, vol. i.) observed that when the mouth was formed to speak a given vowel, the cavity had a certain definite pitch of resonance, or maximum resonance, which he determined by observing the pitch of the whispered vowel. Donders and Helmholtz agree in considering that this characteristic pitch is nearly constant in man, woman, and child, for a given vowel. Donders also observed that the vowels were, in certain cases, accompanied by a "geruisch" or "whish."

Helmholtz attacked the subject in a different way. By means of resonators he applied a qualitative analysis to the sounds which came out of the mouth cavity when a vowel was spoken, and pointed out that a vowel-sound at a particular pitch was characterised, not by a single tone, but by many tones. In an early paper, translated in the *Phil. Mag.* for 1860, he describes not only the analysis by resonators, but the synthesis by means of tuning-forks, which is now a familiar experiment. In this paper he appears inclined to believe that it is the relation between the constituent tones which determines the vowel quality; that, for instance, any pair of simple tones one octave apart will, if properly proportioned, always make an *o*. This theory made considerable way; it is taught with small qualification as to absolute pitch in Tyndall's lectures on sound, and elsewhere; it may be called the relative-pitch theory.

Helmholtz himself does not seem ever to have formulated it, for although the paper referred to distinctly suggests it, he guards himself by saying that the relations observed can only be considered as proved for the particular forks used, and, in fact, his experiments were only made at two parts of the scale.

In the "Tonempfindungen" the relative-pitch theory is entirely abandoned, but it is not a little difficult to ascertain what Prof. Helmholtz's latest theory is. This difficulty is indeed admitted by his translator, Mr. A. J. Ellis.

By Herr von Quanten, whose papers were published in *Poggendorff's Annalen* for 1875, Prof. Helmholtz was understood to mean that for each vowel one, and in some cases two, tones of definite absolute pitch must be strongly present, these tones being those which Prof. Helmholtz calls the characteristic tones of the vowel. This would imply either that each vowel could only be sung on a very few notes, or that the characteristic tones were present as inharmonic partials. Neither of these conclusions being in accordance with fact, von Quanten concluded that Helmholtz was wrong; but Mr. Ellis, with justice, as we think, points out that the true conclusion ought to have been that Helmholtz could not possibly have meant to broach an absurd theory.

We confess that we were ourselves led to believe at first that Helmholtz taught that each vowel contained strongly either its characteristic tone or some of the higher partials of that tone or tones very near these, and that this was what gave it its distinctive character. This was the theory which our first experiments seemed definitely to contradict. We now believe, however, that this is not the doctrine taught by Helmholtz.

Indeed we fail to find in the "Tonempfindungen" any very complete vowel theory, but we think that the following passage, taken from Donders' pamphlet "De Physiologie der Spraakklanken, 1870," expresses very clearly a doctrine which is very generally looked upon as that of Helmholtz.

Donders says (if our translation from the Dutch be correct):—

"Vowels spoken loud are sounds of a determinate timbre maintained unaltered, depending on the form of the mouth-cavity and of the mouth-aperture, and, even without the accompanying 'whish,' characterised by strong comparatively low upper tones not occurring in a definite order relatively to the prime tone, but for each vowel of an approximately constant pitch."

We understand Donders to believe that on whatever part of the scale a vowel be spoken the pitch or pitches of maximum resonance of the cavity are constant for a given vowel, and that indeed the form itself is constant. This may be called the *constant cavity theory*, and is taught by Mr. Ellis as the doctrine of Helmholtz.

We fail to find that Helmholtz himself has stated this doctrine definitely in all its rigidity, although he accepts the results of Donders' experiments, and has himself confirmed and amplified them. Almost every statement made by him concerning vowels is limited to those which he could produce by forks forming an harmonic series with B♭ for the prime. Any experiment described by Helmholtz is of course to be relied on, and so far as we have yet traversed the ground we find that the phonograph gives results in accordance with his experiments as to these constituents, but when we examine the one or two more general statements made by Helmholtz we find room for doubt, both as to his meaning and as to the truth or completeness of the conclusion.

Thus at the end of Chapter V. Helmholtz says:—"Vowel qualities of tone consequently are essentially distinguished from the tones of most other musical instruments by the fact that the loudness of their partial tones does not depend *only* on the numerical order but *chiefly* upon the absolute pitch of those partials; thus when I sing the vowel A to the note E♭ the reinforced tone b''♭ is the twelfth partial tone of the compound; and when I sing the same vowel A to the note b'♭ the reinforced tone is still b''♭, but is now the second partial." The two words marked by us in italics have been introduced for the first time in the fourth edition.

This passage might very well be understood to mean that a certain tone of perfectly or at least very approximately definite absolute pitch must necessarily be present in a given vowel. Further examination has, however, convinced us that Prof. Helmholtz does not require the presence of any characteristic tone or of any one of a group of characteristic tones in a vowel. This is made obvious by Chapter VI. In this chapter, which treats of artificial vowels, we find that in order to make an *e* by tuning-forks Prof. Helmholtz employed *b*♭ and b'♭ as "being adjacent to the deeper characteristic tone f'," which in fact lies midway between them; in the same way he employs f''', a'''♭, and b'''♭ for the same vowel, treating all these as adjacent to the higher characteristic tone b'''♭. Thus the theory of Prof. Helmholtz is satisfied if tones lying anywhere within a whole octave be present, provided the characteristic tone lie somewhere near the middle of that octave. This is consistent with Donders' statement of the theory, provided "approximately constant pitch" be allowed to signify anything within six semitones.

We consider the following abstract as representing the doctrine taught in the "Tonempfindungen":—

1. For a given vowel there is a certain form of mouth cavity which has a pitch (sometimes two pitches) of strongest resonance —as b'♭ for *o*.

2. If this vowel be spoken or sung on any subtone of this pitch, the overtone corresponding to that pitch will be strongly present.

3. If the same vowel be pronounced at some other pitch then these harmonic partials will be reinforced which lie within, say, six semitones of the characteristic pitch.

No opinion seems to be expressed on the following two points:—

1. Whether the mouth cavity for a given vowel remains constant when the pitch of the vowel is altered. Mr. Ellis understands Helmholtz to affirm this, which is apparently Donders' view, but we have failed to perceive any passage in which this is definitely asserted. Helmholtz says the cavity for a given vowel has a pitch of strongest resonance, but this is not

quite the same statement as saying that when that vowel is spoken at all pitches the same cavity is employed.

2. Whether the mouth-cavities for given vowels are supposed to differ phonetically *only* in respect of pitch of maximum resonance.. Helmholtz states clearly that in respect of their pitch of maximum resonance they are different, but he does not clearly say whether or no any other differences are essential. There are passages which seem to show that he considers that any resonator of the required pitch (whether in the least like the mouth in shape or material) would answer as well, or nearly as well, as the special mouth-cavity for the production of a given vowel. On the other hand it is at least conceivable that the cavity for, say, *o* may be very different from that for *a* in other respects than simply in the pitch of maximum resonance. As to this we find no statement in the "Tonempfindungen."

In fine we do not see that Prof. Helmholtz, although he has largely added to our knowledge concerning vowels, has laid down any law by which, given the pitch at which any one vowel is to be spoken, the reinforcement of its constituent tones could be even roughly predicted. This prediction could, however, be roughly made upon the constant-cavity theory, and has been made by Mr. Ellis in his valuable additions to the translation of Helmholtz's work. Prof. Helmholtz seems to do little more than tell us the constituents of a series of vowels sung or said on two notes of one scale, coupled with one peculiarity and in some cases two peculiarities of the resonance cavity. He has avoided all general conclusions except that quoted above, which states that the vowel peculiarity depends chiefly on the absolute, and not on the relative pitch of the partials.

In our next communication we hope to be able to state how far the information we have derived by means of the phonograph contradicts, supports, or supplements the above theories.

Edinburgh, May 29 FLEEMING JENKIN
J. A. EWING

Extinct and Recent Irish Mammals

I BEG to thank Prof. Leith Adams for his criticism, in NATURE, vol. xviii., p. 141, of my "Preliminary Treatise on the Relation of the Pleistocene Animals to those now living in Europe" (*Palæon. Soc.*, 1878), in which, from the nature of the work, it is impossible that mistakes should not be. I cannot, however, plead guilty to some of the mistakes which are placed to my credit:—1. That "the Irish elk is placed among the pre-historic mammals in consequence of its presence in the peat-bogs of England, Scotland, and Ireland." What I wrote (p. 6) was that the presence of the extinct Irish elk in the peat-bogs, which are of well-ascertained pre-historic age, renders it impossible to accept Sir Charles Lyell's definition of the term recent, in which no extinct species are stated to occur.

Of course the Irish elk, as Prof. Leith Adams remarks, has long been known to be met with, almost universally, in the lacustrine marls underlying the peat, and it is thus described in p. 27 of Mr. Sanford's and my own Introduction (*Palæon. Soc.*, 1866). I do not know of its occurrence anywhere *in peat*, but at the bottom of peat-bogs, to which the bones of animals suffocated in the peat in all probability gravitate. It seems to me very unlikely that all the remains at the bottom of peat-bogs belong to a period before the peat was accumulated.

2. I have never held, and still less to my knowledge printed, that "man and Irish elk, reindeer, mammoth, horse, and bear, were contemporaneous in Ireland." Evidence of palæolithic man, the contemporary of the mammoth in Ireland, is, so far as I know, altogether wanting. If Prof. Leith Adams will kindly write me a reference to any such statement of mine it shall be corrected at once.

My list of Irish animals, which merely purports to give the principal historic mammalia, does not profess to give all the mammalia, which will doubtless be fully treated in Prof. Leith Adams' promised work. W. BOYD DAWKINS

Owens College, Manchester, June 9

Alternate Vision

MR. GALTON'S remark (NATURE, vol. xviii. p. 98), that "sometimes the image seen by the left eye prevails over that seen by the right, and *vice versâ*," leads me to describe a curious defect in my own eyesight, which in a different way confirms what he says. While my right eye is fairly long-sighted, my left eye is very short-sighted. For instance, the focal distance of my right eye for your leader type is 18 inches, and for the left eye only $8\frac{1}{4}$ inches. For your letter type the focal distance for the one is 16 inches, and for the other $6\frac{1}{2}$ inches. This is by the light of a Duplex lamp, and by focal distance, I mean the distance at which I can see distinctly. The result of this inequality in my two eyes is that the right—or long-sighted one—involuntarily closes when I read, and I am not aware of its being shut, except when some one who is a stranger to the peculiarity calls attention to it. During the day, however, in looking about both eyes are generally open, though when I look intently at a distant view, I find the short-sighted eye shuts occasionally. But in a general way both eyes are open, and I have two distinct images presented to my brain, one blurred and indistinct, even for faces a yard distant, and the other clearly defined, I believe, to the usual distances. How is it that my brain or mind rejects the blurred image and chooses the distinct one, so that I see everything perfectly clearly. If I get a piece of dust in the good eye, or close it, I immediately see the blurred image, and if this take place in the street, it causes a painful degree of confusion as to distances, &c., so that I am often brought to a standstill by such an occurrence. That both images really are presented to the brain I know. For instance, in travelling by train I frequently amuse myself by placing my eyes so that the short-sighted eye sees a portion of a scene through the window, without the good eye being able to see it. Then I see the blurred image only; but as the train moves the blurred is replaced by the bright one, as the good eye gets to work. The blurred image always appears at a higher level than the other, and it is the same when I shut my good eye for a moment and look at the fire with my bad one. On reopening the good one the blurred fire appears slightly above the bright one, and the latter almost instantly drives the indistinct image away—like a dissolving view. Things appear, as a rule, much flatter to me than to people who enjoy binocular vision. I know this because I have a pair of spectacles so arranged as to equalise my sights. When I put them on, objects like trees put on a delightful fulness and roundness to which I am usually quite a stranger, and the effect is most charming. I may add that two of my brothers have a similar defect of vision.

May 31 J. I. R.

The Eskimo at Paris

I HAVE read with great interest in vol. xviii. p. 16 of your renowned journal the article concerning the Eskimo, the exhibition of whom in Paris, &c., has recently made so great a sensation.

Unfortunately, it seems to me, the writer of the article, M. A. Bordier, has been incorrectly informed with regard to the introduction of these people. It is not to Mr. Geoffroy St. Hilaire, the director of the Paris Jardin d'Acclimatation, but to M. Charles Hagenbeck, the well-known and intelligent dealer in wild animals of our town, to whom science is indebted for the introduction both of the Eskimo, the Hamran and other types of the different tribes of Nubia, and the Laplanders.

I should be much obliged to you if you would kindly insert the above correction in an early number of your journal.

Hamburg, May 28 J. D. E. SCHMELTZ

The Telephone

HAVING seen a paragraph in NATURE communicated by Mr. Severn, of Newcastle, New South Wales, describing a method of using a telephone to enable deaf persons to hear, I have tried the experiment in the manner Mr. Severn describes—by fastening a string to the parchment diaphragm of a simple telephone made of wood, and carrying this string round the forehead of the deaf person, who clasps the string with both hands and presses them over his ears. The experiment in this way was partially successful; the sound of the voice was always heard, and some words were distinguished. Afterwards I fastened a single string to the telephone and got the deaf person to hold the string between his teeth. He then heard every word distinctly, even when spoken in a low tone of voice at the whole length of the room.

63, Strand, W.C. JOHN BROWNING

TILL now I have looked in vain for any account in NATURE of experiments with the telephone or phonoscope, inserted in the circuit of a selenium (galvanic) element (see NATURE, vol. xvii. p. 312).

One is inclined to think that by exposing the selenium to light,

the intensity of which is subject to rapid changes, sound may be produced in the phonoscope. Probably by making use of selenium, instead of the tube-transmitter with charcoal, &c., of Prof. Hughes, and by exposing it to light as above, the same result may be obtained.

I should be glad to know whether experiments have been made in this direction; for if the above should prove true, there is no doubt that many applications would be the result.

Kew, June 3 J. F. W.

Meteor

HAVING just seen a magnificent meteor, I send you an account of it, as from its position it may have been seen at Gibraltar.

At 7.30 this evening a large meteor appeared as nearly as possible N.E. by E. of my position, at about 25 to 28° from the horizon, in a wide opening in the clouds, and proceeded with a moderately fast motion towards the north, slightly descending in a path slightly concave to the horizon. I did not see it disappear, as it went behind some bushes which hid the sky between N. by W. and N. by E.; if it disappeared due N. it would have been about 20° from the horizon as estimated by the altitude of the pole-star. The appearance was very remarkable, the head being of a brilliant green and the tail bright red. When I first saw it I took it for a first-class rocket passing at about 300 or 400 yards from me with a bright Bengal light of green colour at its head. The brightness was certainly from 10 to 15 times that of Venus at its brightest. It shone in the twilight more brilliantly than I ever saw Venus against a dark sky. The tail was not persistent as far as I could judge, against the light sky, and no report was heard, though I listened for several minutes. A bright star, which I believe was Vega, was just below it among the clouds, and afforded a fair standard of comparison; it was from thirty to forty times, at least, brighter than this star. W. A. SANFORD

Funchal, May 27

P.S.—I find that I have forgotten to mention that my position is about two miles south-west of the cathedral of Funchal.

Multiple Rainbow

ON Saturday evening I (and others) observed a rainbow which presented a very peculiar phenomenon. The primary bow, in the neighbourhood of its apex, was apparently composed of three distinct bows. Just below the violet of the principal bow the bright portion of a second bow was observed, and at about half the distance between the bright portions of these two bows was observed the bright portion of a third bow. The secondary bow looked much as usual, and the principal primary bow was very perfect, so far as I could see, on each side. The repetitions of the primary bow extended only through an angle of 35° or 40°, and did not apparently end at the same point.

Between the point of observation and the sun are some pieces of still water in Bushey Park. Overhead were some clouds upon which the sun was shining. I think the phenomenon was due to the reflection of the sun from the clouds. R. S.

Hampton Wick, June 1

Opening of Museums on Sundays

MANY of your readers will be glad to know that the very admirable and extensive museum at Maidstone was opened to the public on Sunday last, and will in future be open on Sunday afternoons from two to six o'clock.

The opening was a great success: the mayor and many of the influential inhabitants were present, and more than 1,000 people visited the museum on that afternoon, the average attendance on week-days being from 50 to 100. The most perfect order was preserved, and every part of the museum received its share of attention, even the library being more than full of readers.

I believe that this is the first and only scientific museum that has yet been opened on Sunday in the United Kingdom, the Art Gallery at Birmingham and Aston Hall being of a different character, and so I have thought it worth while to call your attention to it.

For the sake of those who have not yet visited Maidstone Museum I may say that it is one of the best local museums in the country, having remarkably fine palæontological, conchological, and other collections; that it will well repay a visit, or more than one; and that Mr. Bartlett, the courteous curator, is always ready to give visitors any assistance that he can. Maidstone itself, and the country round, are well worth visiting. I must not forget to mention the cemetery, which is one of the most beautiful in the country.

10, Bolton Row, Mayfair, W., June 10 W. H. CORFIELD, Chairman of the Committee of the Sunday Society

THE FISHERIES OF BRITISH NORTH AMERICA

I.

IT was provided by the Treaty of Washington, that, on payment by the United States of a compensatory sum (to be determined by a Commission) to the Dominion of Canada, the Fishing-grounds of British North America should be entirely thrown open to the fishermen of the Union; those of the United States coast, on the other hand, being opened to the fishermen of the Dominion only as far south as the 39th parallel of N. lat., which is almost exactly that of Washington. While the payment of the compensation since awarded by the Commission is being protested against by not a few influential politicians in the United States, the probable influence of the Fishery clauses on the future of the Dominion of Canada is being carefully considered in those parts of it which they especially affect; and we have before us a very able report on this subject by Mr. H. Y. Hind, M.A., a Member of the Legislature of Newfoundland, of which, as based on a careful scientific study of the physical and biological conditions involved in the questions at issue, we think that a summary will prove interesting to our readers.

It is somewhat startling to be told that "as a maritime power the Dominion of Canada stands *fifth* among the nations of the world." This expression, however, is obviously meant by Mr. Hind to refer, not to its *armed* but to its *commercial* marine, which is only surpassed by that of the Mother country, of the United States, of Norway, and of Italy. Its vessels number more than 7,000, and their registered tonnage amounts to above a million and a quarter tons, increasing at the rate of 60,000 tons per annum; its supply of trained seamen is drawn from a fishing population scattered over 3,000 miles of sea-board; and the annual value of their catch reaches at least 20 millions of dollars. The political importance of sea-fisheries as a nursery for seamen, irrespective of the pecuniary value of the catch, is admitted on all hands; and hence it is that a far-sighted policy looks to the value of the British American Coast fisheries as consisting not only in their present productiveness, but also in the security they afford for the maintenance and permanency of what has of late become one of the greatest industries of the Dominion—the work of ocean-carrying.

Now, while the length of the coast-line in British America not covered by previous treaty-arrangements, which is now opened to the United States fishermen, is about 3,700 miles, and the area of its coastal fishing grounds is about 11,900 miles, the length of the United States coast-line opened to British fishermen, is only 1,030 miles, and the area of its fishing-grounds about 3,500 miles. But the respective values of these grounds are not to be estimated by their relative extent alone; for while the United States fishing-grounds north of the 39th parallel were formerly extremely productive, they are now much less so, chiefly through the improvidence of their own people; the cod-fishery, in particular, having been ruined in a great measure beyond repair. On the other hand, the United States coastal waters south of the 39th parallel still maintain much of their original productiveness, supplying a very large quantity of fish to the markets of New York and the South. But to these prolific fishing-grounds access is forbidden to British-American fishermen, who are thus placed at a great disadvantage compared with those of the United States; the latter being

able, when the approach of winter makes the fishing industry of the Dominion coasts hazardous or impracticable, to start at once for the southern grounds, where they can pursue their calling through the winter months. This is so great an advantage, that it frequently renders a northern summer fishery remunerative, which would not be so if the fishermen were dependent upon it alone.

The fish which frequent the United States coast-waters south of the 39th parallel are chiefly of the "anadromous" kind—that is, they live for most of the year in the sea, where they attain the greatest part of their growth, running up into fresh waters for the purpose of spawning. The chief among these are the shad, the alewife or fresh-water herring, the rock-fish, and the striped bass. On the other hand the "commercial" fishes—the cod, herring, haddock, hake, halibut, and mackerel—are found in greatest abundance where the temperature is kept down by the Arctic current, which at the same time furnishes their great store-house of food, and the temperature congenial to them. On the fishing banks of the open sea, the abundance of hake and cod depends essentially upon the resort of herrings; but it is by the "anadromous" fishes that the cod is attracted in-shore. And the destruction of the cod-fisheries which formerly existed on the New England coast is attributed by the United States Fisheries' Commission to the comparative annihilation of the "anadromous" species, through the obstruction and contamination of the river-waters by the various land-industries established along their banks. Below the 39th parallel, however, the "anadromous" fishes find an accessible winter's home in the warm water off the coast of the Southern States, and enter its rivers to spawn as early as February. The United States fishermen being privileged to follow them thither, are thus placed in a position of great advantage as compared with those of the Dominion; the enterprise of the former being stimulated, while that of the latter is cramped, by the Fishery-clauses of the Treaty of Washington, which, as Mr. Hind points out, "place an obstructive boundary on the operations of the British-American fishermen far more limited and confined than formerly existed under the Reciprocity Treaty, while in the same breath they remove every impediment to perfect freedom of action to the United States fishermen throughout an area of great productiveness and practically unlimited extent."

The Physical conditions under which marine life exists on the coasts of British North America, differ in this important particular from those which prevail in the seas of Northern Europe—that while the great modifying influence of the latter is the warm N.E. flow, popularly known as the Gulf Stream, the former are chiefly dominated by the Arctic Current, which brings down glacial *surface*-water from the coasts of Greenland and Labrador. The existence of a low *bottom*-temperature, wherever the basin is deep enough to admit the Arctic under-flow, is common to both: but while, on the European side—to take as an example what I have called the "Lightning" channel that lies N.E. and S.W. between the Orkney and Shetland Islands and the Faroes—the glacial under-flow from the N.E. is overlaid by a comparatively warm upper-flow from the S.W., on the American side the glacial under-flow from the N.E. is overlaid by a cold upper-flow from the same quarter, urged southwards by the prevalence of northerly winds along the Greenland and Labrador coasts. And alike in the upper and in the under south-moving strata is there a *westerly* tendency (caused by the *deficiency* of easterly momentum which they bring from latitudes higher than 60° into lower parallels) which causes them both to "hug the shore" along the whole coast-line not only of British North America, but of the United States. The superficial Arctic wind-current cannot be distinctly traced further south than New York; but none the less is there a band of cold water intervening between the coast-line of the Southern States and the Gulf Stream; and the *Challenger* soundings have distinctly shown the continuity of this band with the deep Polar under-flow which underlies the Gulf Stream, and surges up on the western slope of the Atlantic basin.

The course not only of the superficial Arctic current, but probably also that of the deep under-flow, is greatly modified by local conditions; that of the former chiefly by the strong tides and local winds of the coast, especially in estuaries, straits, or inlets; and that of the latter by variations in depth—the effect of a shallowing bottom being to bring the cold under-flow nearer to the surface. And thus, as Mr. Hind observes, the extraordinary variations which present themselves on the Dominion Coasts are specially worthy of study in their relation to Fish-life. No such peculiarity is more remarkable, than that which seems almost constant in the Strait of Belle Isle, separating the north end of Newfoundland from Labrador; for here, in the latitude of London, the sea has a glacial temperature all the year round. Pack-ice remains in these Straits through the early summer, with a comparatively high air-temperature; and they are never clear of bergs. Sometimes the surface freezes over again at Midsummer after the breaking up of the winter ice. In 1873 the surface-temperature of the sea in these Straits on four consecutive days in the latter part of June was found to range from 36° to 28°; the air-temperature during the same time ranging between 43° and 68°. The extremely little influence which this comparatively high air-temperature had upon the temperature of the surface water, clearly shows that the latter must be constantly kept down by melting ice, and also by the surging-up of the deep glacial underflow. Numerous cases are cited by Mr. Hind of the influence of winds and tides in lowering the surface-temperature by mixing the deep cold stratum with the superficial; the general rule being that easterly sea-winds generally raise the temperature of the surface-water, while westerly winds cool it. That such changes (as from 52° to 38° in a single day) have no relation to the temperature of the winds themselves, is clearly shown by comparative observations of the sea- and air-thermometers; the moist easterly sea-winds being generally colder (at least during summer) than the dry winds crossing from the land; while the influence of a shoaling bottom, lying in the course of the deep glacial flow, is shown by a sudden descent of the surface-temperature to 33°. So, again, a mixing of the different strata produced by currents along the shoaling waters of the Labrador coast, particularly among the islands, rapidly reduces the temperature; so that, in a cold calm after a storm in December, all the conditions are present for that formation of "anchor-ice," of which Mr. Hind gave an account in a former communication. "The sea on the shoals is uniformly cooled; a clear sky and a north wind assist the radiation of heat; and ice-spicules form with great rapidity in the Labrador current, often increased in local intensity by tides." It has been lately stated, on the authority of Prof. Mohn, as a fact well known to the Norwegian fishermen, that the deep water is often so cold that it freezes if disturbed, although it continues liquid so long as it remains perfectly still; fishes passing into such a glacial stratum being frozen, and coming to the surface as lumps of ice.

Mr. Hind draws attention to a remarkable series of observations of temperature and specific gravity, taken by Dr. Kelly, of Quebec, during the Admiralty Survey of the Gulf of St. Lawrence in 1830-36; which show that a very curious temperature-stratification exists in that vast estuary, obviously produced by the mixing of the great body of fresh water brought down by the river St. Lawrence with the cold Labrador current. A zone of water of a certain degree of warmth is superimposed upon a zone sometimes of lower and sometimes of higher temper-

ature; and these zones are cup- or saucer-shaped, having a general relation to the depth in different parts of the Gulf, and sometimes coming to the surface at variable distances from the coast. In every case, however, *the relative position of the zones is strictly accordant with their relative specific gravities;* the overlying of a warmer by a colder zone being due to the dilution of the latter by the admixture of fresh water, as appears from the following examples:—

	I.		II.		III.	
	Temp.	Sp. Gr.	Temp.	Sp. Gr.	Temp.	Sp. Gr.
Surface	54°	1·0225	43°	1·019	51°	1·0180
5 fathoms	—	—	—	—	42·5	—
10 ,,	46	—	37·5	1·023	38	—
20 ,,	—	—	39	1·0246	32·5	1·0261
30 ,,	34·5	—	—	—	33	1·0266
50 ,,	34	—	33	1·026	—	—
80 ,,	—	—	—	—	34	1·0266
100 ,,	37	1·0270	36	1·0275	35	1·0271
150 ,,	—	—	—	—	35	1·0278

A similar alternating temperature-stratification has been recently observed by the Norwegian Expedition in the seas between the coast of Norway and the Faroes; and I venture to predict that when the *temperatures* of the successive strata shall have been correlated with their respective *salinities* (which are modified by the admixture of fresh water discharged from the Norway fiords), the stratification will be found conformable to the same law of *the heaviest water lying deepest.*

There is one locality not far distant from our shores, in which similar influences have been found to produce equally decided, though less strongly-marked effects; I refer to the Baltic Straits, in which very careful observations of temperature and specific gravity have now been carried on for several years under the able direction of Dr. Meyer of Kiel, and his coadjutors. Here there is an admixture of waters from three different sources—the North Sea, the Baltic, and the underflow of glacial water which is brought as far south as the Skagerrack by a comparatively deep channel lying outside the Norway fiords. The North Sea brings water of ordinary salinity and of a temperature corresponding generally to that of the air: the Baltic outflow brings a variable quantity of water of low salinity: and the deep Norwegian channel brings water of very low temperature. In addition to these factors, there is the operation of winds and tides, which greatly modify the movements alike of the superficial and of the deeper strata. These influences are now so well understood, that, by a careful correlation of them, the temperature and salinity of the waters at the various observing-stations may be closely predicted; very small differences in specific gravity on the one hand, or small variations in level (and therefore in downward pressure) produced by winds and tides, being sufficient to determine movements in great masses of water, tending to the restoration of the disturbed equilibrium. In fact, as Dr. Meyer assured me during a recent visit to this country, "Your trough-experiment is being daily carried out on the great scale in the Baltic Straits, with the like results."

It is now well-established that the Temperature-stratification of the Sea has, as Mr. Hind says, *an all-important bearing* on the great fisheries:—"It determines the vertical positions in the sea, of the zones of minute and microscopic life which constitute the food of the higher forms, up to those of the fish which feed either directly or indirectly upon them." The cold of the Arctic seas is commonly supposed to be inimical to animal life; but hat the very contrary is the fact, is shown by the abundance of fish along those parts of the British North American coast, whose waters are most reduced in temperature by the Greenland and Labrador current, as compared with their paucity along the New England shores, which are less affected by that current. The most noteworthy case is that of the Strait of Belle Isle, in which, though almost every square mile has been annually fished for more than two centuries, continued productiveness is the rule through an average of years. And thus it becomes clear that the relative extent of the *cold-water areas* which feed (so to speak) the several fishing-grounds of the North American coast, must be a factor of the greatest importance in determining their respective values. Thus, while the water-area within the 100 fathoms' line along the coast of the United States north of Cape Hatteras does not exceed 45,000 miles, that of the British-American coasts within the same limit of depth exceeds 200,000 square miles. And while the former is bounded more or less closely by the heated water of the Gulf Stream, which invades it during the summer months by a swing towards the shore, the latter is only bordered by the Gulf Stream along its southern edge, and is continuous to the north and north-east with a limitless sea of cold water, which is the home of those minute forms of marine life that constitute—directly or indirectly—the source of our "commercial" fishes, the cod, herring, and mackerel.

Another advantage possessed by the fishing-grounds of British North America over those of the United States, is their immunity from the ravages of the *blue-fish*—a voracious wandering fish, whose home is in warm southern waters, its northward migration taking place only during summer, and never extending far beyond Cape Cod. Its destructive agency has had much to do with the diminished productiveness of the New England fisheries; and further south is specially exerted on the mackerel schools. According to the estimate of Prof. Baird, the United States Fishery Commissioner, the weight of fish consumed by the blue-fish of the United States coast during the season is about 300,000 *million* pounds. In its turn the blue-fish is largely consumed as an article of human food, being taken in great numbers along the coast of the Southern States; but it is not suited for salting, and is consequently of no value as an export fish. From the fishing-grounds in which the blue-fish is taken in immense quantities during the early winter months, for the supply of the northern markets, British American fishermen are excluded.

Of the influence which Temperature has now been ascertained to exert over the habits (especially the migrations) of these fishes, and consequently over the productiveness of the great "harvest of the sea" furnished by them, as to which a valuable mass of information has been brought together by Mr. Hind, I shall give some account in a future communication.

WILLIAM B. CARPENTER

THE MICROPHONE [1]

THE following experiments were suggested by the description, which appeared in a recent number of NATURE, of the microphone lately invented by Professor Hughes. Instead of the pointed piece of carbon supported between two pieces of the same material as used by him, it occurred to me that ordinary gas cinders would be likely to answer the purpose tolerably well. To test this, I included in the circuit of an ordinary Bell telephone, a single Leclanchè cell, and a small jelly can half filled with cinders broken into pretty coarse fragments. The connections were made by slipping down at opposite sides, between the cinders and the sides of the

[1] Abstract of a paper read before the Royal Society of Edinburgh on June 3, 1878, by James Blyth, M.A., F.R.S.E.

jar, two strips of tin, to which the circuit wires were attached. When the simple instrument was used as a transmitter, articulate sounds were heard very loud and distinct in the distant telephone, though occasionally marred by what appeared to be the rattling of the cinders in the jar. With this transmitter sounds were also quite audible, even when the speaker stood several yards away from it.

I next took a shallow box, made of thin wood, about fifteen inches by nine inches, and filled it with cinders, taking care, in the first place, to nail to the inside of its ends two pieces of tin to which wires could be attached. Having nailed down the thin lid of the box, and included it in the circuit of the telephone, along with one Leclanchè cell, I found that it made both a very sensitive microphone, as well as an excellent transmitter for the ordinary telephone. With three of these boxes hung up like pictures on the walls of a room, and connected in circuit, almost any kind of noise made in any part of the room was revealed in the telephone. Speaking was heard distinctly, and a part-song by two voices in the middle of the floor was rendered with surprising clearness and accuracy.

In my next experiment, still using the same cell in the circuit of the telephone, I tried as transmitter a single elongated cinder with the wires wound tightly round each end. Sounds uttered close to this cinder were quite audible, but I failed to hear them when I substituted for the cinder the carbon of a Bunsen cell with brass clamps firmly attached to each end, into which the circuit wires were screwed. Possibly either the more porous and friable nature of the cinder or the comparative looseness of the wire attachments, may have had something to do with this difference of effect.

I next removed the Leclanchè cell from the circuit and used as transmitter the jelly can containing dry cinders. I sometimes fancied that I heard sounds even with the cinders dry, but they became faintly, though distinctly, audible when the cinders became slightly moistened by the breath of the speaker. However, on pouring water into the jar, so as almost to cover the cinders, the sound was heard on the telephone almost as well as when the Leclanchè cell was in circuit. I did not, however, hear any sound with the cinders removed and water only in the jar, not even when the conducting power of the water was increased by being slightly acidulated.

In my next experiment I tried if the jar with the cinders would act as a receiver as well as transmitter, and was not a little surprised to find that it did so. For this purpose I used similar jelly cans, containing cinders both for transmitter and receiver, and included a battery of two Grove's cells in the circuit. Articulate sounds uttered in the one cinder jar were distinctly heard in the other, and even voices could be distinguished. However, the results were not so good as I have no doubt they will yet be, when better forms both of transmitter and receiver are adopted. Here we have the beginning of a kind of telephone worked entirely by the electric current without the aid of magnetism. I also tried successfully an ordinary telephone as transmitter and a cinder jar as receiver, but in this case, the sounds were somewhat fainter and not so easily distinguished. I remarked, also, that when an intermittent current was sent through the cinder jar, a very distinct rattling noise issued from it.

In order to find out if the cinders in the receiving jar were at all jostled about when sounds were being transmitted to it from a similar jar, the following experiment was tried. A strong battery was included in the circuit, and a clean glass jar containing cinders taken as receiving instrument. When this was taken into a dark room small flashes of electric light were observed here and there amongst the cinders while sounds were being sent.

JAMES BLYTH

RESTING SPORES

A VERY interesting memoir by Dr. Wittrock, the well-known Swedish botanist, was communicated to the Swedish Academy of Sciences in December last "On the Spore-Formation of the Mesocarpeæ and especially of the New Genus Gonatonema." The chief interest of this memoir seems to us to lie in its somewhat novel interpretation of some pretty well-known physiological facts, which we shall as briefly as possible proceed to enumerate.

In one lovely group of green-coloured algæ we find a number of very pretty species, many of which consist of one-celled forms, and others of which, obeying a law of cell growth, not only produce new cells but also cause these to adhere to one another and so, as this growth goes on, give a chain-like or filamentous appearance to the mass. These filamentous green freshwater algæ are very common. Dillwyn, in the beginning of this century, knew and described many of them, and he also seems to have well known that the contents of some of their cells formed oval bodies called resting spores. The merit of having worked out the history of these spores belongs to Prof. A. de Bary, from whose researches it was first made clear that in some of these forms (Zygnema) one of the chains of cells will come to lie alongside of another chain, and then the cell-wall of two opposite cells will grow outwards until they meet. On meeting the tips of these outgrowths will be absorbed, and the two cells will thus communicate by means of this newly-formed canal, whereupon it will follow that the contents of both cells will each go half way to meet the other, and their conjoining will take place in the newly-formed canal, or sometimes in one of the cells; or that the whole of the contents of one of the cells will pass over and combine themselves with the contents of the other. In either case the result will be the formation of a new body—well known as the zygospore, but also known under many other denominations. But, again, in other forms (Mesocarpus), while the initial process will be the same so far as the formation of the cross channel goes, the further steps differ much, it being only the green-coloured portions of the protoplasm of both cells that move over into the canal, whereupon the central portion of this green mass, composed of about equal parts of the contents of the two cells becomes developed into a zygospore, leaving the rest of the cell-contents to fade away. The physiological import of these two quite different phenomena was therefore this: in Zygnema and its allies the total contents of two of the cells were required to form a zygospore—whereas in Mesocarpus this was formed out of only portions of the cell-contents. There is thus no strict analogy between these two forms of zygospores, and they probably should not both receive the same name. De Bary perceiving this, referred to the one as resting-spores formed by the partition of the zygospore [the parts destitute of green contents having been partitioned off], strangely applying this term to that stage when the two cells had combined to form one, and to the other as resting-spores without partition. De Bary's attempt at being logical has apparently been overlooked by many writers on this subject, notably by such eminent investigators as Max Cornu and Sachs, who still apply the term zygospore to both forms, but Pringsheim has grappled with the difficulty in his most thoughtful paper "On the Alternation of Generation in Thallophytes," and suggests that the first stage in the reproductive process in Mesocarpus is the "conjugation"-stage—here the cells join and become, so far as their cell-walls are concerned, united into one. The next stage is the more important one, in which the cell-contents commingle, and the result is the production of the central cell—a carpospore—and of two or four cells which surround it, and form the equivalent of a fruit-like body, or sporocarp,

and of course it would make no matter whether this sporocarp were formed in the connecting canal as in Mesocarpus, or whether it fills this and extends over into both the cells as in Staurospermum, or as in Plagiospermum is altogether formed in one of the cells; the essential feature being the differentiation into the carpospore and its investing covering the sporocarp.

Now Dr. Wittrock has made the rather startling observation that in one and the same species (*Mougeotia calcarea*, Clev.), the formation of the spores may take place equally in the manner of the three above-mentioned genera; also that occasionally even the spores may be formed without any conjugation, and further that in a plant found growing last October in an aquatic stone house in the Upsala Botanical Gardens, and which is described as *Gonatonema ventricosum*, the spores are formed in a neutral way through the agency of cells never intended for and incapable of conjugation. Such spores the author calls agamospores, and he finds a second species of this new genus in Hassall's anomalous *Mesocarpus notabilis*. This memoir of Wittrock's will be found in the *Bihang till k. Svenska Vet. Akad. Handlingar*, Band 5, No. 5, 1878. It is written in English, and illustrated with a plate. Upon it the following observations may not seem altogether out of place.

If the interpretation placed on the phenomena to be witnessed in the Mesocarpeæ by Prof. Pringsheim be accepted, then this family can scarcely be left among the Conjugatæ, and this would hold true also of Wittrock's new genus, as indeed is so stated by himself. But may not the phenomena be interpreted in yet one other way? First, as to the agamospores in Gonatonema. Is it beyond the bounds of possibility that, despite their external likeness to zygospores, these are simply vegetative spores, to be compared to one of the so-called tetraspores in Florideæ? They surely cannot be compared to any form of organism itself the product of the commingling of the contents of two different cells! Another suggestion, to account for this agamospore, has been made to me by my friend William Archer. It is that there may have been a separation between the upper and lower portions of the protoplasmic contents of the same cell, and that these, without waiting for the formality of forming separate cells, may have then and there conjugated. This is certainly a most ingenious suggestion, and is strengthened by the well-known fact that, in some Desmids, after the single-celled frond has divided into two halves, and before the newer portions grow into anything like the similitude of the older portions, the two halves, which were only just parted, will conjugate and form an ordinary zygospore. De Bary gives some pretty figures of this strange phenomenon, which, according to Mr. Archer, might be carried one step further, and there be no parting at all. In favour of my own idea I can only add that the first origin of what, in some of the Florideæ, will form the tetraspores, and the origin of these agamospores, appear to me to be the same. Next as to the sporocarps in Mesocarpus. The differentiation into sexual entities of the protoplasmic contents of cells is confessedly, at first, scarcely perceptible. It would be impossible, in many cases, to say with any confidence, this one is the germ cell, and that one is the sperm cell. But gradually a differentiation appears in that the contents of the former exhibit themselves as passive, and of the latter as active; the contents of the one remain quiescent, those of the other pass over to conjugate with the former, but all through the contents that commingle are almost in every case alike in quantity. Carry the differentiation a step further on, and we find that the contents that commingle may be at first somewhat, and then be strikingly unlike in quantity. The passive contents will be divided into a comparatively small number of portions (in Fucus eight), but these each can be fertilised by the very smallest portion of the active contents. Now may not the Mesocarpeæ be a link between these groups? The contents of each of the two cells divides into certain portions. The fertilising power of the active contents is not sufficient for the passive contents, and hence but one portion—that the most specialised—is fertilised; this forms the zygospore; the other portions remain sterile. Then this spore would differ from the zygospore of Zygnema just in the same proportion as it would differ from the oospore of Fucus, but the fructification would not at all be a representative carpospore, and the at first sight very anomalous case of *M. calcarea* may be explained by supposing that the number of partitions is a matter of but secondary importance, unless the fertilising power of the active contents were to increase. This field of research is an important one, and much as we are indebted for information on these points to the labours of the Swedish botanists, we must still continue to look for fresh facts and new explanations.

E. PERCEVAL WRIGHT

PROF. C. F. HARTT[1]

CHARLES FREDERIC HARTT, whose death by yellow fever occurred at Rio de Janeiro on the 18th of last March, was born at Fredericton, New Brunswick, August 23, 1840. For three years and a half before his decease he had successfully withstood the fatigues of exploration and the labours of organising and carrying on the geological commission of Brazil, an undertaking beset with many trying difficulties.

Prof. Hartt's connection with natural history dates from boyhood. Encouraged by Prof. Cheesman, he made rapid progress in his favourite studies, without, however, neglecting the other branches of learning. But his particular bent always lay toward natural history, language, music, and art.

While a student at Acadia College, he undertook, under the direction of Dr. Dawson, extensive researches into the geology of Nova Scotia, which province he explored on foot from one end to the other. In 1860 he accompanied his father to St. John, there to establish a college high-school. This change of location brought him into another field for exploration, that of the geology of New Brunswick, and he commenced his new labours at once. The Devonian shales at the locality called Fern Ledges, in the vicinity of St. John, were the principal objects of his research. After a long siege of hard work he was amply repaid by discovering an abundance of land plants and insects, of which the latter still remain the oldest known to science. Prof. Agassiz was attracted by this last discovery of the young Canadian naturalist, and invited him to enter his museum at Cambridge as a student. This he did in 1861. Each vacation he returned, either to New Brunswick or Nova Scotia, to continue his explorations. In 1864 Mr. Hartt was employed, with Profs. Bailey and Matthews, on the geological survey of New Brunswick, and, while engaged in this work, obtained the first full proof of the existence of primordial strata in that province. Many of his discoveries in Nova Scotia and New Brunswick were published in the Provincial Government reports, and also in Dr. Dawson's "Acadian Geology."

Upon the organisation of the Thayer Expedition to Brazil, by Prof. Agassiz in 1865, he was appointed one of its geologists, and henceforth to the time of his death he was ever a most devoted investigator of South American natural history. Aided by New York friends he returned to Brazil alone in 1867, this time examining with the greatest care the reefs of the Abrolhos Islands, and those of the coast, as well as the geology of a part of Bahia and Sergipe. The results of his work thus far were pub-

[1] From an article by Mr. R. Rathbun in the *Popular Science Monthly* for June.

lished in 1870 as the "Geology and Physical Geography of Brazil." In addition to the account of Hartt's researches, it included the best results of all who had ever published on the geology of the country.

Early in 1868 he was elected Professor of Natural History in Vassar College, and shortly after of Geology in Cornell University. In 1870, with Prof. Prentice and eleven students of Cornell University, he again went to Brazil. He entered the Amazonian Valley, hoping there to discover, at the falls of the different tributaries of the Amazonas, other fossiliferous formations than the cretaceous, which latter alone he had found along the coast. He was well rewarded, and returned to the United States with large collections of fossils of the palæozoic age, and sufficient other evidence to allow of his giving us a very accurate though general idea of the formation of the Amazonian Valley. His results were strongly opposed to the theory of Prof. Agassiz, of its glacial origin. He returned again to the Amazonas in 1871 with Mr. O. A. Derby. Together they carefully re-explored the same regions gone over before, adding much to the stores already brought to the United States, and also examining the ancient Indian mounds and shell-heaps of numerous localities.

Returning from Brazil once more he remained at Cornell University about three years, quietly working up the results of his later trips, and publishing his reports upon them. In August of 1874, by request of the Brazilian Minister of Agriculture, he went to Rio de Janeiro to submit his plans for the organisation of a Geological Commission of Brazil. He entered on his work in May, 1875, with five or six assistants.

On the reorganisation of the National Museum at Rio, in 1876, Hartt became Director of its Department of Geology, but on account of his many other duties he was soon obliged to resign that position. The results of his researches may be briefly summed up as follows:—Before he went to Brazil on his second trip, in 1867, scarcely anything was known of fossiliferous deposits there, and thus no material existed toward the study of the systematic geology of the country. A few cretaceous fossils had been recorded from Bahia; the Danish naturalist Lund had very fully described the bone-caverns of Lagoa Santa in Minas Gerães, and we knew of coal-plants from Rio Grande do Sul, but beyond this the palæontology of Brazil was a perfect blank. Hartt's greatest achievement in Brazil was probably his solution of the structure of the Amazonian Valley. It was founded on the best of palæontological evidence which proves the existence of an immense palæozoic basin lying between the metamorphic plateau of Guiana on the north, and that of Central Brazil on the south, and through which flows the river Amazonas. Silurian, Devonian, and carboniferous rocks, make up the series in regular succession, and in many localities are highly fossiliferous. He has explained the character of the isolated cretaceous deposits, mostly discovered by himself, existing along the coast from Pará to Bahia, and of the carboniferous and other regions south of Rio. He has shown us the manner in which the rocky structure of Brazil was built up, and has done much toward solving the relations of the crystalline rocks which compose by far the larger portion of its surface. He has explored the shell-heaps, burial-mounds, and other relic-localities of the prehistoric tribes from far up the Amazonas to the southernmost coast province. We owe to him also the first real satisfactory explanation of the reefs of Brazil, which he distinctly shows to be of two kinds—sandstone and coral. He spent much time in studying the customs and languages of the modern Indian tribes of the Amazonas and Bahia, and collected very much material toward a grammar and dictionary of the Tupé Indian language in several of its dialects. But to attempt a complete account of Prof. Hartt's Brazilian explorations and discoveries would require a longer article than we can give here. In connection with the Geological Commission of Brazil he founded a large museum in Rio de Janeiro, which will always bear testimony to his great final undertaking. It forms the most complete repository of South American geology in the world.

A start had just been made toward publishing the reports of the commission when the death of Prof. Hartt deprived it of its main support. But though this will occasion some delay in the publication, it is to be hoped that we shall soon have before us the entire results of this most important of explorations.

Prof. Hartt's published works are not very voluminous. He was so confident of a longer life that he delayed too long, but still he was a constant contributor to American scientific periodicals.

THE DARK CONTINENT[1]

IN our article last week on "Old Maps of Africa" we said that even if it were the case that the great lakes and rivers of Central Africa were known to early Portuguese missionaries and traders, it would not in the least detract from the glory of modern African explorers. Even if the work of those early travellers had not been clean forgotten, it was done so imperfectly that in any case it would have had to be done over again; their work bears about the same relation to that of modern explorers that the observations of an ancient Chaldean shepherd watching with powerless eyes the march of the stars, while he tended his flock on the hill-side, do to those of a modern astronomer armed with all the instruments of an observatory. It scarcely needs a perusal of these two volumes to convince us that it would be simply absurd to attempt to deprive Mr. Stanley of the glory of being the first white man whose keel has cleaved the broad bosom of the Upper Congo. He has done *his* work in such a way that there is no chance of it being ever forgotten.

Let us at once assure those of our readers who may cherish the idea that, after having read Mr. Stanley's letters in the *Telegraph*, they need not trouble themselves with his book, that they labour under a delusion; compared with the book, the letters are a mere prospectus, and therefore we cannot hope within the limits of an article to give any adequate idea of its contents. From a merely literary stand-point, Mr. Stanley's work deserves to take a high rank. We know no other narrative of travel with which it can be compared; it reads more like a prose epic than a story of stern facts, and the reader who remembers his classics will be over and over again reminded of the story of the wanderings of Ulysses as chanted by Homer. No such revelation of African life and African character and African scenery has ever been made, scarcely, we think, even in the half-fictitious pages of Winwood Reade. The trustworthiness of Mr. Stanley's narrative cannot for a moment be doubted; his art has been evidently used simply to enable us to realise with perfect clearness the scenes and events through which he and his followers passed.

From the numerous notices we have published, our readers must be familiar with the great outlines of Mr. Stanley's discoveries. The two volumes before us are concerned mainly with the incidents of the wanderings of himself and followers from Bagamoyo to the mouth of the Congo; another volume, which is promised for autumn, will contain chapters on the hydrography, ethnology, and natural history of Central Africa, with "considerations" on the lakes, lands, and peoples of the equatorial regions; as well as chapters on the hydrography and physical geography of the western half of

[1] "Through the Dark Continent, or the Sources of the Nile, around the Great Lakes of Equatorial Africa, and down the Livingstone River to the Atlantic Ocean." By Henry M. Stanley. Two vols. Maps and Illustrations. (London: Sampson Low and Co., 1878.)

Africa, with special reference to the Livingstone Basin and River, and the volcanic formation of the defile through which the Livingstone falls into the Atlantic. Until the publication of this third volume it would be premature to discuss in detail the scientific results of the expedition, and we shall therefore at present content ourselves with briefly resuming the general results of Mr. Stanley's work.

Mr. Stanley left Bagamoyo on November 17, 1874, with a force of porters, soldiers, and boatmen of about 350. The expedition was thoroughly equipped for its work, and it is evident that the best possible use was made by Mr. Stanley of all his advantages. The objects of the expedition were not rigidly defined, and generally they may be said to have been to clear up the many unsolved problems relating to the sources of the Nile, the great

Scene on Lake Tanganika.

lakes of Central Africa, and the course of the great river, which, coming from the far south, passed Nyangwé, and flowed then no man knew whither. The ultimate source of the Nile was unknown; the configuration of the Victoria Nyanza was so uncertain, and so many objections had been raised to Speke's work there, that, as Mr. Stanley says, there was some danger of its being swept off the map entirely; so defective was our knowledge of the lake, that some geographers, including the sagacious Livingstone, maintained that it was not one lake but many; there was much to clear up in the region between Victoria and Albert Nyanza, and our knowledge of the latter was of the scantiest. The great western affluent of Lake Victoria, the Kitangulé, had to be traced, and our knowledge of Lakes Windermere and Akanyaru extended, as well as of the stretch between the latter and Tanganika.

On the last-named lake, notwithstanding the labours of Burton and Speke, Livingstone, and Stanley himself, and even of Cameron, there was not a little to do to complete our knowledge. Then there was much room for additional work in the interesting country lying between Tanganika and the Lualaba at Nyangwé, where Livingstone has left an everlasting memory as "the good old white man." Last of all there was the mile-wide Lualaba itself sweeping past Nyangwé, "north, north, north," into the great unknown, perhaps to the Nile, perhaps to some great lake, perhaps bending west to the Atlantic; though there could be little reasonable doubt that if a boat could run the gauntlet of the natives, it would find itself ultimately on the estuary of the Congo. These were the geographical problems to be solved, and Mr. Stanley solved them, and he only took two years and a half to do it.

Until he reached Ugogo, nearly half way between Bagamoyo and Lake Tanganika, Mr. Stanley kept not far from the caravan route westwards, well known to all readers of recent African travel. Turning suddenly northwards, he made straight for the Victoria Nyanza, over a rugged table-land, interspersed with plains, and with at least one wide desert, and many villages. In about $5\frac{1}{2}°$ S. lat. he came upon some tiny streams which he ultimately found to be the head waters of a river of something like 300 miles long, that runs into Lake Victoria as the Shimeeyu, and which is undoubtedly the furthest south source of the Nile. Camping at Kagehyi, on Speke Gulf, Mr. Stanley in his trim boat,

Cataract on Lower Livingstone.

the *Lady Alice*, circumnavigated the Victoria Nyanza, defining every creek and gulf, and proving it to be one great lake with an area of 21,500 square miles, an altitude of 4,168 feet, and with border-soundings of from 330 to 580 feet. The lake is bordered with islands all the way round, is much indented with creeks and bays, receives numerous tributaries from all sides, and its shores and many of its islands are thickly inhabited.

Mr. Stanley next set himself to the task of doing for the Albert what he had done for the Victoria Nyanza, but in this he was baulked by the timidity of the escort furnished him by his warm friend Mtesa, King of Uganda, on the north of the latter lake. He was only able to stand on the precipitous shore of what he named Beatrice Gulf. From what he then saw, combined with the information gleaned at the court of King Rumanika, he has plotted on the map accompanying his work the vague outlines of a new lake, to which he attaches the name of Muta Nzigé, somewhat to the south of the Albert Nyanza. The latter he locates in accordance with the recent circumnavigation of Col. Mason, with the proviso that after all there may be a connection between the two. If Mr. Stanley has not yet solved this problem, he has at least opened up a very interesting one, which possibly the Egyptian pioneers may unravel. Coming south to the coast of Ruminika, King of Karagwé, the gentle friend of Speke and Grant, and now of Stanley, he was able still further to add to our knowledge of a region teeming with interest, and again to open up problems which successive explorers must solve. We have now some idea of the great affluent of the Victoria Nyanza, which, issuing from Lake Akanparu, flows north through a long series of swampy lakes before it turns east to feed the great reservoir of the Nile. About Lake Akanyaru itself we know but little. Mr. Stanley, in the maps which accompany his work, no longer makes a long river flow from the west to feed it, though a considerable stream comes south from the Ufumbiro Mountains.

Still, however, he gives it a connection with little Lake Rivu, the supposed source of the Rusizi, the northern affluent of Lake Tanganika. Here is another curious riddle awaiting solution.

Coming to the Tanganika itself, we may say that Mr. Stanley has virtually completed our knowledge of its configuration, having for the first time defined the outline of its southern shore and proved that the Lukuga has not yet become an effluent, but promises ere many years to carry the waters of the lake to swell the volume of the Lualaba. Mr. Stanley adduces the strongest proofs that the Tanganika is rising with comparative rapidity, and it is possible that further research may show that the earliest Portuguese explorers, if ever they reached it, found two lakes on its site, divided by a ridge nearly half-way between its north and south points. Mr. Stanley, before he began his work of exploration, evidently used great diligence to qualify himself as an observer in geology and natural history. That he is a keen observer his work shows, and it is evident he has collected a mass of data in geology and natural history, as well as in ethnology, which will prove of the greatest interest to men of science, and which we may look for in the promised third volume. Evidently the geological conditions of the bed and shores of the Tanganika, as well as of the whole basin of the Livingstone, are unusually interesting and have occupied much of the explorer's attention. Until we have the whole of the data it would be premature to theorise. The lake Mr. Stanley makes out to be 329 miles long, with an area of 9,240 square miles. With 1,280 feet of cord he could find no bottom. Yet though the Tanganika is rising, Mr. Stanley seems to be of opinion that at one period nearly the whole of the great area drained by the Livingstone was under water, and that the numerous lakes to the west and south-west of Tanganika, with the river itself, are all that now remain of the great inland sea, if inland it was. On the banks of the Tanganika itself, high above the lake-level, he found rocks bearing distinct evidence of having been worn and rounded by water. Here is a splendid field for the enterprising geologist.

Of the great river itself, what more can we say but that, in the face of the most stupendous difficulties, he traced its course from Nyangwe to the sea? It is a splendid epic, this narrative of the expedition down this great river, whose banks are lined with the villages of hostile and cannibalistic natives, who literally hunted the little band for hundreds of miles. We doubt much if another man could be found who could have carried such an enterprise through with success. Anyone but Stanley would either have turned or been eaten ere the first cataracts were reached. One village street was fringed on each side with rows of skulls, which he was told were those of the soko—probably a species of chimpanzee. One of these, brought home by Mr. Stanley, was submitted to Prof. Huxley, who has diagnosed it as that of a human being. Of the dimensions of this river we have already spoken, and of its basin, of nearly a million square miles. Its discovery was worth all the sacrifices that were made; and, unless we are to count the pursuit of knowledge as an object of no worth, it must be admitted that Mr. Stanley has here done a thing that entitles him to rank in the first order of the pioneers of science. Apart from its high value as an addition to geographical knowledge, its importance as a highway to new fields of commercial enterprise cannot be overrated. North and south of it yet there are great white spaces to be filled up, but with such a magnificent base-line that should not be difficult to do.

Such is a brief outline of the principal geographical discoveries made by Mr. Stanley; but it gives the very faintest idea of what the reader will find in his book. Africa and African, to those who study these volumes, will be no longer mere names: the genius of Mr. Stanley has infused into them the breath of life. Mr. Stanley's strong human sympathy, aided by his knowledge of the Kiswaheli, has enabled him to bring before us the natives of Central Africa with a dramatic vividness never before attained. Henceforth it will be inexcusable to lump together the Waswaheli, the Wagoro, the Waganda, the Wanyamwesi, the Wajiji, the people of Manyema, and the many other tribes that people this much-watered land, as mere uniformly characterless "niggers." In Mr. Stanley's pages we see these various states and many individuals, each with their distinctive characteristics. The *physique* of the various peoples, their manners, their houses, utensils, and weapons, their dress, their modes of life, and even their modes of thinking and speaking, their legends, are pictured for us by pen and camera and pencil in a manner that must impress the laziest reader. The ethnologists will be able to glean many facts and hints here, and still more we should think from the

Kiteté, Chief of Mpungu, near the Lualaba.

volume that is to follow. Mr. Stanley presents us with a remarkable legend from Uganda, the Kingdom of Mtesa, concerning a blameless priest named Kintu and his descendants, which is well worth the study of the comparative mythologist. We have another strange legend as to the origin of the Tanganika, and we should think that in his wanderings much material of a similar kind must have been collected by Mr. Stanley: if so he would do science a service by publishing it. The chapters devoted to Mtesa and his kingdom are of special interest, and the explorer's friendship with this remarkable potentate promises to be fruitful of results. Further interesting details are given as of the mysterious white people of Mt. Gambaragara on the east shore of Muta Nzigé, which must rouse the curiosity of ethnologists. We learn a good deal also about the wandering Watuta, the terror of Central Africa, and of King Mirambo, a sort of African Napoleon, whom, however, Mr. Stanley speaks of in high terms as superior both in character and intellect to the general run of African "kings." Much new information also have we on the

inhabitants of the Tanganika shores and the artistic people of Manyema, with their elaborately coiffeured heads. To speak of these people, and even many of the tribes on the banks of the Livingstone, as savages is a misuse of language. People who can build houses and organize villages and towns such as they do, who can work their native iron, ivory, wood, and bone, into all sorts of artistic and useful shapes, and who can reason and speak as Mr. Stanley shows us they do, have raised themselves to a level considerably higher than the savage. West of Tanganika, especially, the tribes seem very much mixed up, and there are many evidences that the Livingstone with the neighbouring region is a sort of borderland where several races meet, and where a constant struggle is going on. What can be made of these Africans under competent direction, Mr. Stanley himself has shown us in the case of his own people.

Of the various products, mineral, vegetable, and animal, of the country through which Mr. Stanley passed we have many glimpses. The natural wealth of the country is extravagant, and the botanist especially will find much that will interest him, especially as Mr. Stanley has been at the trouble of frequently giving the scientific names of the plants which he mentions.

There is ample furniture of maps in the work, all of them well-executed, though in Mr. Waller's two large maps there are occasional signs of carelessness in the spelling of names, and, very strangely, the memorable Vacovia of Sir Samuel Baker is omitted from the names on the east shore of Albert Nyanza. Beside the two large maps of East and West Equatorial Africa, by Mr. Waller, there are also an interesting series of five maps by the same hand, showing the progress of discovery in Equatorial Africa. There is, first, a portion of Dapper's map of 1676, very similar to that of 1701, which we gave last week, showing two great central lakes, from the most westerly of which, Zaire lacus, issue both the Nile and Congo. The next one shows our knowledge between 1849-56, with all the features of Dapper's maps swept away, and the first rude indication of Tanganika given. Then, between 1856-63, we have the work of Livingstone, Burton, Speke, Grant, enabling us to more correctly define Tanganika, locate Victoria Nyanza, and shadow out Albert Nyanza. The next stage, 1866-75, shows a great advance. By the labours of Schweinfurth, Baker, Livingstone, Stanley (first journey), and Cameron, the main features, from 10° N. to 15° S., east of 25° E. long. —rivers, lakes, and mountains—are filled in more or less accurately. Last of all come the results of the journey described in these two volumes, and which we have endeavoured to summarise in this notice. There is also a chart of the Lukuga creek, and two beautiful large-scale charts, by Stanford, of the Livingstone or lower falls (thirty-two in number), and of the upper or Stanley falls. Mr. Cooper has, as usual, done his part well in reproducing the numerous and varied illustrations; and altogether the get-up is thoroughly creditable to the publisher.

In conclusion, let us repeat that Mr. Stanley has done a great work, and told us all about it in a great book.

OUR ASTRONOMICAL COLUMN

THE TRANSIT OF MERCURY, 1868, NOVEMBER 4.—The second internal contact at this transit was well observed in many European observatories, though at others the bad definition and tremulousness of the sun's limb vitiated the results. If we calculate strictly from Leverrier's tables of sun and planet, with Prof. Newcomb's value of the mean solar parallax, 8″·848, we shall have the following formula for reduction of the observed Greenwich mean time at any place to the centre of the earth:—

$$t = 20\text{h. } 59\text{m. } 51{\cdot}9\text{s.} + [1{\cdot}4056]\, r \sin l - [1{\cdot}7832]\, r \cos l, \cos(L + 55° 51'{\cdot}5),$$

where l is the geocentric latitude, r the radius of earth at the place, and L the east longitude from Greenwich.

A comparison with observations shows differences as below:—

Place of Observation.	Observed G.M.T. reduced to earth's centre. h. m. s.	Error of the Calculation. s.	
Bonn	21 0 3·4	− 11·5	Three observers: extremes differ, 13·5s.
Christiania	— 6·3	− 14·4	Four observers
Durham	— 12·2	− 20·3	
Greenwich	— 6·9	− 15·0	Six observers.
Leyton	— 12·6	− 20·7	
Lund	— 14·4	− 22·5	
Madrid	— 13·8	− 21·9	Merino. Rupture of ring.
Marseilles	20 59 57·6	− 5·7	Leverrier.
,,	21 0 12·6	− 20·7	Stephan.
Paris	— 7·6	− 15·7	Mean of André, Villarceau, and Wolff.
,,	20 59 57·0	− 5·1	Rayet.
Rome	21 0 10·4	− 18·5	Secchi and Mancini.
Vienna	20 59 55·5	− 3·6	Oppolzer.

At the Royal Observatory, Cape of Good Hope, where the transit was very completely observed, the sun's limb is stated to have been tremulous at the second internal contact, which probably accounts for the larger difference, −32·2s., between calculation and observation.

BRORSEN'S COMET OF SHORT PERIOD.—When the elements of this comet, at its first appearance in 1846, had been satisfactorily determined, it was pointed out by Mr. Hind, in a communication to the Royal Astronomical Society, that the comet must have made a very close approach to the planet Jupiter about May 20, 1842, and that probably to this near approximation the form of the orbit in 1846 might be attributed. The late Prof. D'Arrest examined this question more closely in the year 1857, and by the formulæ of the *Mécanique Céleste*, which had been already applied in the case of Lexell's comet of 1770, he ascertained that a great change of elements was then caused by the action of Jupiter, assuming the mean motion given by the observations of 1846 to be affected with no material error, as we now know to have been the case. He found that the greatest proximity occurred May 20·69, Berlin time, when the distance of the comet from Jupiter was only 0·0511 of the earth's mean distance from the sun, and that previous to April 19, 1842, the elements of the comet's orbit were as follows. The elements of 1846 are added for comparison:—

	Elements before the great perturbation. ° ′		Elements in 1846. ° ′
Mean longitude, 1842, April 19·5...	237 16	...	—
Longitude of perihelion	133 27	...	116 28
,, ascending node ...	107 44	...	102 40
Inclination to ecliptic	40 51	...	30 57
Eccentricity	0·59275	...	0·79386
Semi-axis major	3·68645	...	3·15352

These figures prior to 1842 are necessarily only a first approximation to the orbit then described, but they sufficiently explain the circumstance of the comet not having been observed before that year, since the perihelion distance was then greater than 1·5, and as Prof. D'Arrest remarked, under this condition Brorsen's comet would hardly be observable.

According to Dr. Schulz's elements for 1873, when the comet was last visible, the nearest approach of its orbit to that of Jupiter now takes place in 283° 30′, when the distance is 0·124, and thirteen revolutions of the comet are almost exactly equal to six revolutions of Jupiter. D'Arrest, from a rough calculation, considered that the orbit might again undergo great or complete change from the action of this planet in the year 1937. The only

other planet which the comet can approach with its actual elements is Venus, which, near the ascending node, may be within 0·11.

MIRA CETI.—According to Schönfeld's calculation the next *minimum* of this variable will occur on June 23. and the next *maximum* on October 11. There are comparatively few observations of the former phase and more attention to it is desirable. At present it is assumed that the perturbations of the maximum deduced from Argelander's formula, apply also to the nearest minimum. In this case the sum of the perturbations is +29·9 days.

GEOGRAPHICAL NOTES

WE last week referred to the important work done by Sir Andrew Scott Waugh in connection with the Great Trigonometrical Survey of India, and from the recently issued Report of Colonel Walker, the present Superintendent of the Survey, it will be seen that the work is being carried on with unabated energy. The Report refers to 1876-77, and tells us that during that year an area of 5,019 square miles was covered by principal triangulation; under secondary triangulation 5,400 square miles have been covered with points for the topographical survey, 3,100 miles have been operated in *pari passu* with the principal triangulation, and in an area of 23,600 square miles, lying mostly in portions of the Himalayas which are inaccessible to Europeans, a number of points have been fixed which will be valuable for geographical rectifications. The topography of upwards of 5,000 square miles has been completed in scales varying from half an inch to two inches, while several important geodetic operations were accomplished. In these Reports there is generally some important geographical work to record, accomplished by one of the native officials of the Survey. During the year 1876, the Mullah, one of the Survey explorers, made a survey up the course of the Indus from the point where it enters the plains above Attok, to the point where it is joined by the river of Gilghit. This is the only portion of the Indus which had remained unexplored. Here the river traverses a distance of some 220 miles, descending from a height of about 5,000 feet to that of 1,200 feet above sea-level. Its way winds tortuously through great mountain ranges, whose peaks are rarely less than 15,000 feet in height, and culminate in the Nanga Parbat, the well-known mountain, whose height, 26,620 feet, is only exceeded by a very few of the great peaks of the Himalayas. The river in many places is hemmed in so closely by these great ranges that its valley is but a deep-cut, narrow gorge, and, as a rule, there is more of open space and culturable land in the lateral valleys, nestling between the spurs of the surrounding ranges, than in the principal valley itself. No European has ever penetrated this region, and the Mullah only managed it by travelling as a privileged trader. Very difficult of access from all quarters, it is inhabited by a number of hill tribes, independent and suspicious of each other, and protected from each other by natural barriers and fastnesses. Each community elects its own rulers, and has little intercourse with its neighbours, and with the outer world only by means of privileged traders.

THE captain of a German steamer, just arrived at Hongkong, reports a singular condition of things in the island of New Britain, in the South Seas. He found the whole of the north-east coast enveloped in dense smoke, and he experienced great difficulty in proceeding up the channel between it and New Ireland, as fields of pumice-stone, several feet in thickness, covered the surface of the water. On February 9 he reached Makada, Duke of York group, and found that three craters had broken out in the New Britain peninsula, at the foot of the so-called Mother and Daughters Mountains, from which dense masses of pumice-stone were continually being thrown up. The passage between Duke of York Island and Blanche Bay had been completely closed by a compact field of pumice-stone, about five feet in thickness, according to the statement of the captain to a Hongkong paper. A tidal wave swept over Blanche Bay on February 10, and soon afterwards a new island appeared, about three-quarters of a mile in diameter. This island is situated to the south of Natopi, or Henderson Island, and where it now is no bottom was previously obtained at seventeen fathoms. It is probable that other alterations have taken place which could not be observed at the time, owing to the masses of floating pumice-stone. The captain of the vessel mentioned further states that the water in Blanche Bay was scalding hot for two days, and that immense quantities of boiled fish and turtle were thrown on shore, and eagerly devoured by the natives, who were starving in consequence of the unusual dryness of the season.

THE party which left England last month for Egypt on their way to reinforce the Church Missionary Society's expedition to the Victoria Nyanza, will proceed by steamer to Suakim, the port of Southern Egypt, accompanied probably by a dragoman engaged by the British Consulate at Cairo. At Suakim it is proposed that they shall engage camels to transport them across the desert to Berber, on the Nile, whence they will travel by steamer to Khartum. From that point they will journey under Col. Gordon's protection, and will, doubtless, have no difficulty in reaching Gondokoro. Thence it is arranged that they shall proceed by the Egyptian military outposts to the frontiers of Uganda, in which country Col. Gordon now has an agent, whose presence will no doubt insure safety to Europeans.

A LETTER from the French Ogowé Expedition was read at the last meeting of the Geographical Society of Paris. It is quite a year since it was written, and some apprehensions have been entertained as to the safety of the explorers. M. de Brazza states that the Ogowé is reduced to small proportions and flows from the south, so that it gives the impression of being really an arm detached from the Congo. The expedition was to travel northwards in order to examine the sources of a powerful affluent. Illness was prevailing amongst the small party, and the hostility of the native tribes was growing stronger.

THE forthcoming congress of the Geographical Society of Paris will not be international, but national, although it will be open to foreigners. The principal aim of the congress will be to organise a federation, between the Paris Society and similar institutions which its influence has started in large provincial cities during the past five years—viz., Lyons, Bordeaux, Marseilles, and Montpelier, where a society for the whole of Languedoc was recently established.

A REUTER'S telegram states that the schooner *Eothen* will probably sail from New York on Monday next for the Arctic regions to search for relics of the Franklin expedition. No doubt the purpose of this expedition is to obtain the relics reported to be in the possession of some of the mainland Eskimo.

A MEETING of the subscribers to the African Exploration Fund of the Royal Geographical Society will be held in the theatre of London University at 3 P.M. on Friday, June 14. Sir Rutherford Alcock, K.C.B., Chairman of the Committee, will preside.

ON A NEW METHOD FOR DISCOVERING AND MEASURING ÆOLOTROPY OF ELECTRIC RESISTANCE PRODUCED BY ÆOLOTROPIC STRESS IN A SOLID[1]

TORSION of a metal tube within its limits of elasticity produces æolotropic stress, of which the mutually perpendicular lines of maximum extension and maximum

[1] Abstract of a Paper read by Sir W. Thomson at the Physical Society, May 25.

contraction are spirals, each very nearly at 45° to the length of the tube.

From the author's early experiments (described in his paper on Electrodynamic Qualities of Metals, published in the Transactions of the Royal Society for 1856), showing a diminution of electric conductivity by pulling force in metallic wires, and Mr. Tomlinson's recent confirmations and extensions of those results, it is to be expected that the conductivity of the substance will be less in the direction of extension and greater in the direction of contraction in the stressed substance than the conductivity (equal in all directions) of the substance when free from stress. Hence, if an electric current be maintained along a tube, torsion would cause it to flow in spiral stream-lines, with spirality of opposite name to that of the twist. The whole flow may be resolved into two components: one right along the tube, the other round it. The latter would (like the current through a galvanometer-coil) deflect a needle hung in the interior of the tube with its axis perpendicular to the tube when undisturbed. Or it would magnetise a bar or wire of soft iron placed within the tube. The current itself would (except near the end of the tube) produce no external effect directly; but either of those appliances may be used to give an external indication.

Since the last meeting of the Physical Society, when the author raised this question of the spiral electric stream lines in a twisted tube, experiments have been made for him by Mr. Macfarlane in the physical laboratory of the University of Glasgow, on the last-mentioned plan; and on the former plan by Mr. J. T. Bottomley in the physical laboratory of King's College, London, by kind permission of Prof. Adams, and with the valuable assistance of his staff. Mr. Macfarlane, using a small mirror magnetometer suspended externally in the neighbourhood of one end of an iron wire placed within a brass tube, found that when the twist of the substance was right-handed the end of the wire next that end of the tube by which the current enters becomes a true north pole. Mr. Bottomley, with the cell and suspended mirror and needle of an ordinary dead-beat mirror galvanometer supported by an independent support within a brass tube along which a current is maintained, found that the true north pole of the needle is moved towards the end of the tube by which the current enters. Thus both Mr. Macfarlane's and Mr. Bottomley's observations confirm the anticipation that the electric conductivity is least in the direction of greatest extension, and greatest in the direction of greatest contraction of the metal. The apparatus by which Mr. Bottomley had made his experiment was exhibited to the meeting. It included a mode of balancing the effect on the internal needle by placing a circular portion of the main circuit at a proper distance from it, the centre and plane of the circle being in and perpendicular to the axis of the tube. From a measurement of the distance from the centre of the circle to the needle, when the balance is obtained, the ratio of the maximum to the minimum conductivity can be calculated.

NOTES

We publish a remarkable paper this week, by Mr. J. Blyth, on a new form of the microphone, which needs neither battery nor telephone. The curious importance of Mr. Blyth's invention need not be insisted on. By backing-up the pictures in a drawing-room it might, as has been suggested by a learned professor, be converted into an Ear of Donysius, and Horace's words, "suppositos cineri doloso," would come to have an awful meaning.

At the Royal Society on Thursday last, all those proposed as new Fellows, and whose names we have already published, were unanimously elected.

Prof. Simon Newcomb has published as a Supplement to the "American Ephemeris and Nautical Almanac," a few instructions for the observation of the total eclipse of the sun on July 29. The instructions refer to the limits of path of the shadow, instruments, arrangements for observation, the actual observation, search for intra-mercurial planets, drawings of the corona, &c. Prof. Newcomb's paper is accompanied by a series of photolithographic maps of the part of the United States concerned. They include a region extended about 150 miles on each side of the limits of totality. These maps contain certain special features which will render them very useful to observers. For example, on the right-hand side of the track is found, at convenient intervals, the Washington mean time at which the centre of the total phase reaches the several points, where the times are marked. From each of these points a dotted line extends across the path of totality; this line is the projection of a diameter of the shadow at the time indicated, so that the middle of the total phase occurs at this time all along the dotted line. These, and other features, render these maps of special utility.

A letter has been received in London by Dr. George Bennett, of Sydney, from Signor L. M. D'Albertis, the New Guinea traveller, dated from Sydney, New South Wales, April 14, 1878, in which he says, "I have taken my passage in the *Garonne*, which leaves direct for London on the 1st of May next, and expect to arrive in London about June 15, when I hope to see you. It is my intention to bring all my ethnological and other collections of natural history with me."

A laboratory for the study of marine zoology, in connection with the biological department of the Johns Hopkins University, will be organised this summer at Fort Wool, about a mile from Old Point Comfort, Va. The fort contains commodious buildings for laboratories and dormitories. The necessary apparatus for collecting and studying marine animals, nets, dredges, microscopes, reagents, aquaria, tables, &c., as well as a small scientific library, will be provided by the University. Through the kindness of the Maryland Commissioner of Fish and Fisheries, the boats used by the Commission will be at the service of the laboratory. The laboratory is organised mainly with a view to the wants of advanced scientific investigators, and there will be no formal courses of lectures. There will, however, be accommodation for a few less advanced students, and suitable instruction will be furnished to meet their individual needs.

Botanists will be gratified to learn that the publication by Prof. Asa Gray of his great work upon the "Flora of North America" has been commenced, and will be continued as rapidly as practicable. Many years ago a work with the same title was started by Drs. Torrey and Gray and carried through the Compositæ, where it stopped. The vast extension of the field of American botany, consequent upon the discoveries in California, Oregon, and other regions west of the Mississippi, has made the want of a manual extremely imperative, and this will be furnished by the work referred to. For the purpose of better satisfying the wants of students the work begins with the Gamopetalæ immediately following the Compositæ, and the ground covered by the original "Flora" will be taken up after the other orders are completed. The entire work will consist of two volumes of about 1,200 pages each, the first part, now issued, embracing about 400 pages. The book is to be had from the curator of the Harvard Herbarium at Cambridge.

The Japanese Government, which is making such rapid strides towards modern civilisation, has just awakened to the necessity of preserving its forests, and stringent regulations have been passed, which shall not only hinder the too rapid destruction of the forests, but increase the area covered by woodlands.

A VERY interesting paper was recently read before the Asiatic Society of Japan, by a native of that empire, in which the records of the earthquakes in that insular region for the past fifteen centuries were carefully compiled and classified. It appears that since the year 406 A.D. the authorities of Yeddo and other large cities have preserved, almost without interruption to the present time, descriptions of all earthquakes occurring, with their accompanying phenomena. As a rule it has been observed that the great shocks were preceded by a rise of temperature and violent atmospheric perturbations. The general average of great earthquakes has been ten in the century. The average of the present century is, however, double that number, and in the ninth century there were no less than twenty-eight destructive earthquakes. The list referred to describes a total of 150 great earthquakes during the past fifteen centuries, and a host of minor shocks. It is certainly one of the most novel and valuable contributions to this department of meteorology, and it is to be hoped that it will appear in a form and language available for European *savants*.

THE Japanese Government are evidently also losing no time in extending their system of telegraphic communication, for we learn from a Japan contemporary that there are now 125 telegraph stations in the country, and it is estimated that there are 5,000 miles of wire in operation; 1,000 miles more are in course of construction, and still further extensions are contemplated. Considering that the first telegraph line for practical purposes was not erected in Japan before the end of 1869, the result achieved is by no means unsatisfactory.

WE have before us three small publications, which indicate the activity of scientific research, especially botanical, in the United States. 1. *The Botanical Directory of America for* 1878 shows an array of names which would compare favourably with the number that could be included in such a list in the old country. Even those who are aware how much good work has been done by American women in several branches of science would hardly be prepared to find so large a proportion of ladies as are to be seen in the present list. 2. *Jahresbericht des naturhistorischen Vereins von Wisconsin für das Jahr* 1877–78, is a record of the year's work of the Natural History Society for the remote State of Wisconsin, so largely settled by Germans. Though some of the papers are in English—including an interesting one by Dr. E. N. Bartlett on *Aspergillus*, detected by him as a parasite in the ear, causing partial deafness—the official language of the Society appears to be German. 3. "A Catalogue of the Flowering Plants and Higher Cryptogams growing within thirty miles of Yale College," published by the Berzelius Society, appears to be carefully executed. The total number of indigenous species is—1,037 flowering plants, 52 vascular cryptogams, and 221 Muscniæ, besides 196 introduced flowering plants. It is accompanied by a map.

"VIS MEDICATRIX NATURÆ." In the light of this venerable saw we do not think it inappropriate in these pages to support an appeal which Dr. Dawson W. Turner asks us to make on behalf of the thousands of patients in the hospitals in and around London. The true healing art is based on rigid scientific research, and one of the most effectual methods of assisting the physician's efforts is to keep the patient in a cheerful mood and divest his thoughts from himself and his afflictions. A potent means to this end is cheerful reading, and we are sure that in this direction many of our readers will be able to assist Dr. Turner in his beneficent mission. Mr. Turner finds that of this class of books none are so acceptable to the sick and suffering, who *can* read, as the cheap one-volume editions of the best of our standard novelists—Scott, Dickens, and Marryat, especially, and then Trollope, Miss Sewell, Mrs. Gatty, and a host of others. Dr. Turner rightly makes a point of excluding everything that is the least "sensational." The lighter sort of serials are also acceptable, such as *Good Words*, *Aunt Judy's Magazine*, *Leisure Hour*, and so forth, as well as picture- and scrap-books, especially if the leaves are pasted on linen. We would add that we are sure many of the patients would welcome some of our more popularly written illustrated scientific works, which tell of greater wonders than ever novelist imagined, and the reading of which, besides amusing the patients, would leave a solid residue of knowledge behind. Dr. Turner's address is 13, Salisbury Street, Strand, W.C.

IN the forthcoming *fête* to be given by the City of Paris, no less than 200 electric candles will be kept burning during the whole of the night in several parts of the city, besides the regular display, which has been increased since our last note.

SOME of our readers may remember that Leverrier proposed to the French government to extend weather-warnings not only to agriculturists but also to the men who risk their lives in collieries. The mournful accident which has occurred near Wigan recently adds force to Leverrier's proposal, and surely, on the chance of its preventing such accidents, the plan might be tried.

IN Lisbon and its vicinity there was a violent shock of earthquake, accompanied by a storm of wind, at eleven o'clock on Saturday night, the duration about six seconds, and the direction east to west. Much alarm was caused.

THE Société Française d'Hygiène Publique, has appointed a Commission for utilising the Giffard Captive Balloon in the study of questions connected with hygiene, such as the influence of rapid decrease of pressure on vital functions, the causes of vertigo, &c. A preliminary programme has been published already.

THE Congress of Hygiene appointed by the French Ministry for the occasion of the Exhibition, will take place at the Trocadero Palace in the month of August. A number of excursions of special interest will be organised on the occasion. The initiative committee meets every week on Wednesday at the Pavillon de Flore (Palais des Tuileries), in the room where the late Congress of Geography was recently held.

PROF. PELIGOT of Paris discusses, in a recent number of the *Ann. de Ch. et Phy.*, the composition of ancient glass, combining his own analysis with a careful study of all passages on the subject in ancient authors. The specimens which he examined all contained mixtures of soda and potash, with but minimal quantities of life—one-third to one-half of the amount used at present. Prof. Peligot comes to the conclusion that flint-glass was entirely unknown in ancient times.

FROM the recent report of the secretary of the Société Chimique de Paris, we notice that its membership is at present 383, consisting of 140 members dwelling in Paris, and 243 non-resident. The yearly receipts amount to over 15,000 francs, of which a third is saved for investment. The Society possesses now a capital of 44,000 francs. Its bi-monthly *Bulletin* forms a yearly volume of 1,200 pages, and has a circulation of over 400 outside of the Society.

WE recently drew attention to Winkler's remarkable lunar landscape, now being exhibited in London. Something even more extensive, if not, perhaps, quite so artistic, is to be attempted by an American artist, if he can procure a sufficient number of subscribers. Mr. Henry Harrison, of Jersey City, has already painted a picture of the moon three and a half days old, and although we have not seen it ourselves, it is so highly spoken of by Dr. H. Draper and Mr. Rutherfurd, that we do not hesitate calling our readers' attention to the artist's proposed publication. The picture represents the moon with the terminator at Mount Glacier, showing the earthshine on the surface in shadow in which some of the most prominent features, *i.e.*, the craters Copernicus and Tycho, the Appennine Mountains, and nearly

all the "meres" are visible. Having submitted the work, Mr. Harrison tells us, to gentlemen of scientific repute, and being encouraged by their favourable criticisms, he has concluded, if a sufficient number of subscriptions can be obtained, to publish a work under the title of "Telescopic pictures of the Moon," in oil colour chromos (the only medium for facsimile reproduction of paintings) 2 feet in size, with an image of 18 inches in diameter, in six progressive pictures of the following phases:—1. Three days old crescent, terminator at Mount Glacier. 2. Five days old, terminator at the crater Katharina. 3. Seven days old, or first quarter. 4. Nine days old, sunrise at the crater Copernicus. 5. Full moon; and 6. Last quarter. An outline drawing with letter-press description, bearing the names and sizes of all objects, will accompany the work, which will be completed in about a year from the time the first phase has been issued, and will be furnished to subscribers complete for 30 dols., or 5 dols. for each plate. The description will appear gratuitously with the last issue. Subscribers should send full name and address to Henry Harrison, P.O. Box, 179, Jersey City, New Jersey.

A VERY successful experiment has been made at Lockport, New York State, in supplying heat to houses by steam supplied from a central station, in much the same way as gas is supplied. The experimental works in Lockport were commenced last year, and during the late winter about 200 houses in the city were heated from the central supply, through about three miles of piping, radiating from the boiler-house, containing two boilers 16 feet by 5 feet, and one boiler 8 feet by 8 feet. These boilers were, during the winter, fired to a pressure of 35 lb. to the inch, with a consumption of 4 tons of anthracite, costing 4½ dols. a ton during the summer, but one boiler is fired consuming a ton and a half of anthracite in twenty-four hours, and a pressure of 25 lb. per inch maintained. The boiler pressure of 35 lb. in winter, and 25 lb. in summer, is maintained through the entire length of the three miles of piping up to the points of consumption, where there is a cut-off under the control of the consumers. The distribution of heat in the apartments is by means of radiators, consisting of 1 inch pipes 30 inches long, placed vertically either in a circle or as a double row, and connected together, top and bottom, with an outlet pipe for the condensed water, which escapes at a temperature a little below boiling, and is sufficient for all the domestic purposes of the house, or is used as accessory heating power for horticultural and other purposes. The steam has also been applied at a distance of over half a mile from the boilers for motive power, and two steam-engines of ten and fourteen horse-power are worked from the boilers at a distance of half a mile, with but a slightly increased consumption of fuel. The laid on steam is being also used for cooking purposes, for boiling, and even baking, and Mr. G. Maur, F.G.S., who describes the system, witnessed in a house three quarters of a mile from the boilers, a bucket of cold water raised to boiler heat in three minutes by the passage of the steam through a perforated nozzle plunged in the bucket. The operations of the Heating Company have been up to the present time of an experimental character, and from the 200 houses already supplied with the heating connection, the actual cost of the coal that would have been used for heating has been provisionally received in payment, and the amount has left a wide margin over the working expenses, though the company's operations at present cover but a small portion of the area for which they have provided plant.

THE additions to the Zoological Society's Gardens during the past week include two Mandrills (*Cynocephalus mormon*), an Ocellated Monitor (*Monitor ocellatus*) from West Africa, presented by Mr. G. H. Garrett; two Greater Spotted Woodpeckers (*Picus major*), British Isles, presented by Mr. J. A. Cooper; a Greater Sulphur-crested Cockatoo (*Cacatua galerita*) from Australia, presented by Mr. N. Portocalis; a Horned Lizard (*Phrynosoma cornutum*) from Texas, presented by Mr. J. C. Witte; a Common Chameleon (*Chamæleon vulgaris*) from North Africa, presented by Mr. W. W. Spicer; two Indree Owls (*Syrnium indranee*) from Ceylon, deposited; two Common Seals (*Phoca vitulina*) from British seas, a White-fronted Amazon (*Chrysotis leucocephala*) from Cuba, two Oyster Catchers (*Hæmatopus ostralegus*), British Isles, purchased; two Horned Tragopans (*Ceriornis satyra*), an Impeyan Pheasant (*Lophophorus impeyanus*), bred in the Gardens.

THE ROYAL OBSERVATORY

THE annual visitation of the Royal Observatory took place on Saturday week, when the Astronomer-Royal read his Report, which refers to the year ending May 2.

The Report on the buildings and grounds, movable property, manuscripts, library, astronomical instruments, &c., is, as usual, satisfactory. The new railway through the town of Greenwich has apparently had no effect at the observatory.

The usual varied astronomical observations have been carried on with the usual diligence, the advantageous observation of the small planets being, however, limited by the want of ephemerides.

To facilitate the observations of stars, a new working catalogue has been prepared, in which are included all stars down to the third magnitude, stars down to the fifth magnitude which have not been observed in the last two catalogues, and a list of 258 stars of about the sixth magnitude of which the places are required for the United States Coast Survey. The whole number of stars in the new working list is about 2,500. An extensive series of observations was made, during the autumn, of about seventy stars, at the request of Mr. Gill, for comparison with Mars, Ariadne, and Melpomene.

Among the observations made we may mention 3,970 transits, the separate limbs being counted as separate observations, and 3,824 circle observations, each requiring a separate reading of the six microscope micrometers. Twenty-nine sketches of Mars were obtained with the great equatorial near his opposition, forming a complete record of the appearances of that planet during the entire rotation. Preparations were made for observing the Transit of Mercury on May 6, but owing to the unfavourable state of the weather no result of importance was obtained. A great amount of work has been done in the reaction of astrononomical observations.

The computations for the "Nine-Year Catalogue" of 2,263 stars, including some supplementary investigations, were completed in the course of 1877, and the introduction has been prepared and sent to the printer. The catalogue is drawn up in the same form as previous catalogues, the only noteworthy alterations being the addition of another decimal place to the R.A.'s and annual precessions in R.A., which are carried to 0s·001 and 0s·0001 respectively. The right ascensions are thus made to correspond more nearly with the north polar distances as regards the degree of accuracy exhibited.

During the past year the sun's chromosphere has been examined with the spectroscope on seventy-nine days (on two of these through part of the circumference only); prominences were seen on fifty-eight days. All the observations, however, tend to show that the solar prominences have been few in number and insignificant in size for many months.

All observations with the spectroscope have been completely reduced; the position-angles of prominences being converted into heliographic N.P.D.; and the displacements of lines in the spectra of stars being reduced so as to exhibit the concluded motion in miles per second, after applying a correction for the earth's motion.

The areas, position-angles, and distances from the sun's centre, of sun-spots and faculæ, have been measured to the end of 1877, and in duplicate July 5, 1877.

The correction of the position-angles and distances for the effects of refraction and distortion, and their conversion into heliographic longitude and latitude, have been pushed forward as rapidly as circumstances would admit after the measurements had been completed. As there is a considerable accumulation of arrears since 1873, which will require many months for their reduction, it has seemed desirable to commence with the year 1876, with the view of including in the volume for 1876 the

complete deductions from the measures of sun-spots and faculæ in that year, if they can be prepared for press in time, leaving the complete results for the years 1873 to 1875 to be included in the next volume; the areas, as distinct from positions, having been already printed in the volumes for 1874 and 1875.

The usual magnetical and meteorological observations have been carried on, and considerable progress made with their reduction.

The following are the principal results for magnetic elements in the year 1877:—

Approximate mean westerly declination	18° 57′.
Mean horizontal force	3·901 (in English units). 1·799 (in metric units).
Mean dip	67° 38′ 46″ (by 9-inch needles). 67 39 54 (by 6-inch needles). 67 40 40 (by 3-inch needles).

Under the head of Extraneous Work, information is given as to the reduction of the Transit of Venus observations.

At the date of the last Report the determination of the longitudes of the British stations was not quite complete (that of Kerguelen being then imperfect). But, under a demand from the House of Commons, a strong effort was made to finish all introductory calculations, and to effect computations of solar parallax by comparing all eye-observations of ingress of Venus among themselves, and all eye-observations of egress of Venus among themselves. The different stages of phenomena at the ingress were discriminated by Capt. Tupman with great care, and Sir George Airy believes with great general success, although Capt. Tupman himself has been induced lately to modify his interpretation of the observers' language in one or two instances. Finally, a report was made to the Government on July 5, giving as the mean result for mean solar parallax 8″·76, the results from ingress and from egress, however, differing to the extent of 0″·11. A more complete calculation by the Astronomer-Royal, including in one series the observations both at ingress and at egress, and recognising the possible errors of R.A. and N.P.D., gave sensibly the same mean result for parallax. This is liable to no error except from the interpretation of observers' language. All has subsequently been re-examined by Capt. Tupman; different interpretations have, in a few instances, been put on the records; several observations from colonial stations have been combined; instead of using different phases in the observations (both of ingress and of egress), attempts have been made to ascertain the one phase of "contact of limbs;" the notes of a few unpractised observers have been rejected, and the result for parallax has been increased to 8″·82 or 8″·83.

The numerous photographs taken at the various stations had been carefully measured by Mr. Burton, and have since been re-measured by Capt. Tupman; and (by photographs of Mr. De la Rue's scale of equal parts) the measure of photographic distortion had been well ascertained. But the results from photography have disappointed Sir George Airy much. The failure has arisen perhaps sometimes from irregularity of limb, or from atmospheric distortion, but more frequently from faintness and from want of clear definition. Many photographs which to the eye appeared good, lost all strength and sharpness when placed under the measuring microscope. It was once remarked to Sir George Airy, "You might as well try to measure the zodiacal light." A final result, 8″·17, the report states, was obtained from Mr. Burton's measures, and 8″·08 from Capt. Tupman's.

The Report next alludes to the progress made in the numerical lunar theory. The developments of the effect of every possible error (expressed as a symbolical variation) in the co-efficients and arguments of the assumed lunar ordinates upon every term in the three fundamental expansions of—(1) Areas in the ecliptic, (2) Radial forces in the ecliptic, (3) Forces normal to the ecliptic—have been computed and printed. The corresponding solar perturbing forces have been computed entirely for the first of these (care being taken to extend the decimal calculation further for those terms whose effect may probably be increased in solution of the equations, a process in which many figures are almost necessarily wasted), and partially for the second and third. Until all have been completed the Astronomer-Royal cannot draw any positive inference from the comparison of these terms with those of the ordinate expansions; but a cursory collation of those relating to the areas led him to suppose that there might be some error in the computations of the annual equation and related terms. A most jealous re-examination has, however, detected nothing, and has confirmed Sir George Airy's belief in the general accuracy of the numerical computations.

Finally, Sir George Airy strongly urges upon the Board the necessity for the erection of a separate room for the library of the Observatory.

COSMICAL RESULTS OF THE MODERN HEAT THEORY

IN the *Sitzungsberichte der Wiener Akademie der Wissenschaften*, Herr J. Loschmidt has published a treatise on the equilibrium of temperature in a system of heavenly bodies with regard to gravitation, from which we note the following highly interesting details:—"Sir W. Thomson and Clausius simultaneously,[1] drew from their researches the surprising conclusion that the whole universe at some definite period, however remote, would infallibly come to an end. First, all ponderous masses in the universe will eventually have united to one enormous heavenly body; and secondly, upon this body all visible motion will have ceased, all forces having changed to mere molecular motion, which in the shape of heat of universally uniform temperature will be spread in this mass. This state of general death will then last eternally." Herr Loschmidt, in the course of his researches, has arrived at widely different conclusions. He begins by adopting the general view that the sun is in a state of slow progression of cooling, and that the time will unavoidably arrive when his surface will have solidified, long after all his planets have fallen in upon him, and after his upper and partly also his lower strata have assumed very nearly the temperature of the surrounding universal space. But granting that thus a period of rest and death will have arrived for our solar system, Herr Loschmidt maintains, at the same time, that this period cannot be of unlimited duration; the state of things just described can, according to his views, not be a state of equilibrium. "The previous liquid state of the sun has caused a continued mixture of the warmer parts near the centre with the colder ones near the surface. Thus, however, the equilibrium of temperature, which requires a certain increase of temperature towards the interior, was rendered impossible. At the moment of solidification of the external layers the deeper ones will be far colder than the theory of the state of equilibrium demands. Because, according to this theory, the surface should have the temperature of universal space (about − 140° C. according to Pouillet), but this temperature should rapidly increase towards the interior, reaching at the centre the enormous figure of 250,000,000° C. And it is just because at the moment of the beginning of solidification of the sun no such distribution of temperature took place in the interior, that the state above referred to cannot be of eternal duration. During an extremely long period, in spite of the low temperature of his surface, the solidifying sun will constantly absorb radiant heat from the store in the universe and will concentrate this heat in his interior. We suppose, for a moment, that it would be physically possible that this process of absorption is carried on to the end without the inclosed and dissociated gases in the interior breaking through the solidified surface or crust on account of their enormous tension. We then calculate the amount of heat accumulated in the end and find that it would easily suffice to raise the entire solar mass to ⅖ths of that temperature which the state of equilibrium demands at the centre, viz., to 100,000,000° C. This figure is raised if the average molecule of the solar mass, instead of being supposed to be of the density of oxygen, is taken to be of the density of carbonate of lime; in that case it would be 125,000,000° C. We may compare these results to the quantity of heat which was produced during the condensation of the solar system from the cosmical nebula, according to the theory of Laplace and Kant. Helmholtz has calculated that the heat thus generated would suffice to raise the solar mass to a temperature of 28,611,000° C., if it is supposed to have the heat capacity of water. If, instead of water, other substances are taken as starting points, this temperature is considerably raised; so in the case of carbonate of lime or silicic acid, the heat capacity of which is 0·2, the resulting temperature would be 140,000,000° C.

"The close correspondence of both amounts speaks in favour of a *periodicity in the history of solar systems*. In the first portion of its cosmical period the dark solidified body absorbs heat

[1] [Clausius *verified* Thomson's statements about dissipation just as he *verified* (after experiment had proved it) J. Thomson's statement of the lowering of the freezing-point of water by pressure. Some Germans still call this "simultaneous discovery." Helmholtz, at least, does not.—ED.]

from universal space, and thus the temperature in its interior is gradually increased to an immeasurable extent. Then the moment arrives when the exterior crust can no longer resist the rising pressure of the inclosed masses, which have, of course, become gaseous. An explosion must result. The greater part of the mass which is converted into gas is dispersed over a great space, and thus by far the greater part of the accumulated heat is converted into gravitation and force of rotation o fthe dispersed masses. Now the second portion of the solar period begins, which, as a process of condensation of cosmical nebulæ and subsequent slow cooling of the bodies formed by this condensation, has been frequently discussed since the days of Laplace."

This is Herr Loschmidt's idea of the typical course of a cosmical period, if fully developed according to the laws of heat. But he thinks that it is highly probable that this full development can be but rarely realised in the case of a solar system, since the duration of the heat-absorption will generally find a premature end in the impossibility of the external crust resisting the enormous pressure of the inclosed gases until the maximum of temperature is arrived at. "Upon our sun, for instance, in a state of equilibrium of temperature, the surface temperature would be $-140°$ C., while at a depth of half a (German) mile we already would find a temperature of 3,000° C. Here all known substances would be in a state of liquid incandescence. The solid crust could therefore not be thicker than half a (German) mile. In this case, therefore, the typical course described would evidently be interrupted prematurely by an explosion.

"The consequences of a solar eruption of this kind are naturally very different under different conditions. Thus with a comparatively small accumulation of heat and corresponding low tension, the result would be simply the return to incandescence of a dark heavenly body, while with greater concentration of heat some portions may be separated from the principal mass and carried to great distances, where, forming themselves into planets, they would revolve round the principal mass in elliptical orbits. This theory, therefore, easily explains the origin of planets, like those of our system, and the manner in which they were carried to their respective places and are provided with their forces of rotation and revolution, and also how after all in the principal solar mass a quantity of heat would remain, which would cause a far higher temperature upon its surface than exists at present upon our sun. The principal solar mass would thus be again enabled to radiate light and heat to its planets and into the universe, until again the moment of solidification and re-beginning of absorption of heat has arrived. The total result under the most varying circumstances always remains the same: *periodicity of the dynamical solar phenomena*.

"If finally we look for proofs for our theory in the heavens, we direct our attention to dark burnt-out suns on the one hand and to suddenly appearing new suns on the other. It is strange that modern times have given examples of both classes of phenomena. As a representative of the first class we have the dark companion of Sirius, calculated in advance by Bessel from the disturbances, and actually seen by A. Clark and Pond in 1862. This enormous mass has only just been rendered visible by the most powerful instruments, although it is nearly seven times the size of our sun. A second example is the companion of Procyon which, though calculated with certainty, has not yet been seen on account of its still greater darkness. Examples of the other class we have in the well-known new stars of Tycho Brahe and Kepler, besides the new star in Corona of 1866 and the one recently seen by Schmidt and others in Cygnus (December, 1876). In both these latter cases eruptions of incandescent hydrogen were proved beyond doubt by spectral observations."

THE METEOR

A METEOR of unusual brilliance was seen of the "fire-ball" type on Friday night by several correspondents. All agree that the time was about 9.50, the moon at the time being in her second quarter, and about 30° above the horizon in the west-south-west. At Twickenham its observed course was from south-west to north-west passing the azimuth of the moon at the time 69° from south to west, at an altitude of about 14°, its path being nearly parallel to the horizon, or declining very slightly towards its disappearance, which was sudden, at 9h. 52m. 30s. Greenwich mean time. Colour, bright emerald green; apparent diameter, about one-third of that of the moon, this being the greater diameter of the elliptical figure. The light thrown by the meteor in this locality was decidedly green. Mr. Lecky, writing from the Scientific Club, states that the course of the meteor was about 90° below the moon, its motion very slow, and it became extinguished rather suddenly, without any apparent bursting, when it had passed about the same distance to the north of the moon. The meteor appeared to Mr. Lecky to be about the same size as the moon. Mr. L. J. Whalley saw it from the Brompton Road. Facing west he saw it pass from south to north, its path being inclined downwards at a few degrees to the horizon, and its altitude about 30°. The fore-part appeared rounded in shape, and of a bright green colour (like nickel sulphate), whilst the tail tapered off, and was of a red to a purplish tint.

Mr. Walter Fowler saw it from Cambridge. Its path, he states, was from south to north, almost parallel with the horizon, with a slight declination northwards. During its course, which lasted about twenty seconds, it emitted innumerable sparks variegated in colour.

Another correspondent saw it from London Street, Greenwich. Its apparent altitude, he states, was about 28° or 30° above the western horizon, and it passed horizontally over the tops of the houses in a direction about two points to the west of north. He observed it for about three seconds. It appeared in passing under the moon to be about 6° or 8° underneath her lower limb, and about the same degree of brightness and equal to it in size. The meteor, he states, had a tail about equal to six or seven diameters of its nucleus; the central part of the tail and the nucleus were of a pale orange hue and fringed with violet rays. The tail was in the line of motion, and was not a perfect cone, but appeared to expand into a fin-like form at the extremity.

Mr. F. J. Richardson, of Dimchurch, near Rugby, observed the meteor, "of considerable size," cross the sky, apparently about 30° above the horizon. The direction of its path was from south to west, and its colour appeared a mixture of orange and green. It remained visible for about thirty or forty seconds, and then suddenly disappeared.

Mr. R. Langdon, writing from Silverton Station, Devon, states that it moved slowly towards Ursa Major, and exploded a little beyond that constellation. Its colours were, first, very pale blue (nearly white), then deep blue, and finally, the several fragments after explosion were blood-red. Dr. Morison saw it from Jersey. When first seen it was about 30° from the zenith in a direction nearly due north. The diameter of its disc, which was apparently circular, was rather more than half that of the full moon, which it far surpassed in brilliancy, shining with a beautiful white light. The meteor descended towards the horizon, leaving a very faint luminous trail behind it, and was lost to sight, while still remaining entire, behind a high wall. It was altogether visible about thirty seconds.

Our Paris correspondent writes that a splendid meteor was seen in the department of Aisne and at Versailles about ten o'clock in the evening, travelling westwards at a small altitude. It was in diameter about one-sixth that of the moon, the brilliancy admirable, and the tail four or five times the length of the moon's diameter. No noise was heard.

Mr. Denning, of Bristol, writing to the *Times*, states that the meteor had a very long path, almost horizontal, from east to west, which it traversed with a gradual motion, casting off a short train of sparks as it sailed along, and showing sensible variations in the brilliancy of its pear-shaped nucleus. The position of the observed part of its path was noted from 1° above the star Spica Virginis to 6° above the moon, but to include the whole extent of its visible course the line must be extended in each direction, and have a length of at least 75° from, say, slightly below Alpha Libræ to slightly above Alpha Leonis, running almost parallel with the ecliptic. The meteor was considerably brighter than Venus, and perhaps equal to a body one-fourth of the moon's diameter. Mr. H. Middleton Rogers states that while walking along a footpath close to Knole Park, he saw it passing apparently from south to north, very nearly parallel to the horizon, with a very slight declination towards the north. When he first saw it it was about 5° from the moon, (taking the moon's diameter roughly at half a degree.) It passed slowly along about 3° below the moon, or about 30° above the horizon, and continued its course for about 20° further towards the north, when it suddenly disappeared. The light was of a very pale green, as nearly as possible like the light of a glow-worm highly intensified. As it passed under the moon its brilliancy caused the moon to look of a muddy yellow colour—

like a street lamp in a November fog. A *Times* correspondent at Cheltenham says that the path of the body was almost due east and west, and the apparent time of flight about 20″. The meteor was also observed at Southampton, Tunbridge Wells, and Beckenham.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

The Sedgwick Memorial Committee (Cambridge) have passed the following resolution, which has been sent to the Vice-Chancellor:—"That a communication be made on behalf of the Committee to the University to the effect that a sum of about 12,000*l*. is now at their disposal for a memorial to the late Prof. Sedgwick, and that the Committee are prepared to apply this money towards the erection of a new geological museum when a plan satisfactory to the Committee has been approved by the Senate."

On the nomination of Prof. Miller, Mr. W. J. Lewis, M.A., Fellow of Oriel College, Oxford, has been approved as Deputy Professor of Mineralogy for twelve months, from October 1, 1878, Prof. Miller assigning to Mr. Lewis two-thirds of his stipend.

Mr. J. A. Ewing, B.Sc., F.R.S.E., has been appointed Professor of Mechanical Engineering in the University of Tokio, Japan.

SCIENTIFIC SERIALS

Bulletin de l'Académie Royal de Belgique, No. 1, 1878.—In researches on Daltonism, here described, MM. Delbœuf and Spring used a solution of fuchsine between two convergent plates of glass (the red is wanting in M. Delbœuf's sight). Thus a suitable thickness of red could be readily selected, and it was found that colours previously confounded showed notable differences. A solution of chloride of nickel interposed between objects and the eye produces in non-Daltonians the same confusion as that of Daltonians. Fuchsine opposes and destroys the effect of chloride of nickel: so that the non-Daltonian in whom the latter produces confusions ceases to have these when he looks also through the fuchsine. Daltonism is regarded as merely an exceptional exaggeration of a peculiarity found in all eyes to a certain degree.—M. Terby furnishes fifteen figures of Mars as observed during the opposition of 1877.—The physiological action of *Gelsemine*, on respiration, circulation, and temperature, is described by MM. Putzey and Romiée.—M. de Koninck announced that his son found, in the Ardennes, the very rare mineral carpholite, hitherto only met with in the Harz and Bohemia.

No. 2.—From experiments with regard to the fertilising action of the grey chalk of Ciply, in Belgium (which contains 11·50 per cent. of phosphoric acid), M. Petermann concludes that bicalcic phosphate, called precipitated phosphate, and the phosphates of iron and alumina, have the same agricultural value as the phosphoric acid of soluble phosphates, that is, their phosphoric acid may be immediately assimilated by plants. He therefore advises the disuse of the Cipley Chalk, and he considers it can only be utilised in agriculture after its transformation into precipitated phosphate. (M. Stas thinks this conclusion too absolute.)—M. Quetelet reviews observations of the movements of the magnetic needle at Brussels from 1828–76. The magnetic line diverges very little from a central axis, with which it makes an angle of about 5°. It turns round this axis in a direction opposite to that of the earth's diurnal motion; the angle described annually is about 42′·2, and the complete revolution would appear to be effected in 512 years. The secondary movements and accidental displacements do not sensibly affect the principal secular movement.—M. Donny recalls experiments he made, in 1843, with Prof. Mareska, on liquefaction of gases. They often compressed air (with a hydraulic pump) in the capillary part of a manometer to more than 500 atm., and M. Donny thinks they may have liquefied the gas without knowing it.—MM. Navez describe a combination of an induction coil with the telephone for speaking at great distances. The induced currents are sent into the line, while the sending instrument is inserted in the local circuit connected with the battery. The receiving telephone is somewhat modified.—The subjects for prizes offered by the Academy for 1879 are announced in this number.

Reale Istituto Lombardo di Scienze è Lettere. Rendiconti, vol. xi. fasc. iv.–vi.—We note the following papers in these numbers:—Deformative hypertrophy of the nails, by M. Sangalli.—Claustrophobia, by M. Verga.—Some experiments with the telephone, by M. Serpieri.—On the dominant diseases of the vine, by MM. Garovaglio and Cattaneo.—On the kinematics of a solid body, by M. Bardelli.—Lecture experiment (illustrating liquefaction of gases), by M. Brugnatelli.—An experiment on electrostatic induction, by M. Cantoni.—On a case of heterogenesis observed in nature, by MM. Battista and Corrado.—Reduction of argentic and ferric chloride, by M. Tommasi.—Geological observations on the Carso di Trieste and the valley of the Recci with reference to water supply, by M. Taromelli.

THE *Bulletin de l'Académie Impériale des Sciences de St. Pétersbourg* (t. xxiv. No. 4) contains the following papers of interest:—Development into converging series of the odd negative powers of the square roots of the function $1 - 2\eta U + \eta^2$, by Dr. J. Backlund.—Variation of the volume of liquids through the effect of temperature, by M. Avenarius.—On some new forms of crystals of ilmeno-rutile, by P. Jeremejew.—On the development of excrescences (cephalodia) on the thallus of *Lichen*, *Peltigera aphthosa*, Hoffm., by M. Babikoff.—On a new case of divisibility of the numbers of the form $2^{2^m} + 1$, found by the Rev. J. Pervouchine, by V. Bouniakowsky.—A note on the opposition of planets during 1877, by A. Sawitch.—On an extremely slight earthquake observed by means of a very delicate level on May 10, 1877, by M. Nyrén.

Morphologisches Jahrbuch, vol. iv., supplement, dedicated to Carl von Siebold.—On the cranial skeleton of alepocephalus, a clupeoid fish, by Prof. Gegenbaur, two plates, 42 pp.—Fossil vertebræ, by C. Hasse, dealing with the relationship of the genus Selache; two plates. The author believes this genus to have developed from Carcharodon in the tertiary period.—The gorilla's brain and the third frontal convolution, by Prof. von Bischoff, a controversial article referring to Prof. Broca's researches and views.—Contribution on the coral family Antipatharia, by G. von Koch.—The disposition and development of elastic tissue, by L. Gerlach, with two beautiful plates.—The development of the muscular structure of the human foot, by G. Ruge, 36 pp. one plate.

THE *Notizblatt des Vereins für Erdkunde zu Darmstadt* (iii. xvi. Nos. 181 to 192) contains some interesting statistical data from the Hessian Central Statistical Office. The papers of geological interest are: On the crystalline lime of Auerbach on the Bergstrasse, by R. Ludwig.—On the minerals found in the cavities of the melaphyr from Traisa and in the basalt of the Rossberg, by the same.—On the minerals and fossils found near Hering (Hessen), by the same.—Comparative account of the products of all Hessian mines during the years from 1860 to 1876, by Herr Tecklenburg.—On the fauna of the real Cyrene emery of Sulzheim, near Woerrstadt (Hessen), by Dr. O. Boettger.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, May 2.—"Preliminary Notes on Experiments in Electro-Photometry." By Prof. James Dewar, F.R.S., Jacksonian Professor, University of Cambridge.

Edmond Becquerel, in the year 1839, opened up a new field of chemical research through the discovery that electric currents may be developed during the production of chemical interactions excited by solar agency.

Hunt, in the year 1840, repeated, with many modifications, Becquerel's experiments, and confirmed his results.

Grove, in 1858, examined the influence of light on the polarized electrode, and concluded that the effect of light was simply an augmentation of the chemical action taking place at the surface of the electrode.

Becquerel, in his well-known work, "La Lumière," published in 1868, gives details regarding the construction of an electrochemical actinometer formed by coating plates of silver with a thin film of the sub-chloride, and subsequent heating for many hours to a temperature of 150° C.

Egeroff, in 1877, sugested the use of a double apparatus of Becquerel's form, acting as a differential combination, the plates of silver being coated with iodide instead of chloride.

The modifications of the halogen salts of silver when subjected to the action of light have up to the present time been used most

successfully in the production of electric currents, and although mixtures of photographically sensitive salts have been shown by Smee to produce currents of a similar kind, yet no attempt has been made to examine the proper form of instrument required for the general investigation of electrical actions induced by light on fluid substances.

This subject has occupied my attention for some time, and the completed investigation I hope to present to the Society. In the meantime the following description will give an idea of the method of investigation.

A little consideration shows that the amount of current produced by a definite intensity and quality of light acting during a short period of time on a given sensitive substance in solution, is primarily a function of the nature, form, and position of the poles in the cell relatively to the direction in which the light enters, and the selective absorption, concentration, and conductivity of the fluid.

Diffusive action taking place in such cells complicates the effects and is especially intricate when insoluble substances are formed. In order to simplify the investigation in the first instance, poles that are not chemically acted upon, and a sensitive substance yielding only soluble products on the action of light, were employed. For this purpose platinum and chlorous acid or peroxide of chlorine were selected.

The best form of cell had one of the poles made of fine platinum wire fixed as closely as possible to the inner surface where the light enters, the other pole being made of thicker wire placed deeper in the fluid.

As the action is confined to a very fine film where the light enters, the maximum amount of current is obtained when the composition of the fluid is modified deep enough to isolate temporarily the front pole in the modified medium. Under these conditions the formation of local currents is avoided, and the maximum electromotive force obtained.

In cells of this construction the amount of current is independent of the surface of the fluid acted upon by light, so that a mere slit sufficient to expose the front pole acts as efficiently as a larger surface. This prevents the unnecessary exhaustion of material, and enables the cell to be made of very small dimensions. By means of such an apparatus the chemical actions of light and their electrical relations may be traced in many new directions.

The amount and direction of the current in the case of chlorous acid is readily modified by the addition of certain salts and acids, and thus electrical variation may be produced, resembling the effects observed during the action of light on the eye. . . . .

Certain modifications taking place in chlorous acid that has been prepared for some time increase its sensibility, and as a general result it is found that the fluid through these alterations increases in resistance. We have thus an anomalous kind of battery where the available electromotive force increases with the resistance. The addition of neutral substances which increase the resistance without producing new decompositions, improves the action of the cell.

Care has to be taken to use the same apparatus in a series of comparative experiments, as infinitesimal differences in the contact of the active pole render it difficult to make two instruments giving exactly the same results. Cells have been constructed with two, three, and four poles, and their individual and combined action examined. Quartz surfaces have also been employed instead of glass, thus enabling the chemical opacity of different substances to be determined.

The electrical currents derived through the action of light on definite salts are strong in the case of ferro- and ferri-cyanide of potassium, but remarkably so in the case of nitroprusside of sodium.

Of organic acids the tartrate of uranium is one of the most active. A mixture of selenious acid and sulphurous acid in presence of hydrochloric acid yields strong currents when subjected to light in the form of cell described. The list of substances that may be proved to undergo chemical decomposition is very extensive, and full details will be found in the completed paper.

Geological Society, May 8.—Henry Clifton Sorby, F.R.S., president, in the chair.—Charles Preller Sheibner, Ph.D., was elected a Fellow of the Society.—The following communications were read:—On the glacial phenomena of the Long Island, or Outer Hebrides (second paper), by James Geikie, F.R.S. In this paper the author gave some additional notes on the glaciation of Lewis, and a detailed account of the glacial phenomena of Harris and the other islands that form the southern portion of the Outer Hebrides. In concluding, the author pointed out that we may now arrive at a true estimate of the thickness attained by the ice-sheet in the north-west of Scotland. If a line be drawn from the upper limits of the glaciations in Ross-shire (3,000 feet) to a height of 1,600 feet in the Long Island, we have an incline of only 1 in 210 for the upper surface of the ice-sheet; and of course we are able to say what thickness the ice reached in the Minch. Between the mainland and the Outer Hebrides it was as much as 3,800 feet. No boulders derived from Skye or the mainland occur in the Till of the Outer Hebrides, and this was explained by the deflection of the lower portion of the ice-sheet against the steep wall of rock that faces the Minch. The under part of the ice that flowed across the Minch would be deflected to right and left against the inner margin of the Long Island; and the deep rock-basins that exist all along that margin are believed to have been scooped out by the grinding action of the deflected ice. Towards the north of Lewis, where the land shelves off gently into the sea, the under strata of the ice-sheet were enabled to creep up and over the district of Ness, and thus gave rise to the lower shelly boulder-clay of that neighbourhood, which contains boulders derived from the mainland. The presence of the overlying interglacial shell-beds proves a subsequent melting of the ice-sheet, and a depression of the land for at least 200 feet. The overlying shelly boulder-clay shows that the ice-sheet returned and overflowed Lewis, scooping out the older drift-beds and commingling them with its bottom moraine. The absence of kames was commented upon, and shown to be inexplicable on the assumption that such deposits are of marine origin; whilst if they be of torrential origin their absence is only what might be expected from the physical features of the islands. The only traces of post-glacial submergence are met with at merely a few feet above present high-water mark.—Cataclysmic theories of geological climate, by James Croll, F.R.S. Communicated by Prof. Ramsay, F.R.S. The author commenced by calling attention to the great diversity of the hypotheses which have been brought forward for the explanation of those changes in the climate of the same regions of the earth's surface which are revealed by geological investigations, such as alterations of the relative distribution of sea and land, of the ecliptic, and of the position of the earth's axis of rotation, all of which, he maintained, have proved insufficient or untenable. Sir William Thomson has lately maintained that an increase in the amount of heat conveyed by ocean-currents combined with the effects of clouds, winds, and aqueous vapour, is sufficient to account for the former prevalence of temperate climates in the Arctic regions, and this view, the author stated, he had himself been contending for for more than twelve years. He thinks, however, that alterations in the eccentricity of the earth's orbit is the primary motive cause, whilst Sir William Thomson believes this to be the submergence of circumpolar lands, which, however, in miocene times, appear to have been more extensive than at present. He pointed out that a preponderance of equatorial land, as assumed by Sir Charles Lyell to account for the milder climate of Arctic regions in miocene times, would rather tend to loss of heat by rapid radiation into space, whilst water is remarkably powerful as a transporter of heat, so that, in this case, equatorial water rather than equatorial land is needed. In speaking of the glacial climate, the author maintained that local causes are insufficient to explain so extensive a phenomenon. He indicated that we are only too prone to seek for great or cataclysmic causes, and although this tendency has disappeared from many fields of geological research, this is not the case in all. His explanation of the causes of a mild climate in high northern latitudes is as follows:—Great eccentricity of the earth's orbit, winter in perihelion, the blowing of the south-east trades across the equator perhaps as far as the tropic of Cancer, and impulsion of all the great equatorial currents into northern latitudes; on the other hand, when, with great eccentricity, the winter is in aphelion, the whole condition of things is reversed; the north-east trades blow over into the southern hemisphere, carrying with them the great equatorial currents, and glacial conditions prevail in the northern hemisphere. Thus those warm and cold periods which have prevailed during past geological ages are regarded by the author as great secular summers and winters. —On the distribution of ice during the glacial period, by T. F. Jamieson, F.G.S. The author believes that a study of the distribution of ice during the glacial period proves that the greatest

accumulation of snow took place in precisely those districts which are now characterised by a very heavy rainfall, and he pointed out how exactly this is in accordance with the views of Prof. Tyndall as to the conditions most favourable to the development of glaciers.

Zoological Society, May 21.—F. D. Godman, F.Z.S., in the chair.—A communication was read from Lieut.-Col. R. H. Beddome, C.M.Z.S., containing the description of a new genus and species of snakes, of the family of Calamariidæ, from Southern India, proposed to be called *Xylophis indicus*.—Mr. P. L. Sclater, F.R.S., read the tenth of a series of reports on the collection of birds made during the voyage of H.M.S. *Challenger*, containing an account of the birds of the Atlantic Islands and Kerguelen's Land, and of the miscellaneous collections made by the expedition.—Mr. J. Wood Mason, F.Z.S., described several new or little known Mantidæ from India, Australia, and other localities.—Mr. H. W. Bates, F.Z.S., read a paper containing the description of new genera and species of Geodephagous Coleoptera from Central America, belonging to the families Cicindelidæ and Carabidæ.—Mr. G. French Angas, C.M.Z.S., read the description of a new species of *Tudicula*, which he proposed to name *T. inermis*.—A communication was read from the Marquis of Tweeddale, F.R.S., being the ninth of his contributions to the ornithology of the Philippines. The present paper gave an account of the collection made by Mr. A. H. Everett in the Island of Palawan, and contained the descriptions of nine new species, namely, *Tiga everetti*, *Dicrurus palawanensis*, *Broderipus palawanensis*, *Trichostoma rufifrons*, *Drymocataphus cinereiceps*, *Brachypus cinereifrons*, *Criniger palawanensis*, *Cyrtostomus aurora*, and *Corvus pusillus*. The collection likewise contained three examples of the remarkable *Polyplectron emphanes*, of which the locality was previously unknown, and specimens were excessively rare.—Prof. A. H. Garrod, F.R.S., read a paper in which he gave a description of the tracheæ of *Tantalus loculator* and of *Vanellus cayennensis*.—A second paper by Mr. Garrod contained some notes on the anatomy of the Great-headed Maleo (*Megacephalon maleo*).

Victoria (Philosophical) Institute, May 31.—Annual Meeting; the president, the Right Hon. the Earl of Shaftesbury, K.G., in the chair.—From the annual report it appeared that the number of members is now 756.—The Address was delivered by Principal Rigg, D.D., and contained a review of various systems of philosophy now popular.

PARIS

Academy of Sciences, June 3.—M. Fizeau in the chair.—The following among other papers were read:—Direct determination at sea of the azimuth and route of a ship, by M. Faye. This is for iron ships, and involves keeping the ship some time in a fixed direction indicated by the log-line and determined astronomically. The log is slightly modified in form.—New researches on the fossil mammalia of South America, by M. Gervais. The author has examined the recent collections of MM. Ameghino, Brachet, and Larroque, from the province of Mines, in Brazil, and some parts of the Argentine Republic. He is able to add some new details about the Toxodon, and describe, *inter alia*, a new species of Machairodus, and two new species of Glyptodon (the species of which, he estimates, certainly exceed a dozen).—On the chalk of the Central Pyrenees, by M. Leymerie. He finds there a bed immediately under the first eocene layer, containing quite a special marine fauna, among which are numerous urchins.—M. Cornu was elected member in the section of physics in place of the late M. Becquerel.—Direct fixation of carbonic acid, sulphurous acid, and phtalic anhydride, on benzine; synthesis of benzoic acid, hydride of sulphophenyl, and benzoylbenzoic acid, by MM. Friedel and Crafts. The authors suppose in these syntheses an organo-metallic combination of aluminium by the reaction of the chloride of this metal on the hydrocarbons.—On the manufacture of cast manganese and on the volatility of manganese, by M. Jordan. More than 100,000 kil. of this cast manganese (from treating ores of manganese in the blast furnace) have already been supplied to French steel works. Manganese is volatile at the temperature of metallurgical furnaces; and this fact explains several anomalies remarked in the manufacture of very manganesised products.—On Daltonism; sanitary precautions, and preventives, by M. Favre. There are in France more than 3,000,000 persons affected with Daltonism; the number of women affected is to that of men as 1 : 10. Nine out of ten cases can easily be cured in youth; the best means being methodic exercise on coloured objects. This should be attended to in all schools, and mothers should seek to develop the chromatic sense in their children. No one should be admitted to the service of railways, the navy, or schools of painting, without being examined in colours. No Daltonians should be charged with service involving the use of coloured signals.—Information was given regarding observation of the transit of Mercury in the United States.—On the densities of vapour, by M. Troost. He describes the behaviour of vapour of acetic acid, hyponitric acid, sulphur, and hydrate of chloral. Sulphur vapour behaves like ozone, whose density is independent of pressure, and whose transformation into oxygen takes place in proportion as the temperature is raised.—On metallic allotropy, by M. Schutzenberger. By electrolysis of metallic solutions, allotropic varieties of other metals besides copper (*e.g.* lead) may be got. It is impossible to decide by direct experiment whether or not allotropic copper contains occluded hydrogen eliminable at 100°. In any case the proportion of hydrogen could not exceed 0·03 per cent.—Method of determination and separation of stearic acid and oleic acid proceeding from saponification of tallow, by M. David. The principle of this process is based on the new fact that when into an alcoholic solution of oleic acid one pours acetic acid drop by drop, a moment comes when, suddenly, the oleic acid separates completely.—On the structure of nerves in invertebrates, by M. Cadiat. In crustacea, insecta, and annelida, the nerves have no myeline, which in vertebrates is found between the cylinder axis and the wall proper of the tube (the grey fibres of the great sympathetic excepted). In gasteropodous and acephalous molluscs the sheath of Schwann is almost always wanting.—On the relations between the volume of motor or sensitive cells of nervous centres, and the length of passage of the impressions transmitted, by M. Pierret. The dimensions of the nerve-cells are in direct ratio of the distances which the motor incitations proceeding from them, or the sensitive excitations reaching them, have to traverse.—There were several other papers on chemical subjects, determination of arsenic in volumes, reciprocal combinations of metallic sesquisulphates, some combinations of platinum, nitrogenised acids derived from acetones, cyanide of ethylene, researches on peptones, &c.

CONTENTS

ERRATA.—In Mr. Broun's article on Cosmic Meteorology, vol. xviii. p. 152, 1st column, line 7, for 464, read 8,464; and p. 153, 2nd column, line 16, for "relate to magnetical and meteorological phenomena," read "relate magnetical to meteorological phenomena."

THURSDAY, JUNE 20, 1878

TERTIARY FLORA OF NORTH AMERICA

United States Geological Survey of the Territories—Tertiary Flora. By L. Lesquereux. Vol. vii. (Washington, 1878.)

THE volume for the present year, of considerable thickness,[1] is entirely occupied by a most important work upon the Tertiary flora of North America. Although the geological and stratigraphical portions of the work are not so distinctly set out as they might have been, this by no means lessens the palæontological value of the work which is more especially in the author's province.

The country occupied by the Lignitic beds, described in the present volume, is a plain stretching for 600 miles between the Missouri and the Rocky Mountains. The ascending gradient is not more than ten feet per mile and the beds are horizontal; the whole Lignitic series is therefore exposed in sequence from east to west, commencing with the Cretaceous lignites, described in a previous volume (which occupy about one-third of the area), and ending with the newer Miocene or perhaps Pliocene beds.

The Tertiary Lignitic coal-fields of Western America alone, were estimated by Taylor in 1848 to occupy the enormous area of 250,000 square miles, or twice the area of Great Britain; but M. Lesquereux supposes that coal-beds of other ages must be included, and he points out (p. 7) that nothing positive was known of the great North American Lignitic until Dr. Hayden began his researches in 1854, and to the remarkable accuracy of whose work he pays a well-merited tribute.

Their area on the Upper Missouri is stated at not less than 100,000 square miles, and in the succeeding paragraph it is estimated at about 140,000 miles. This is their extent in that region of the United States alone, but their actual extent is much greater, since they stretch far across the border into British possessions; they have been traced southward along the base of the Laramie range to beyond the Arkansas River, and by outliers as far as Albuquerque, and again westward they are connected with the coal-fields of the Great Colorado Basin and the Laramie Plains.

Under the heading "Stratigraphy" we are told that there is no unconformability, physical, or other break in the sequence from the so-called Cretaceous lignites to the Tertiary Lignitic. It is then mentioned that there is a transition bed of coarse sandstone, irregularly deposited, whose thickness is not stated. There are next given detailed sections, some of Upper and some of Lower Lignite, none of them, however, presenting a thickness of more than 480 feet. It is strange that only a small paragraph (p. 12), extracted from Hayden's Report in 1874, to which no prominence is given, warns us that the Lower Lignites are of vast thickness—3,000 to 5,000 feet, but whether this thickness is all of Tertiary or the older lignites we are left to ascertain elsewhere, whilst we only incidentally gather that there is an Upper and a Lower Lignite. The faulty construction of the introduction arises from M. Lesquereux having too loosely strung together extracts from various works, which render it so disconnected as to be all but useless except to those who possess a previous knowledge of the subject. The thickness of, and as far as possible, the area occupied by, each division of the Lignite should have been as clearly set out as we find them in Hayden's Report for 1874, which is frequently referred to in the present work.

The third section is almost wholly occupied by verbatim extracts from the arguments of Professors Hayden, Meek, and Cope as to the age of the Lignitic. The conflicting nature of the evidence is apparent, the balance, apart from the botanical evidence, not here referred to, being decidedly in favour of Tertiary age. In the seventh chapter of the Annual Report, 1874, Peale had already given, in tabulated form, the views of authorities in regard to the age of each group of the Lignites which have been referred to Tertiary. In a much condensed form this might have been introduced into the present work with advantage. None of the Lignitic freshwater mollusca have been specifically identified with foreign forms; the vertical distribution of these being well-marked and limited, the evidence they would present should be of great value.

In this section (p. 24) a table is introduced incidentally, showing four groups of strata, which, omitting the lithology, the column of localities and all mention of fossils other than vegetable, reads thus:—

Names.	Fort Union group; Lignite group.	Wind River deposits.	White River group.	Loup River beds.
Sub-divisions.	"Great number of dicotyledonous leaves, stems, &c.; *Platanus*, *Populus*, &c.; with very large leaves of true Fan Palms."	"Petrified wood."	"Petrified wood."	[No vegetable remains mentioned.]
Thickness.	2,000 feet or more.	1,500 to 2,000 feet.	1,000 feet or more.	300 to 400 feet.
Foreign equivalents.	Eocene.	?	Miocene.	Pliocene.

From this table we should certainly conclude that the leaf remains were confined to the "Lignite group," and consequently that the botanical part of the work referred only to fossils of Eocene age. The work being on the "Lignitic" we are still further confirmed in this opinion, and dismiss the overlying "deposits," "group," and "beds" from our minds. With this impression we come to "Part II. *Description of the Tertiary Fossil Plants*," and are in no way prepared for such a surprise as is reserved for us in Part III., 273 pages further on, on finding that the leaves are figured and described from beds of Eocene, Miocene, and Pliocene age indiscriminately.

In the second and third parts M. Lesquereux is evidently working at more congenial subjects, and deserves praise for the painstaking way in which he has described the material before him. The truism that all determinations based upon leaves alone are provisional and untrustworthy, has deterred English botanists from working at our Tertiary floras; and thus while we see magnificent works on this subject brought out in France, Germany,

[1] About 380 pages and 65 plates.

Austria, Italy, Switzerland, and America, no work of any importance has appeared in England. But though they are so, this in no way lessens the value of descriptions and figures for the purpose of comparing different floras, their distribution, the climatal conditions of each age, and inferring the relative age of isolated remains of land surfaces, volcanic outbursts, elevations, &c. It matters less whether a widely spread leaf-form is referred to oak or beech, than to ascertain that it is characteristic of a definite age and had a definite distribution. Fortunately there are palæo-botanists like M. Lesquereux who, having done their utmost to assign a leaf to its right genus, are content to wait for certain proof until the discovery of fruits and more perfect specimens.

Among the more interesting plants described are a *Lycopodium* and three species of *Selaginella*, all well preserved. The discovery of these is remarkable, as M. Lesquereux states that none of the Lycopodiaceæ had previously been known between Oolitic and recent times. The ferns are also of especial interest. Besides *Lygodium* and *Pteris* (which forcibly recall both Eocene and Miocene European forms), we have in *Gymnogramma Haydenii*, from the Lower Lignite, a form also common at Bournemouth (now described from more perfect fronds as an *Anemia*), and to the Aix-la-Chapelle flora as *Asplenium Forsteri*, Deb. and Ett., and to Sézanne as *Asplenium subcretaceum*, Saporta. Conifers and palms are numerous, but being fragmentary are less easy to compare with those of other localities. Still less so are other Monocotyledons, except a single Miocene *Smilax* leaf identified with a European species.

The Dicotyledons are very numerous, 212 species being described. Of these the leaves ascribed to *Myrica*, a large group mostly from the Upper or Miocene series, have an Eocene aspect. The forms assigned to *Betula*, *Alnus*, *Carpinus*, *Corylus*, and *Fagus*, are very similar to each other, and appear, from the figures, to have perhaps been too much subdivided. Any of these might be identified with leaves from Bournemouth, where the beds are undoubtedly Eocene. The leaf-forms from the Lower Lignite described as oaks have been determined with considerable hesitation, and are of most dissimilar character, as we see indicated by such specific names as *Q. platinia*, *Q. viburnifolia*, *Q. negundioides*. They have been classed as oaks on account of greater or less resemblance to species so described by European authors holding very diverse views. The single *Castanea* is indistinguishable from an Alum bay-leaf. In the simple forms of the Saliciniæ we have perhaps the nearest approach in character to European forms, and a large proportion of them will be found identical with leaves from Bournemouth. We must not, however, attach undue importance to the apparent similarity of simple ovate or lanceolate leaves, especially when comparisons are made from drawings only. The poplars form a large group, but, considering the variability of their leaves, species appear to have been unnecessarily multiplied, some being determined from fragments of leaves and others from single specimens, as *P. melanarioides*, *P. melanaria*, &c. *P. Zaddachi* is a familiar leaf in the English Bagshot and Woolwich beds, and in the Arctic beds, so-called Miocene; *P. Richardsoni* also appears identical with this form. Leaves, so similar to each other in outline and variation, that from the plates they can scarcely be distinguished, are described respectively as *Quercus platania*, *Populus lævigata*, and *Platanus Raynoldsii* and we long to know why they are so distinctly separated. The only leaf ascribed to *Ulmus* is from the Miocene stage, but appears identical with a Bournemouth form. Again *Quercus acrodon*, *Planera Ungeri* and *Fagus feroniæ* could all be matched with leaves from Bournemouth, and so resemble each other that it appears strained to have separated them under three genera and to form two new species from such material. On the other hand, one of the five figures (Fig. 5, pl. 28) ascribed to *Ficus lanceolata* is quite distinct from the rest and is certainly not that species, but a common British Eocene form. *Ficus multinervis*, Heer, is one of our most abundant Eocene plants, and, as suggested by Saporta, is more probably a *Laurus*. Leaves referred to *Ficus oblanceolata*, Lesq., analogous to another of our species, might reasonably have been separated as two species. As a group, the leaves called Ficus remind us of those met with in the Eocene. A remarkable feature in the flora is, that but a single form is referred to the Proteaceæ, and from its characters it seems unnecessary to refer even this to that group. Of the Lauriniæ, *Laurus* has an Eocene aspect, whilst the species of Cinnamonum appear correctly identified with Miocene forms. The nine species of *Viburnum*, which would perhaps have been better reduced to two, are analogous to a Bournemouth leaf, but still more so to the *Viburnum* of Sézanne. A leaf referred to the Australian genus, *Caliicoma*, is of simple lanceolate form, with serrated edge, whilst two other forms referred to *Ericaceæ* are very indistinctly characterised. Many of those named *Sapindus* and *Diospyros*, as well as *Zizyphus* and *Rhamnus*, are of essentially Eocene facies and very similar to Bournemouth forms. The leaves placed together as *Ilex dissimilis* are so unlike that it seems doubtful whether they could have belonged to the same species.

The apparent defects which are here pointed out, may be partly due to imperfect figures, and reference to the specimens themselves might uphold the correctness of M. Lesquereux's separate determinations, since the specific identity or otherwise, of leaf forms, is often, after all, very much a matter of individual opinion.

The third part of the work contains a tabulated list of all the plants with the relative position of the beds from which they are derived, and also their possible relationship to those of the Eocene and Miocene strata of Europe. This is followed by a careful digest of the matter contained in the work, from which the following important facts are to be gathered. The lower group contains 200 out of the 325 species described, and is so isolated that but sixteen of the forms pass into the higher tertiaries, and these include none of the essential types, as the palms, magnolias, *Grewiopsis*, *Viburnum*, *Rhamnus*, &c. The second group has thirty-four species, twenty peculiar to it, and is, on the whole, correlated rather with the overlying Miocene than with the Lower Lignitic. The third group is unhesitatingly pronounced to be Middle Miocene, on account of the relation of its plants to the so-called Miocenes of Alaska, Greenland, Spitzbergen, and Europe; and it is further said that no Eocene type is present in the group. The fourth group is also

Miocene, and indicates a temperate climate such as that now prevailing in the middle zone of the United States, as from Ohio to North Alabama. The larger number of its species are identified with, or analogous to, those of Greenland, Spitzbergen, and Alaska, whilst a few are related to Pliocene plants, and three species are still living. Mr. Lesquereux devotes the concluding pages of his work to proving the Tertiary age of the Lignitic series. If, as he states, he has correctly ascertained that, in the first or lowest group, 120 species represent Tertiary, and only six can be considered at all as Cretaceous forms, he has made good his case, and all European palæontologists will agree with his views as to the age of the Lignitic.

The study of a very large series of British Eocene plants in my own collection, from well-defined horizons, has enabled me to draw somewhat different conclusions from those of Lesquereux and Heer. Unfortunately no great and undoubtedly Eocene flora has ever been described or published, and I therefore use the Bournemouth as a typical Eocene flora. The flora of Œningen, made so familiar to us by Heer, is a typical Miocene flora, and although most unlike a true Eocene flora, contains many plants common to other isolated fragments of strata which contain mixed floras, that is, floras with percentages of Eocene as well as Miocene plants. There being no typical series from the Eocene available as a standard of comparison, the plants common to the Miocene have alone been taken to determine the age of these beds, and the unknown Eocene forms have thus been enrolled as Miocene, and in their turn used to identify other still more distinctly Eocene beds as Miocene; much in the same way as the Barton beds were formerly identified, from their possessing a few species in common, as London clay, and the species peculiar to the Barton horizon subsequently made use of to identify Calcaire-Grossier and Bracklesham beds in their turn with the London clay. The errors which have thus possibly been committed even by Heer, who has been led to class all the many floras he has so ably described, either as Cretaceous or Miocene, were therefore unavoidable, and scarcely reflect upon his judgment.

The Lower Lignite is, in my opinion, undoubtedly Eocene, and probably contemporaneous with our London Clay or Lower Bagshot. The sudden incoming of palms and European plants of tropical kinds, and of mammals, and the displacement of the indigenous and temperate flora and of the lingering Dinosaurians, is evidence clear and unmistakable, that the continents became united at this period. Simultaneously with this sudden increase of temperature in America we find a corresponding increase in Europe, as seen on comparing together the faunas of the Thanet Sands and London Clay. The increase was in all probability due to the final rise of a land barrier completely shutting out all the cold northern currents, which at the present day set towards the equator, and so materially modify the ocean temperature. I think we are thus able to fix the comparative age of the Lower Lignitic, which, being upwards of 2,000 feet thick, probably required a great part of our Middle and Upper Eocene period for its deposition. I regard the second group as of our Upper Eocene age, and the third and fourth groups as Miocene.

Now comparing Dicotyledonous leaves of the Lignitic flora with those of Arctic regions, we find several, even of those from the Lower Lignite, common to both. The greater part of these are included in the following list:—

Viburnum Whymperi, Heer.
Fraxinus denticulata, Heer.
Diospyros brachysepala, Al. Br.
Andromeda Grayana, H.
Cissus tricuspidata, H.
Vitis Olriki, H.
Ficus tiliæfolia, Al. Br.

We find common to the Lower Lignitic and the Miocene of Switzerland—

Quercus neriifolia, Al. Br.
" *chlorophylla*, Ung.
" *Godeti*, H.
Salix angusta, Al. Br.
Populus melanaria, H.
" *mutabilis*, H.
Ficus tiliæfolia, Al. Br.
Cinnamonum Scheuchzeri, H.
" *polymorphum*, Al. Br.
Daphnogene anglica, H.
Diospyros brachysepala, Al. Br.
Cornus Studeri, H.
Berchemia multinervis, Al. Br.
Rhamnus alaternoides, H.
" *rectinervis*, H.
" *Rossmässleri*, Ung.

or far more than sufficient to have identified the formation with Miocene had its true position not been otherwise ascertainable. None of these are, however, very distinctive leaves, and, with very few exceptions, they might, had the English Eocene Flora been published, have been referred to it with greater approximation to certainty. The exceptions are of little value, *C. Scheuchzeri* being identified on half a leaf, while the references to *P. mutabilis* and *C. polymorphum* are extremely doubtful. The truth is that so many of the ovate and lanceolate leaves of the Miocene and Eocene resemble each other that it would be easy to compile a sufficient list to refer, with plausibility, any given flora to either age, according to the author's fancy.

I have not yet had leisure to enter more minutely into the question, but it appears to me that the fact of a proportion of the Lower Lignitic leaves, which are of undoubted Eocene age, being also found in the Arctic floras, and the untrustworthy nature of the evidence on which these have been referred to Miocene, still leaves the question of their true age, on palæo-botanica evidence, unsettled. We know that in Eocene times these regions were land, and that floras existed upon them, and passed from one continent to the other, whilst in Miocene times, from the decrease in temperature, we infer that the submergence of this bridge had commenced. Further, the high temperature in the Eocene time would have permitted a temperate flora to grow in these latitudes, and the Miocene temperature would not. Lastly, it is difficult to conceive that the same quality o flora could have grown contemporaneously in latitudes so widely different as the United States and Greenland or Spitzbergen, but there is no difficulty in realising that a decreasing temperature, such as prevailed in the Miocene would have gradually driven the northern forms southward, and thus the very similarity of the Miocene flora of America to that of the Arctic circle renders it unlikely that they were of the same age. J. S. GARDNER

FOURIER'S "ANALYTICAL THEORY OF HEAT"

The Analytical Theory of Heat. J. Fourier. Translated by A. Freeman. (University Press, Cambridge, 1878.)

THERE cannot be two opinions as to the value and importance of the *Théorie de la Chaleur*. It has been called "an exquisite mathematical poem," not once but many times, independently, by mathematicians of different schools. Many of the very greatest of modern mathematicians regard it, justly, as the key which first opened to them the treasure-house of mathematical physics.

It is still *the* text-book of Heat Conduction, and there seems little present prospect of its being superseded, though it is already more than half a century old. It contains the first satisfactory definition of Conductivity, the first statement of the *dimensions* of various physical quantities, and the invaluable expression for periodic quantities in terms of harmonics. Many important problems of heat conduction are completely solved, and the results are given so as to be immediately applicable in practice, as for instance to the cooling of spheres (including the secular cooling of the earth) the propagation of periodic changes of temperature into the crust of the earth, &c.

But the heat equations are of the same form as those in certain other branches of physics. Here they are solved once for all, and form a store from which all may freely help themselves. Thus, a very minute fragment of the work sufficed, by its application to electric currents, to render the name of Ohm famous. More important portions have been applied to Diffusion, to Signalling through Submarine Cables, and to various other important questions.

With all its transcendent excellences this great work had two faults at first, and of late it had acquired a third.

(1.) It was a little prolix. Like Ampère's great work, and some others of that wonderfully fertile period, it was made up as a sort of patchwork of memoirs sent to the French Institute. Each memoir was, as it were, complete in itself: and the putting together into one work, without judicious paring down, necessarily involved a good deal of repetition.

(2.) It was so full of printers' blunders and mere slips of the pen that it must have been very carelessly revised.

(3.) It had become very scarce, and consequently expensive.

The Syndics of the Pitt Press deserve great credit for reproducing the book:—and the printers have done their share of the work well. Still, the result can hardly be called satisfactory. For this there are many reasons.

(1.) We think it was a great mistake to translate the book into English. The poetry, except so far as it was in the formulæ, is gone; and the prolixity, which was tolerable in the original, is painful in the translation. The text should have been considerably compressed in translation, or else simply reproduced in French. Every one who has any right to read Fourier reads French, or at least ought to be able to do so. Again, though *Conducibilité* and *Conductibilité* are good French, Conductibility (being altogether erroneous) has hitherto been confined to the lowest class of English books. CONDUCIBILITY, which Mr. Freeman most commonly employs, is not an English word at all;[1] and, even if it were, could not possibly mean Conducting power, or Conductivity.

(2.) We have compared at least one whole chapter with our own annotated copy of the original. Roughly speaking, only about 50 per cent. of the misprints in the original have been corrected. The others, some very misleading, are reproduced. The worst of those we have noticed are at

pp. 124 [Eq^{n}. (*a*)], 134, 189, 226.

In p. 181 an erroneous reference is reproduced, and in order to make it fit the text the reference mark is shifted from the general equation (really referred to) to a mere particular example.

(3.) The translator has added a few notes, some by the late Leslie Ellis. But they are very fragmentary. Surely more than a single sentence might have been devoted to the experimental results of Forbes [and Ångström]; Stokes and Duhamel ought to have been mentioned with reference to conduction in non-isotropic solids—and Thomson's proof that Fourier's solution of the problem of the cooling sphere is *complete* deserves much more than the mere casual mention it has received.

OUR BOOK SHELF

Anthropology. By Dr. Paul Topinard, with a Preface by Prof. Paul Broca, translated by Dr. R. Bartley. (London: Chapman and Hall, 1878.)

THIS volume forms another of the Library of Contemporary Science, and it purports to elucidate a science which is well described by Paul Broca as being one of vast dimensions and one in process of rapid development, as well as one which has hitherto not received sufficient attention. The masters of the science engaged in original research naturally shrink from the labour of writing a handbook of a popular character: and it fell to Dr. Topinard's lot to make the attempt—in which attempt he seems pretty fairly to have succeeded. This work falls into three sections: the first treats of the physical characters of man, and of his place in nature. The chief human anatomical peculiarities are briefly alluded to, with a somewhat needless—to our mind—reiteration of the assertion that the organisation of anthropoids is a counterpart of that of man, and differs widely from that of the other Simian groups. The second section treats of the races of mankind; and here we have a great many important and interesting facts marshalled in fair order before us. A few more woodcuts would have been an improvement to this portion. In the concluding section the origin of man is discussed; and the author passes in array the monogenestic theory of Quatrefages, the polygenestic theory of L. Agassiz, the transformation theory of Lamarck, and the natural selection theory of Darwin, and works out in detail the application of each to man and his genealogy. The translation, which is generally good, might, however, in places be improved, and it is sometimes a little confused.

[1] On reference to Richardson we find one instance of the use of the word, by (Bishop ?) Wilkins. We freely give Mr. Freeman any benefit which he can extract from the following passage:—

"Duties deriving their obligation from their conducibility to the promoting of ends."

It may interest readers of NATURE to be told that, in looking for the word in the Supplement to the *Imperial Dictionary*, we found the following extraordinary statement (illustrated by a diagram) about Conjugate Foci:—"when rays, falling upon a lens, are so refracted as to converge and meet in a point, either nearer the lens than the principal focus, or farther from it, the point in which they meet, and the principal focus, are called, with respect to each other, *Conjugate Foci*."

The Tailed Amphibians, Including the Cœcilians. A Thesis presented to the Faculty of Michigan University by W. H. Smith. (Detroit, Michigan, 1877.)

THE title of this little volume tells its own story. It is a detailed catalogue of all the species of tailed amphibia known. In addition to using the works of all the best writers on this group, Mr. Smith has availed himself of the specimens in his University Museum, and from these has drawn up many of the descriptions and characters. A number of artificial keys are given to the genera and species; the synonymic lists appear to have been worked out with care, and to have been brought down to date. A list of authors on the subject of the work is appended, and here and there, after the diagnoses of the species, will be found details of their habits, geographical distribution, and development.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Indian Rainfall

AGREEING in the main with the views put forward by Mr Archibald in his letter in NATURE (vol. xvii. p. 505), I beg leave to refer briefly to one or two points in which I differ from him, and I hope that you will be able to find space for this note, because Mr. Archibald has done me the honour of mentioning my name so frequently in his letter, that I might reasonably be supposed to entertain opinions identical with his own on all points regarding the question under discussion.

In the first place I would point out that the atmospheric current which brings the winter rains of Northern India, whilst it has nothing to do with the summer monsoon, does not descend in the Punjab, as Mr. Archibald says, and then proceed eastwards to the North-West Provinces and Behar, and sometimes even as far as Calcutta, but blows in just the opposite direction, appearing as a south-east wind over the Gangetic plain and the Eastern Punjab. The place of its descent in the winter months is farther south, in latitude 22° or 23° N., and thence it flows northwards in almost the same manner as the summer monsoon.

In the next place, I think the hypothesis of the approximately inverse variation of the winter rain, as compared with sun-spots, does not necessarily postulate a corresponding inverse variation of solar radiation. Such a relation I consider to be highly probable, but the somewhat meagre data I was able in a former communication (vol. xvi. p. 505–6) to adduce in favour of it were only intended to prove that the question of "solar activity" was yet an open one, and that it did not follow that solar radiation was most intense at times of maximum sun-spots, because many meteorologists, reasoning from magnetic and other analogies, assumed it to be so. The direct solution of the question must be accomplished by actinometric observations, as Mr. Blanford proposes, and, while it remains unsettled, it will probably be best to try and correlate the variations of rainfall with those of some other meteorological element upon which rainfall depends. I have recently been occupied with an analysis of the rainfall observations of twenty stations in Northern India, embracing between them 11° of latitude and 24° of longitude, and extending over periods of from fifteen to forty-nine years, and I find a remarkable coincidence between the variations of the winter rainfall and those of the temperature of the tropics as given by Köppen in his exhaustive paper in the *Zeitschrift der oesterreichischen Gesellschaft für Meteorologie*, vol. viii., Nos. 16 and 17. When the rainfall deviations of the different stations are thrown into the form of percentage variations from the local mean and are then combined and the results "bloxamed," we get a series of numbers which gives a curve from 1834 to 1877 resembling Köppen's curve very closely, when the latter is extended up to 1877. The two curves not only resemble each other in all their more important fluctuations, but their epochs of maximum and minimum approximately coincide. These are:—

Max.	Tropical Temperature	1842·7, 1854·7, 1865·1, 1876·3(?)
	Winter Rain	1842·7, 1855·0, 1865·5, 1876·9(?)
Min.	Tropical Temperature	1836·9, 1847·7, 1858·4, 1874·8
	Winter Rain	1837·8, 1848·1, 1860·6, 1874·7.

It would therefore appear to be highly probable that the periodic variation of the winter rainfall of Northern India is caused by a corresponding variation in the temperature of the tropics, which determines, within certain limits, the quantity of vapour added to the air and the direction and velocity of the atmospheric currents. It appears, also, from the table, that the maximum of winter rainfall is attained nearly a year *before* the minimum epoch of sun-spots, as given by Wolf. I have found that this is also the case with the winter rainfall of London, and Mr. Draper has shown (NATURE, vol. xvii. p. 16) that the same relation holds good at New York.

The co-existence of severe droughts in Hindustan with devastating floods in Burmah and Assam, is a very strong argument against the theory of Dr. Meldrum that the rainfall of the whole globe varies directly with the sun-spots; but it would naturally follow from the view advocated by Mr. Archibald, because, in very hot years, which are approximately those of minimum sunspot, the general tendency to a cyclonic circulation of the atmosphere round the Asiatic continent in the summer months would be so intensified as partially to obliterate the smaller cyclonic indraught towards Central India, which brings up a moist current from the Bay of Bengal to the Himalaya and the plains of Northern India. S. A. HILL

Allahabad, May 18

A Twenty Years' Error in the Geography of Australia

IN almost every detailed map of Australia, including some of the latest, we find, at the head of the Alligator River, in about S. lat. 13½°, and E. long. 133°, some such note as this:—"Steep walls, 3,800 ft." This is copied from the map illustrating "Leichardt's Journal," published in London in 1847. This map was (as stated in the preface) drawn by S. A. Perry, Esq., Deputy Surveyor-General of New South Wales, from materials furnished by Leichardt, and was engraved in London by Arrowsmith. As Leichardt only returned from his first expedition at the end of 1845 or beginning of 1846 he could have had no opportunity of correcting or revising this map. Mr. James Wilson, the geologist to the North Australian Expedition under Mr. A. C. Gregory, having passed over much of the same country, and finding the plateau nowhere more than 1,600 feet above the sea, came to the conclusion that Leichardt's supposed statement was an engraver's or printer's error which had escaped correction, and gave his reasons for this view in the *Proceedings* of the Royal Geographical Society, vol. i. p. 230, and subsequently in the same society's *Journal*, vol. xxviii. p. 137 (1858). Notwithstanding the extreme improbability—almost amounting to absurdity—of there being precipices of the enormous height of 3,800 feet, in a country where there were no important mountains, and where Gregory, who had passed within eighty miles, and M'Douall Stuart, who had passed within forty miles of the place, found nothing but a moderately-elevated plateau, with ravines never exceeding 600 feet deep, no notice appears to have been taken of Mr. Wilson's correction, but the "3,800 ft." has been copied again and again in works of reputation and authority. We find it even in the new edition of the "Encyclopædia Britannica," art. "Australia," given as an established fact in the following words:—"On the north side of the continent, except around the Gulf of Carpentaria, the edge of the sandstone table-land has a great elevation; it is cut by the Alligator River into gorges 3,800 ft. deep."

The curious thing is, however, that this marvellous phenomenon, which, if it existed, would be unapproached in Australia and equalled nowhere but among the mountains of the great continents, is not even alluded to in the published journal of the traveller who is supposed to have discovered it! On Leichardt's *map* the "steep walls" are noted between the stations of November 10 and 11, but in his "Journal" we find no reference to anything remarkable till November 17, when he comes to the head of a magnificent valley, into which he was obliged to descend, and which caused him much delay and circuitous explorations on account of its steep rocky walls estimated by him to be "1,800 feet high." It is pretty clear, then, that the

"3,800 feet" is a map error, and that even the 1,800 feet is merely an estimate, and probably an over estimate; for we must take into consideration the evidence of other explorers in the same region, and the appalling effects of coming, in a nearly level plateau, to the brink of such a precipitous rocky barrier.

I am making a similar correction to the above by means of a note in a work I am now engaged upon (on Australian Geography), but as the error has obtained such wide circulation and seems so hard to kill, it becomes advisable to call attention to it as soon as possible, and in a way that will be likely to attract attention.

ALFRED R. WALLACE

Opening of Museums on Sundays

YOUR last number contains a letter from my friend Prof. Corfield, which I confess to having read with some little astonishment. He expatiates, and with justice, on the merits of the town of Maidstone, whose citizens do not scorn the grace which "palæontological, conchological, and other collections" must add to life spent in a country "well worth visiting," and who appropriately find their last resting-place in a cemetery "which is one of the most beautiful in the country." I would not demur a moment to such a fascinating picture, were it not that Prof. Corfield, led away by a pardonable enthusiasm, expresses his belief "that this is the first and only scientific museum that has yet been opened on Sunday in the United Kingdom." Surely the Chairman of the Committee of the Sunday Society need not go to Maidstone for the first victory in the very just cause which he upholds, seeing that for the last quarter of a century the three buildings which contain the Botanical Museum of the Royal Gardens, Kew, have been open to the public from two till dusk every Sunday throughout the year.

Royal Gardens, Kew W. T. THISELTON DYER

Socialism in South Africa

I NOTICED this morning that along the bottom of the front wall of my house, on the verandah, there lay a quantity of red-dish-brown powder; there was enough to fill a coffee-cup. On looking closer I saw that it was made up of small and larger fragments which glistened, and on inspecting some in my hand they turned out to be the heads, legs, trunks, &c., of countless ants. A number of these animals were still on the wall above, and my attention being now arrested, I watched them, and saw that they were contributing to the carnage beneath. This species of ant is a small, comparatively harmless one, the chief sin of which is that it makes its way to every species of food and swarms on it. As is usual with ants, the general body of insects is accompanied by larger individuals, which are provided with heads and jaws quite disproportionate to their bodies, and with these jaws they do all the cutting up. Among the ants on the wall there was a large sprinkling of these "soldier ants," and the whole community seemed to be bent on destroying them. The proportion of heavy-jawed to ordinary ants was about one to ten. I saw a group of little ones fastening on to a big one, which made desperate efforts to release itself. At first the big one bit several little ones in two, and the parts dropped down from the wall; but after a while the little ones severed all the legs of the big one, and finally got on his back and cut him in two. The group then dropped down to swell the mass below. Similar scenes were enacted elsewhere on the wall. The commencement of one combat was as follows:—A big ant walked along till it met another big one, and the two shook antennæ. Just then a little one seized hold of a hind leg of one of these big ones. Neither took any notice, but continued a rapid conversation. Suddenly other small ones came up, when the big one whose leg was grabbed turned furiously on the little one and seized him by the middle. This could not be done until the big one had doubled himself up; as soon as he had hold of his small antagonist he lifted him in the air and snipped him in two. Meanwhile all the big one's legs had been seized by little ones, and the party seemed to turn over and over, little bits tumbling down, now a leg, now half an ant, till the big one was vanquished.

The ant is most assuredly subject to passions. The way in which the big ant turned on the little one was singularly indicative of rage. The determined manner in which he laid hold of the little one was quite human. If I had had a magnifying glass, the scene would have been really exciting.

Maritzburg, Natal, May 12 F. E. COLENSO

The Telephone Relay or Repeater

THE writers have been at work since the announcement of the invention of the Bell articulating telephone in endeavouring to devise means by which the telephone might be relayed. Quite a number of devices have been tried, but, from the exceedingly feeble amount of the movements of the diaphragm of the receiving telephone, they have heretofore been unsuccessful in obtaining any practical results.

The discovery by Prof. Hughes of the inexpressibly delicate microphone has given us the means by which we have *finally* at last solved this very important problem. We apply the microphone as a telephone relay or repeater by attaching it directly to the diaphragm of the receiving telephone. The microphone so attached is a miniature one consisting essentially of three pieces of carbon, arranged as described by Prof. Hughes. The two parallel pieces are cemented directly to the telephone diaphragm, and the third piece placed in cavities near their ends. The microphone forms, of course, part of the new circuit in which it is desired to repeat the telephonic message. By the movements of the telephone diaphragm the microphone produces such variations in the electrical current traversing its circuit as to cause the original message to be repeated to any instruments placed therein.

We have tried our telephone relay or repeater on several telephone lines, and find it to work satisfactorily. By attaching a number of miniature microphones to the receiving diaphragm and suitably connecting the battery, increased delicacy will undoubtedly be obtained.

EDWIN J. HOUSTON
ELIHU THOMSON

Central High School, Philadelphia, U.S., June 7

New Form of Microphone Receiving-Instrument

HAVING been experimenting with the microphone, and studying the effect of the passage of the current on a galvanometer, it occurred to me that if the needles were *fixed*, strains would be produced in it by the action of the current. To test this, I passed a few yards of copper wire (about No. 30) on a small bar magnet *lengthwise*, and found, on placing it to the ear, that sounds were heard on interrupting the current; these sounds were much intensified by placing the magnet inside the lid of a pasteboard box.

Having a six-inch horse-shoe magnet beside me, I passed along *one of its limbs* from two to three yards of the same wire, and on placing the lid of a tin box on the *flat sides* of the ends of the magnet, an excellent receiving-instrument was obtained. With this tuning-fork, sounds, singing, whistling, speaking, and violin music were heard distinctly. A single Leclanché coil was used, the transmitter consisting of two small pieces of carbon pencil touching slightly, and connected with an open pasteboard box.

W. J. MILLAR

Glasgow, June 17

A Waterspout

AMONG the meteoric phenomena of which we have heard recently, not the least interesting occurred on Thursday the 14th near the Kelston Round Hill, about three miles to the west of Bath. Shortly after five o'clock in the evening the inhabitants of the village of Weston, which lies between Kelston Hill and Bath, were startled by a volume of water advancing like a tidal wave along the Kelston Road; in a minute the water was upon them, flooding the houses and laying the main street four feet deep under water; with such force did it come that a stone weighing five hundred-weight was carried several yards, while smaller ones were taken a much greater distance.

It was not known in the village from where the water had come, but it so happened that about five o'clock I was proceeding to Weston Station by the Midland Railway from Bristol to Bath, and when in sight of the Round Hill I was struck by the blackness and lowness of the clouds in its vicinity. Suddenly there was a flash of lightning, and immediately after the Hill was enveloped in what appeared to be a storm of rain of unusual density.

On arriving home I was not altogether surprised to find the commotion in the village, and I at once attributed the source of the water to the cloud which I had seen; I therefore made my way in the direction of Kelston Hill.

On arriving under the brow of the Hill it was very clear that something more than an ordinary storm had occurred. Near the

end of a lane (Northbrook) leading to some fields, the hedge on the right for some yards was lying in the road, but the field beyond at this point presented only the appearance of an ordinary storm, while the lane itself was like the bed of a river. To the left was a field of standing grass; for about twelve feet from the hedge the grass remained intact, then for about the same distance it was as though it had been mown down. This torrent, for such it might have been compared to, came to almost a sudden termination a little above the end of the lane, but it extended down the Hill till it was joined by two others, one of which had carried a hedge away bodily.

The increased volume of water then poured down over some gardens, uprooting trees and vegetables; in less than ten minutes the hedges were lost sight of, and the water rose to a height of eight feet. This was occasioned by a block caused by an arch, which carried off the water from a small stream, not being large enough to take the increased volume. Finally it burst over, scooping the ground out in front of some cottages several feet deep and flowed on as a river some yards wide, again destroying gardens in which were valuable stocks of vegetables.

Near this point the volume of water was again increased; in all five distinct water-courses could be made out, all of which had done considerable damage to grass, cornfields, and gardens. Finally, all united in one body and poured into the village of Weston, levelling three walls as it came, and thence passed into the river Avon.

I gather from spectators at Kelston Hill that it began to be cloudy at half-past four in the afternoon; at five there was a rattling clap of thunder, followed by a downpour of rain—in "bucket-fulls," as one expressed it; but all seemed to agree that the greater portion of the water fell under the brow of the hill, where it came down in several columns. There were no houses close to the spot; had there been they must have been washed away.

The atmosphere had been perfectly still all day, but very sultry. Heavy rain fell in the neighbourhood, and the storm to which I have referred specially was accompanied with hail, which in a few minutes covered the ground some inches deep.

What I have described is no doubt what is popularly termed a waterspout.

The damage done was at first estimated at 2,000*l*., but it is now feared that this amount will not cover it.

Weston, near Bath, June 17 E. WETHERED

Fortunate "Escape"

AN evening paper of to-day's date has the following:—

"HOUSE STRUCK BY LIGHTNING.

"During the thunderstorm yesterday, at about 2.30 P.M., a large stack of chimneys at the residence of Mr. Robert Avis, at Putney, was struck by lightning, which split the chimney-shaft down the whole height, the electric current passing down the chimney and into a sitting-room on the ground floor. *The door of the room was fortunately open, and the current escaped without causing injury to the family*, who were in the room at the time of the shock."

The italics are those of one ELECTRIFIED

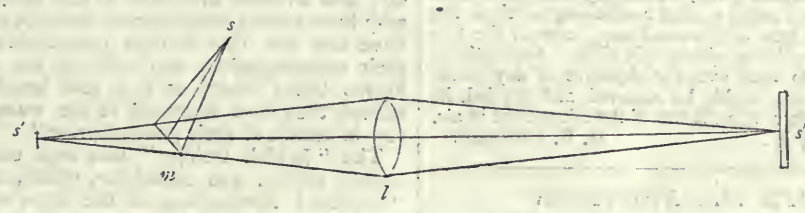
June 17

Velocity of Light

WILL you have the kindness to publish the following as a preliminary announcement:—

The following method of measuring the velocity of light dispenses with Foucault's concave reflector, and permits the use of *any* distance.

In the figure, s is a division of a scale ruled on glass; m, a revolving mirror, l, an achromatic lens; s'', a fixed plane mirror at any distance from l.



The point s is so situated that its image s' reflected in the mirror m is in one of the foci of the lens l, while the image of s' coincides at s'' with the mirror, the latter being placed at the conjugate focus. With this arrangement, when m turns slowly, the light from s'' is reflected back through the lens, so that an image is formed which coincides with s. When, however, the mirror rotates rapidly, the position of m will have changed while the light travels from m to s'', and back again, so that the image is displaced from s in the direction of rotation of the mirror.

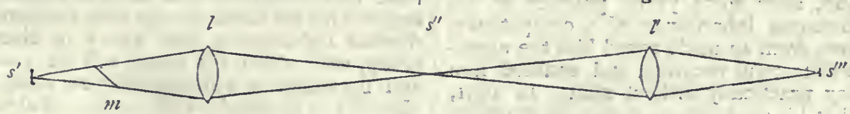
Let V be the velocity of light; D, twice the distance $m s''$; n, the number of turns per second, and r the distance $m s$; then, calling δ the deflection, V is found from the formula—

$$V = \frac{4 \pi r n D}{\delta}.$$

In a preliminary experiment the deflection amounted to five millimetres when the mirror revolved 128 times per second.

The following is another plan which would probably give more light than the above.

s is as before the image of the scale reflected in the mirror m; its image would be formed at s'' by the lens l, and the image of s'' would be formed at s''', where the plane mirror is placed. In this case also, the rays are reflected back, so that the scale and its image coincide notwithstanding the (slow) rotation of m.



ALBERT A. MICHELSON

U.S. Naval Academy, Annapolis, Maryland

University College

THE fiftieth anniversary of the opening of University College falls within this year. It is intended to celebrate the occasion by a gathering of members of the corporation, present and past, professors and masters, old students of the college and school, and other friends and benefactors of the institution, to be held within the precincts of the college, on Tuesday, July 9, at 1 o'clock P.M. The Right Hon. Earl Granville, K.G., Chancellor of the University of London, has kindly accepted the invitation of the President, Council, and Senate to attend and lay the first stone of a further extension of the college buildings and preside at the luncheon; and the presence is expected of many other persons of distinction interested in the welfare of the college and in the promotion of University education.

The space at the disposal of the college, even since the school has been entirely withdrawn to the south wing, is far from adequate to the rapidly increasing requirements of modern education. The Fine Art Department has been obliged to refuse pupils. The Council has, moreover, after prolonged experience

of the satisfactory working of the Ladies' Educational Association, recently decided to open the Faculties of Arts and Law and of Science to women.

Again, there is a very general demand for increased facilities of instruction in engineering and other branches of applied science, which can nowhere be so efficiently met as in connection with a flourishing scientific school like that of University College. Lastly, the numbers of the school have for some years been steadily increasing, and it is not unreasonable to hope that it may soon outgrow its present space. On all these grounds an urgent necessity is now imposed upon the college to undertake a considerable enlargement of its buildings.

Application for tickets should be made to the Jubilee Celebration Committee as early as possible. TALFOURD ELY,
University College, London, June 18 Secretary

Examination of Small Organisms in Water

IN order to examine the minute organisms that inhabit water, such as rotifers, vorticellæ, and kindred microzoons, the arrangement I proposed some years ago in the *Quart. Journ. of Micros. Sci.* will, I believe, be found most convenient. This is to inclose the objective in a brass or other metal tube having its lower end closed by a piece of thin microscopic glass coming close up to but not touching the object-glass. With this protection we can plunge the end of the microscope into a small tank, filled with water, containing the small living organisms, and examine them at our leisure for days or even weeks. The thin glass plate immersed in the water gives us a perfectly steady, flat water-surface, which is not disturbed by any agitation of the surface-water of the tank. Objectives of an inch, half an inch, a quarter of an inch, and even an eighth of an inch focus, may be thus used under water, and all the trouble of catching and ensnaring the small animals is thus avoided. This invention I first employed for the examination of morbid secretions, such as urine. I have since employed it for watching the operations of minute creatures that inhabit water, which may thus be seen in their natural habitat and under normal conditions, which is not the case when they are seen in the usual way, between the two layers of glass on an ordinary microscopic slide. Any optician can make such a tube to screw over the objective of any microscope, and, though it can readily be removed and applied, its presence does not interfere with the use of the microscope in air.

53, Montagu Square, W. R. E. DUDGEON

THE LATE MR. HEWITSON

THE memory of the warm-hearted gentleman above-named deserves a passing notice in these columns, for the effect of his labours on at least one department of natural history has been great. William Chapman Hewitson, who died at Oatlands, near Walton-on-Thames, on May 28 last, aged seventy-two years, was by birth a Northumbrian, and, after the somewhat rough education of a Westmoreland school, took up the calling of a surveyor. His passion for natural history was exhibited in very early life, and, after some years' practice of his profession, the fortunate inheritance of a competence, and something more, from an uncle saved him the necessity of pursuing a distasteful vocation, and enabled him to indulge his fancy practically without stint. In 1831, while still engaged in his professional duties, he projected his "British Oology," the first part of which appeared in April, 1831, and the last in 1838. As he himself subsequently wrote:—

"The book was itself as migratory as the birds, the eggs of which are depicted in its pages; many of the plates were drawn at night after a long day of railway surveying in the fields, and the letter-press was printed wherever the author happened to be stationed at the time. There were few collectors to aid him in those days, and it is with a grateful feeling he remembers now the helping hand which was then held out to him by his friend Mr. Yarrell."

Yet the work was a great success. Such beautiful figures of eggs—all drawn on stone by the author—had never before been seen, for his touch was as delicate as his eye was correct, and great care was bestowed upon the colouring. His zeal for the task he had undertaken, led him with two friends, one of whom was Mr. John Hancock—perhaps the best ornithologist now living—to visit Norway and explore its coasts in quest of those many British birds, of the nidification of which nothing was known except that it was not carried on in these islands. This expedition in 1833 to a country hitherto so little explored by Englishmen as Norway, was no small proof of enterprise, and, with the simultaneous attempt, with a like intent, made in Iceland by Mr. G. C. Atkinson, bore good fruit, not merely in its immediate results, but even long afterwards; for it was doubtless the example of these gentlemen[1] that prompted the subsequent exertions of Wolley, Hudleston, Salvin, Tristram, and others; while the successes in recent years of Alston, Harvie Brown, Danford, and Seebohm, may also be traced to the same cause. The influence has even extended to the United States, as witness the explorations conducted by Kennicott, Macfarlane, and their indefatigable successors under the authority of the Smithsonian Institution. The result has been that the true home of almost every species of bird which inhabits Europe at any time of the year has been discovered, and the same with a large number of those which inhabit North America, and thus, of course, has accrued a great gain to ornithology.

Mr. Hewitson, however, did not pretend to foresee this sequel to his enterprise and that of his friends. His aim was far humbler. In his own words:—

"However unimportant in itself the branch of natural history which I have attempted to elucidate, the beautiful and varied objects which compose it are amongst the first to excite the imagination and call forth in boyhood those feelings, that love for nature, which are inherent in us all; and however the cares or the pleasures of after life may have erased those earlier feelings, there are few who have not one day derived pleasurable emotions from their contemplation, and who do not remember those joyous times when, at the first breaking loose from school, they have hastened to the wood and the hedge-row in search of their painted prize."

The "British Oology" was soon out of print and a second edition was called for, which, under the title of "Coloured Illustrations of the Eggs of British Birds," was begun in 1842 (when the author took the opportunity of publishing a Supplement to his former issue) and finished in 1846, while in 1853—only eleven years later—a third edition was demanded. This, completed in 1856, remains unquestionably the best publication on the subject; for, though the plates were not so carefully coloured as in the second edition, the number of species represented, chiefly owing to the discoveries of Wolley, was largely increased. But in the meanwhile Hewitson's taste had turned towards another department. He had begun with his usual energy that wonderful collection of diurnal *Lepidoptera*, and works in illustration of that group, with which his name will be always associated, and by which it will probably be most widely known. His villa at Oatlands, with its beautiful view and charming garden, was a sight not to be forgotten, to say nothing of the glorious contents of his cabinets. Here he passed the last twenty-five years of his life, or more; seldom leaving home, always glad to welcome a visitor whose tastes agreed with his own, and occasionally returning to his old "flame," when he could thereby assist a friend—as witness some of the plates in the earlier volumes of *The Ibis*. The promulgation and subsequent prevalence of the doctrines of evolution, however, greatly disturbed him; and perhaps the only thing that ruffled his temper was to hear that one naturalist after another had embraced what to him seemed a pestilent heresy.

[1] It is fair to mention that in 1830 Hoy began a series of tours into the Netherlands with the same object, and in 1831 Salmon made an egg-collecting voyage to Orkney and Shetland, but the places they visited bore no comparison in remoteness and difficulty of travelling to those above-mentioned.

So firmly did he stand on the ancient ways that he has been often heard to say—and he may have even expressed the sentiment in as many words in some of his writings—that he could not look into one of his insect-drawers without disgust did he not believe in the direct and independent creation of each individual species. At any rate he never lost an opportunity of avowing his hatred of Darwinism, though his opposition to it made no difference in his feelings towards those of his friends who were Darwinians.

It is understood that before his death he had arranged for the ultimate transfer of his magnificent collection of Butterflies to the British Museum, where, according to the terms of the compact, its present condition is to remain undisturbed for twenty years. Mr. Hewitson, who was buried at Walton-on-Thames, had been a widower for many years and left no children. A portion of his very considerable fortune he is said to have devoted to charitable purposes, but a large portion of the remainder to his old and tried friend, Mr. John Hancock, while his copyrights go to his publisher, Mr. Van Voorst. It is believed also that Mr. Kirby is to make a catalogue of the collection of *Lepidoptera* before it is removed to the British Museum. A. N.

ANDREAS VON ETTINGSHAUSEN

WE regret to record the death in Vienna, on May 25, of Baron von Ettingshausen, one of the oldest of European physicists. He was born in Heidelberg, November 25, 1796. After the completion of his academic studies, he entered the philosophical faculty of the Vienna University as privat-docent for physics and mathematics in 1817. Two years later he accepted the professorship of physics in Innsbruck, but was called back in 1821 to Vienna, to the chair of mathematics, which position he exchanged in 1834 for the professorship of physics. In 1852 he accepted the direction of the newly-grounded Physical Institute, completed its organisation, and raised it to its prominent position as a centre of physical investigation. Some years since he was compelled by increasing age to retire from the duties of his professorship, after a half-century of unwearied activity.

As an investigator Ettingshausen was first known by his mathematical contributions. In 1834 he was one of the first to apply Faraday's discovery of electric induction; and the magneto-electric machine devised by him at this time, and bearing his name, marks an important step in the progress of this branch of physics. Of his later researches we would mention those on the movements in homogeneous systems of molecules, on the parallelogram of forces, on the law of isochronism in the vibrations of the pendulum, and on the formulæ for the intensities of reflected and refracted light, in all of which the mathematical element was predominant.

Ettingshausen's literary work was confined chiefly to his "Vorlesungen über höhere Mathematik," which appeared in 1827; his "Lehrbuch der Physik," published in 1844, and to the editorship of the "Zeitschrift für Physick und Mathematik," from 1826-1832.

As a lecturer Ettingshausen was one of the leading celebrities of the Austrian capital. His auditorium was thronged not only by the students but by the educated classes of Vienna, who were attracted by his rare combination of oratorical power and experimental elegance.

In the Physical Institute he rendered services of the greatest value. For a number of years Vienna was unexcelled in the opportunities it offered to young physicists, and the present activity in physical research existing throughout the Austrian universities is undoubtedly due in a great measure to the healthful impulse given by Ettingshausen a score of years since. It is probably to the same source that we can trace the marked mathematical character of the modern school of Austrian physicists, nearly all of whom have been trained under his eye.

Ettingshausen's varied services made him the recipient of numerous decorations, and some years since he was raised by the Emperor into the nobility. He was a leading member of the Vienna Academy of Sciences, which he assisted to found, and for a long series of years its general secretary. His researches appeared chiefly in its *Sitzungsberichte*. He leaves behind him a son, Baron Constantine v. Ettingshausen, the well-known authority on palæontology.

A NEW CRATER ON THE LUNAR SURFACE

WHEN examining the surface of the moon on May 27, 1877, Dr. Hermann J. Klein, of Köln, observed, with his 5½-inch dialyte by Plössl, a great black crater on the Mare Vaporum, and a little to the north-west of the well-known crater Hyginus. He describes the crater as being nearly as large as Hyginus, or about three miles in diameter, and, being deep and full of shadow, and as forming a conspicuous object on the dark grey Mare Vaporum. Having frequently observed this region during the last twelve years, Dr. Klein felt certain that no such crater existed in this region at the time of his previous observations. Dr. Klein communicated his observations to Dr. Schmidt, of Athens, the veteran selenographer, who assured him that this crater was absent from all his numerous drawings of this part of the lunar surface; neither is it shown by Schroter, Lohrmann, nor Mädler, who carefully drew this region with the fine refractor at Dorpat. On one or two subsequent occasions Dr. Klein obtained further observations of this new crater. He found it to be either without a wall or with a very low one, but to be a deep conical depression in the surface. Shortly after sunrise the crater takes the appearance of a dark grey spot, with an ill-defined edge.

In April, 1878, Dr. Klein communicated his observations to the editor of the *Selenographical Journal*, who at once took the proper steps to have this object observed by the members of the Selenographical Society. The day for observing this region was unfortunately cloudy, and no observations could be made in England, but Mr. J. Ward, of Belfast, caught a glimpse of the moon through a temporary break in the clouds. He at once saw the crater in the position assigned to it by Dr. Klein, and described it as being a black crater with a soft edge. The next opportunity for observing this crater was May 9, but the occasion was not favourable, the sun being then high above the horizon of this part of the moon. The day turned out cloudy. Messrs. Backhouse and Neison observed through thin clouds, and saw in the position of the new crater a dark elliptical spot. On May 11 Messrs. Knott, Neison, and Sadler observed in this place a dark ovoid mark or shading. So far, then, the English observations have been perfectly in accord with those of Dr. Klein, although bad weather has rendered it impossible to see the new crater as a crater.

Mr. Neison repeatedly examined and drew this portion of the lunar surface during the years 1871-1875, and discovered a number of minute details in the region where Dr. Klein has seen the new crater. Quite close to this object are a number of much smaller craters, several under a mile in diameter. Several of these are shown by Schroter, Lohrmann, Mädler, and Schmidt. It may be regarded, therefore, as absolutely certain, that previous to 1876 there did not exist on this portion of the lunar surface a deep black crater of three miles in diameter, and it is thus Dr. Klein describes the new object seen by him. Mr. Neison has expressed the opinion that it is most improbable that he could have missed seeing so conspicuous an object as the present dark marking which it is certain exists now in this region. If, therefore, the existence of

Dr. Klein's new crater be confirmed, it will form the strongest possible evidence of a real change on the surface of the moon, a change, moreover, of a volcanic nature.

The Mare Vaporum in which the new crater is situated lies close to the centre of the visible surface of the moon, so that objects in this region are very slightly affected by the lunar librations. Fortunately it is a portion of the surface which has been most carefully studied by Lohrmann, Mädler, Schmidt, and Neison; for had this new crater of Dr. Klein appeared in a less well-known region, much doubt would have been felt as to whether it had previously existed or not.

DEEP-SEA DREDGING OFF THE GULF OF MEXICO

THE last number of the *Bulletin* of the Museum of Comparative Zoology at Harvard College, Cambridge, Mass., contains a letter from Alex. Agassiz to the superintendent of the United States Coast Survey, detailing the results of some recent dredging operations in the United States schooner *Blake*. A series of deep-sea dredgings were made in the first place across the Florida Channel from Havana to Sand Key, out to the Tortugas reefs, then across the Gulf to the Yucatan Bank, to Vera Cruz, about the Alacran reef and then across the Yucatan Channel, and in the trough of the Gulf Stream to Sand Key, Florida—in all about 1,100 miles of lines taking the shortest distance from point to point. The results of the cruise are full of interest; we can only allude to a few of them. The great Alacran reef is an atol—an atol existing not as Darwin suggests to be the case with atols in general, in an area of depression, but in one of elevation, like those in which the Florida and Bahamas reefs are found. The formation of the Alacran reef is in full activity, the eastern slope is nearly perpendicular, rising to a height of twenty fathoms from the surface in a comparatively short distance. It is exposed to the full force of the north-east trades and the surf breaks heavily against the great masses of *Madrepora palmata*, which build up the narrow line of coral barely flush with the level of the sea. The western slope is much more gentle, and here the reef consists of a number of half-made narrow islands. These are mere strips of sand formed by the breaking-up of the exposed masses of coral, which are gradually cemented together by the accumulation of the loose material held in suspension by the water. Here, in the shallower parts, grow huge masses of Astræa, of Gorgoniæ, of Mæandrina, which now and then rise to the surface.

Along the Cuban coast the dredge brought up immense numbers of siliceous sponges, a species of Favosites, which we are tantalisingly told is perhaps the most interesting coral ever dredged. We presume it was found living, and we all know that this genus was founded by Lamarck for some fossil corals, only found in the very oldest strata (Silurian and Devonian), a young Holopus in excellent condition (probably the fourth or fifth specimen ever found). The dredge worked well to a depth of upwards of 2,000 fathoms. One haul in 860 fathoms brought up an unusually large number of two and one valved mollusca, including many of exquisite beauty. Some most gorgeously coloured crustacea were brought up from a depth of 1,920 fathoms, and what are we to say to an isopod allied to Aega, and upwards of eleven inches in length and three in width? Amongst the strange fish, we read of one like a huge tadpole with a gigantic round cartilaginous head, and without eyes; of another with a drawn-out flat head, very little eyes, but possessed of gigantic filaments, as long as the whole body, and extending from the tips of the pectoral and lower caudal fins. Some of the Holothurians were striped with bands of a deep crimson colour.

Certainly the wonders of the deep-sea are not yet exhausted, and though the treasures found by our own *Challenger* expedition were great, it could reap the produce of but a very narrow belt out of the great expanse of the ocean world.

A steel wire rope was used by Capt. Sigsbee. The time required to reel in was always below one minute per 100 fathoms, sometimes not more than twenty seconds, while the time required to strike bottom averaged thirty-five to forty-five seconds per 100 fathoms in the deepest soundings of 2,000 fathoms. The wire rope was of galvanised steel with a hemp coil; it measured $1\frac{1}{8}$ inch in circumference, and weighed 1 lb. to the fathom, and had a breaking strain of over 8,600 lbs., and its own weight made the use of heavy weights to sink it unnecessary.

The *Blake* is now on a cruise to explore the inner portions of the Gulf of Mexico, commencing with a run from the Tortugas to the mouth of the Mississippi River, in which we wish her crew of all ranks every success.

E. PERCEVAL WRIGHT

METEOROLOGICAL NOTES

MR. ELLIS has made a valuable contribution to the diurnal variation of the barometer in a paper published in the *Journal* of the Meteorological Society of London, which gives the hourly variations from the means of each month as deduced from a discussion of the photographic records taken at the Royal Observatory during the twenty years ending 1873. The forenoon maximum occurs from May to July about 9 A.M., being fully an hour later than at Kew. The morning minimum at the same season becomes less marked than at other times of the year, as happens in situations more or less continental in middle and higher latitudes; and this feature of the diurnal variation is, it may be remarked, decidedly better marked at Kew than at Greenwich. Mr. Ellis gives, for comparison with Greenwich, the curves for Oxford, Washington, Cape of Good Hope, and Ascension, from which he draws the broad conclusion that in high latitudes the forenoon maximum occurs earlier when the sun rises early, it being however omitted to be pointed out that this holds good only in situations more or less continental or removed from the more immediate influence of the sea. Thus the forenoon maximum which occurs at Greenwich at 9 A.M. and at Kew at 8 A.M., is delayed at Falmouth and Valentia to about 11 A.M. or noon; whilst at Helder the time of its occurrence in June is about 2 P.M. The hourly barometric values for the twenty years were arranged with reference to the time of the moon's meridian passage with the result that no certain indication of lunar variation was apparent. We hope that by-and-by the main details of this elaborate discussion will be printed; such details as will embrace, at least, the hourly values of each day and month of the twenty years for the examination of many inquiries referring to both civil and lunar days, which are now rising into questions of the highest importance.

PROF. LOOMIS has recently examined all the cases of violent winds of the United States which have been recorded as having occurred from September, 1872, to May, 1874, the number of cases on which the wind rose to or exceeded forty miles an hour being 250. During the six months from November to April, violent winds were more than five times as frequent as during the other six months of the year. The great preponderance of violent winds are from the north; thus from north-east, north, and north-west, the number were 143, whereas from south-east, south, and south-west, there were only 58. Generally speaking, violent winds increase in frequency and intensity over North America with latitude. Local conditions exercise a considerable influence on the force of the wind. Thus violent winds are of most frequent occurrence near the Gulf of St. Lawrence and the Great Lakes, particularly Lakes Michigan and Erie.

High winds are also frequent along the dry prairie region bordering on the Rocky Mountains. An interesting table is given, showing the relation between the wind's velocity and the barometric gradients, which may be accepted as rough approximations, but the stations in the United States and in Canada from which the isobaric lines have been drawn, are by far too wide apart from each other to supply the data required in dealing with this important phase of weather.

UNDER the heading of a "Brief Sketch of the Meteorology of the Bombay Presidency in 1876," Mr. Fred. Chambers introduces some original suggestions regarding the Indian drought of that year. His method of examination proceeds on the supposition that the droughts of India may be connected with the varying states of the sun's surface as regards temperature, and in the light of the consequences which result from this supposition the observations made in the presidency, from Kurrachee, in the north, to Belgaum in the south, are discussed from which it is shown that the abnormal barometric movements of 1876 were mainly variations in the intensity of the usual seasonal movements; and that, as regards the rainfall, among the causes which produced the drought in 1876, were those very causes which in ordinary years produce a less average rainfall in the eastern than in the western districts of the Presidency. The general conclusion which is drawn, explicitly for the guidance of further inquiry, is that the same principles which explain the usual alternation of the seasons, also explain in a great measure the varying rainfalls of different years. This mode of discussion deserves to be widely adopted in dealing with secular variations in meteorological phenomena, particularly in view of the large scientific issues involved in the relations between the solar and terrestrial atmospheres.

PROF. MOHN publishes a brief account of the fall of the volcanic ash which was shot up into the air during the eruption in Iceland on March 29, 1875 (NATURE, vol. xi. p. 514), and thence carried eastward by the strong westerly winds which then prevailed; and a map is given showing by curved lines the hours at which the ash began to fall along the extensive track stretching 980 miles from Iceland to Stockholm. The most interesting point in the inquiry is the manner in which the lofty mountain range of Scandinavia appears to have influenced the hour of the fall of the ash. Since the time between the ash leaving Iceland and falling on the coast to the east of Stockholm was twenty-seven hours, the mean rate at which it was borne onward was thirty-six miles per hour. During the first twelve hours of its course it drifted eastwards at a uniform rate of fifty miles an hour. It had then approached to from sixty to eighty miles of the mountains of Norway, but thereafter its speed suddenly fell from fifty to twenty-seven miles an hour. The interesting point is that a mountain system such as that of Norway, lying across the wind's path, would appear to exercise a decided influence in reducing the velocity of the aerial current under its level to the extent of nearly one-half, at a distance of sixty to eighty miles to windward.

OUR ASTRONOMICAL COLUMN

THE TOTAL SOLAR ECLIPSES OF MAY 16, 1882, AND AUGUST 18, 1887.—There will be two total eclipses of the sun within the next ten years, which may be observed without entailing a long sea-voyage from this country. The first will take place on May 16 (or May 17, civil reckoning), 1882. In this eclipse the central line commences in long. 3° 11′ W., lat. 10° 40′ N.; totality will occur with the sun on the meridian in 63° 44′ E. and 38° 35′ N., and at sunset in 138° 51′ E. and 25° 25′ N. The duration of total eclipse on this occasion is comparatively short. Probably if observers proceed from England to the central line, they would station themselves in Upper Egypt, not far from one of the points whence the late transit of Venus was successfully observed. In 32° E. and 26° 44′ N. close to this line, totality commences at 20h. 32m. 45s. and continues 1m. 10s. At a point upon the same, not far from Sherm, at the extremity of the peninsula of Sinai, on the Gulf of Akaba, in 34° 28′ E., and 28° 2′ N., the duration of totality is 1m. 17s. The eclipse will be total at Teheran for 1m. 4s. with the sun at an altitude of 67°, commencing May 16, at 22h. 36m. 10s. local mean time; the central line passes about fifteen miles south of this place. A total eclipse may also be witnessed, though for a few seconds only, near Shanghai.

The second of the eclipses to which reference has been made is the one long mentioned in our popular treatises as the next eclipse that will be total in England, but the central line commences in Germany. The following are the elements:—

G.M.T. of conjunction in R.A., 1887, August 18, at 17h. 14m. 33s.

	°	′	″	
R.A.	148	7	45·2	
Moon's hourly motion in R.A.		36	38·2	
Sun's „ „		2	19·5	
Moon's declination	13	33	10·1	N
Sun's „	12	54	5·2	N
Moon's hourly motion in declination		9	13·0	S.
Sun's „ „			48·8	S.
Moon's horizontal parallax		60	12·4	
Sun's „ „			8·75	
Moon's true semi-diameter		16	24·4	
Sun's „ „		15	48·9	

The central eclipse begins at 16h. 10·2m. in 11° 39′ E. and 51° 38′ N., and ends in 173° 47′ E. and 24° 32′ N., and the eclipse is central with the sun on the meridian in 102° 15′ E. and 53° 46′ N.

The following are also points upon the central line:—

21° 36′ E.	53° 50′ N.	34° 15′ E.	56° 5′ N.
24 9	54 21	38 23	56 40
29 19	55 17	42 1	57 7

It will be seen that the track of the eclipse is chiefly over Russian territory. In the longitude of Moscow and in latitude 56° 33′, totality will continue 2m. 30s.; in Moscow itself the eclipse will also be total, though for about fifty seconds only, commencing at 18h. 44m. 40s. mean time, with the sun at an altitude of 17°. By a direct calculation for Berlin a total eclipse for 1m. 40s. results, but the sun is barely at an altitude of 3° when it ends. It is likely that a person wishing to view the phenomena of a total eclipse with the sun at fair elevation, will find Moscow, or its vicinity, the most accessible position; we will therefore append formulæ by which the times of beginning and ending of totality in this neighbourhood, as also the track of the central eclipse, and the north and south limits of totality may be determined, and will also take the opportunity of illustrating the process by a further example, seeing that many experience difficulty in the application of such formulæ. Following the methods described by Mr. W. S. B. Woolhouse in his excellent paper on the calculation of eclipses, transits, and occultations, in the Appendix to the *Nautical Almanac* for 1836, with slight change of notation, we have from the above elements for computation of beginning and ending of total phase at any place not far from Moscow:—

$$\text{Cos. } w = +\ 58{\cdot}7257 - [1{\cdot}92757] \text{ sin. } l + [1{\cdot}43336] \text{ cos. } l \text{. cos. } (L - 75^\circ\ 51'{\cdot}8).$$
$$t = 17\text{h. } 32\text{m. } 29{\cdot}6\text{s. } \mp [1{\cdot}87565] \text{ sin. } w - [3{\cdot}11123] \text{ sin. } l.$$
$$- [3\ 81636] \text{ cos. } l \text{. cos. } (L - 23^\circ\ 34'{\cdot}5).$$

And for determination of latitude of central line, and of north and south limits of totality in any assumed longitude not far from that of Moscow, putting

$$n \text{ . sin. } N = +\ [1{\cdot}92757]$$
$$n \text{ . cos. } N = +\ [1{\cdot}43336] \text{ cos. } (L - 75^\circ\ 51'{\cdot}8).$$

We have

$$n \,.\, \cos.\,(N + l) = \begin{cases} -\,[1·77616] & \text{for N. limit.} \\ -\,[1·76883] & \text{for central eclipse.} \\ -\,[1·76137] & \text{for S. limit.} \end{cases}$$

In these formulæ, as has been previously explained when presenting similar ones, all quantities within square brackets are logarithms; l is the *geocentric* latitude, or the geographical latitude diminished by the angle of the vertical; L the longitude from Greenwich, counted positive towards the east; and t results in mean time at Greenwich.

First, let it be required to find the latitude of the central line and the north and south limits in the longitude of the Observatory at Moscow, 2h. 30m. 17s., or 37° 34′·3 east of Greenwich.

Longitude	+37° 34′·3
Constant	−75 51·8
A	−38 17·5
n. sin. N	+1·92757
Constant	+1·43336
Cos. A	+9·89480
n. cos. N	+1·32816
Tan. N	+0·59941
N	75° 52′·9
Sin. N	+9·98668
n	+1·94089

For Central Line.

Constant	−1·76883
n	+1·94089
Cos. (N + *l*)	−9·82794
N + *l*	132° 17′·4
N	75 52·9
... ...	56 24·5
Add angle of vert...	10·6
Lat. of central line.	56 35·1

For North Limit.

Constant	−1·77616
n	+1·94089
Cos. (N + *l*) ...	−9·83527
N + *l*	133° 11′·0
N	75 52·9
l	57 18·1
Angle of vert. ...	10·5
Lat. of N. limit ...	57 28·6

For South Limit.

Constant	−1·76137
n	+1·94089
Cos. (N + *l*) ...	−9·82048
N + *l*	131° 24′·5
N	75 52·9
l	55 31·6
Angle of vert. ...	10·8
Lat. of S. limit ...	55 42·4

In this manner by assuming other longitudes near that of Moscow we trace out the belt of totality.

Next, to find the times of beginning and ending of the total phase at any point in the vicinity. Calculating for the observatory of Moscow, the geographical latitude of which is + 55° 45′·3, we proceed thus:—

Geographical latitude...	+55° 45′·3	Constant ...	−23° 34′·5
Angle of the vertical ...	10·7	L	+37 34·3
Geocentric latitude (*l*)..	+55 34·6	B	+13 59·8

Constant	−1·92757
Sin. *l*	+9·91639
	−1·84396
No.	−69·8167
	+70·7604
Nat. cos. *w*	+ 0·9437
Log. cos. *w*	+9·97483

Constant	+1·43336
Cos. *l*	+9·75228
Cos. A	+9·89480
	+1·08044
No.	+12·0347
Constant	+58·7257
	+70·7604

Constant	1·87565
Sin. *w*...	9·51962
	1·39527
No. ...	24s·8

Constant	−3·11123
Sin. *l* ...	+9·91639
	−3·02762
No. ...	−1065s·6
	−3593s·8
	−4659s·4
	h. m. s.
	−1 17 39·4
Constant	17 32 29·6
	16 14 50·2
Long. E.	2 30 17·0
Middle..	18 45 7·2 Moscow M.T.
	∓24·8
Totality begins	18 44 42·4 ,, ,,
Totality ends.	18 45 32·0 ,, ,,

Constant	−3·81636
Cos. *l* ...	+9·75228
Cos. B...	+9·98691
	−3·55555
No. ...	−3593s·8

GEOGRAPHICAL NOTES

THE *Japan Gazette* publishes an account of a visit recently paid by a Japanese steamer to the Bonin Islands, about which but little is known. Some eighteen months ago the Japanese took possession of the islands (which are in N. lat. 27°, about 520 miles from Yokohama), and established their head-quarters at Port Lloyd, Peel Island, which is the only harbour in the Bonins. The islands are described as high, rocky, and even mountainous; and the shores are, for the most part, precipitous, and lined with coral reefs. The vegetation is chiefly tropical, palms of various kinds being abundant. Wild goats and pigs abound on all the islands, and deer on one of them. Lemons, sweet potatoes, bananas, Indian corn, &c., thrive there; but the attempt to introduce cocoa-nut trees has not yet proved successful. On the return voyage the steamer visited the outlying Japanese island of Hachijo, which has an area of forty miles, and is said to contain 10,000 inhabitants. It is mountainous, and its sides to a great extent precipitous. At the northern end of the island there is a volcanic peak, rising to a height of 2,800 feet above the sea. The roads on the island are mere narrow and stony paths, and the people are poor. Three-fifths of the population are said to be women. Almost every available spot on the hill-sides in Hachijo is terraced and cultivated, but sufficient rice cannot be grown, so that sweet potatoes form one of the principal articles of food.

AT the meeting of the subscribers to the African Exploration Fund held the other day, a resolution was passed to adopt the route recommended by the Committee, from Dar-es-Salaam, towards the northern end of Lake Nyassa, and thence, if possible, to the south end of Lake Tanganyika. The return journey might be made as far as possible along the valley of Lufigi. As we have already intimated, Mr. Keith Johnston, with whom will be associated another European, will lead the expedition, which will probably leave England in October next.

THE distribution of prizes of the Geographical Society of Paris, which had been postponed owing to the forthcoming exhibition, will take place at the Sorbonne on the 27th inst. Mr. Stanley, it is understood, will be present to receive the gold medal awarded him. The National Geographical Congress will take place in the beginning of September in the hotel built by the Paris Geographical Society, and which will be inaugurated on this occasion. It is said on good authority that the presidency of that Congress will be given to M. de Lesseps.

THE GREAT FROZEN SEA [1]

MANY readers, we believe, will prefer this brief brightly written narrative of the last English Arctic Expedition to the two weighty volumes of Sir George Nares's, recently noticed in these pages. Capt. Markham is an enthusiastic Arctic explorer, and as these volumes testify, is well fitted by his personal qualities, his experience, and accomplishments, to take a leading part in work of this kind. He has evidently a thorough knowledge of Arctic work and a full appreciation of the kind of observations which ought to be attended to in an Arctic expedition. His interesting volume affords a very satisfactory idea of the incidents of the expedition and of the nature and amount of work done.

Capt. Markham's name must be known to all as the leader of the sledge party that attained the highest northern latitude, and as might be expected, his pages contain an impressive narrative of the adventures of the party. As one reads the story of this heroic attempt to reach the pole he is not merely surprised that the party turned when they did, but that they did not resign the attempt at the end of the first week, for it must then have become evident that the goal was unattainable by that route at that season with the means at command. Had the men not been made of splendid stuff, physically and morally, they could not possibly have endured the terrible hardships described by Capt. Markham. Certainly Sir George Nares did not exaggerate when, in addressing his men before leaving England, he told them "that if they could imagine the hardest work that they had ever been called upon to perform in their lives intensified to the utmost degree, it would only be as child's play in comparison with the work they would have to perform in sledging."

Capt. Markham seems to think that work by the

Highest northern camp, 33° 20′ 26″ N. lat.

Smith Sound route is practically complete; and he leaves one with the impression that it would be useless to attempt to reach the pole by that route. Assuming that the attainment of the pole is in itself a worthy object for an expedition, we are inclined to think that the conclusion as to its unattainability has been too hastily drawn from the experience of one expedition. At the same time we quite agree with Capt. Markham that there are other routes which, while they hold out some hope of a successful passage to the pole, would also afford opportunities of obtaining valuable scientific observations. Capt. Markham rightly says that Behring Strait is a portal leading to a vast region, the history of which has hitherto been as a sealed book. This, it is stated, is the route to be followed next year by the expedition to be sent out by Mr. Gordon Bennett. Mr. Bennett is having a map of the polar regions constructed for the purpose of showing the effect of the various currents towards and from the polar area, and, if one may judge from this, there is much to say in favour of the Behring Strait route; but all such polar-current charts must be regarded with grave suspicion, as being founded so largely on conjecture. We quite coincide with Capt. Markham's strong advocacy of the route by Franz Josef Land. So far as known at present, that, we think, is the best basis of operations for further work towards the north. Perhaps we may hear of something important being done in this direction by the Dutch Expedition which recently went out in the *Willem Barentz*.

Capt. Markham gives a fair idea of the kind of scientific work carried on by the expedition, and we hope that the many magnetical, hydrographical, meteorological,

[1] The Great Frozen Sea. A Personal Narrative of the Voyage of the *Alert* during the Arctic Expedition of 1875-76. By Capt. A. H. Markham, R.N. (late Commander of H.M.S. *Alert*). (London: Daldy, Isbister, and Co. 1878.)

and other physical observations which were made will be published in well-arranged form. At the furthest point reached, a bread bag filled with the scrapings of the pannikins and a little pemmican was lowered to the bottom of the sea, and, having been kept there for some hours, was hauled up, and was found to be almost alive with numerous small crustaceans and foraminifera. With the thermometer a series of temperatures was taken at every ten fathoms, while the specific gravity of the surface-water was also obtained. Tidal action was apparent, though it was impossible to collect any exact data.

Capt. Markham, like his relative, Mr. C. R. Markham, is evidently of opinion that the Eskimo entered America from Asia, spreading eastward, and finding their way to Greenland by crossing at almost 81° 54′. This is, we confess, the theory which most readily presents itself, but those who have studied the subject most deeply, and in all its aspects, have come to the conclusion that the Eskimo are virtually indigenous, and came northwards from the American continent itself, the migration being from America to Asia, and not the other way. Indeed, some ethnologists go so far as to maintain the essential unity of origin of all the American families, and that all the differences in physique, language, &c., may be explained by differences of environment. In the case of America, probably, more than anywhere else, language is a really important factor in the ethnological problem. (See Prof. Sayce's article last week on "The Ethnology of North-West America.")

Capt. Markham gives an extremely pleasant account of the winter amusements on board the *Alert*—the Royal Arctic Theatre, the Thursday Pops., the school for the men, &c. The last-mentioned institution appears to have been a great success, and we are sure the men will feel the benefit of it all their lives. One feature of the Thursday Pops. we must mention with special approval; except on the evenings exclusively devoted to the legitimate drama, these entertainments were always preceded by a lecture delivered by one of the officers on some interesting and at the same time instructive subject, adapted to the knowledge and intelligence of the audience. In this way thirteen lectures were given altogether, and with the exception of one on a historical subject by Mr. White and one on Sledging Experiments by Capt. Nares, they were all on scientific subjects. Capt. Nares began the series by a lecture on Astronomy, which was followed by lectures by the other officers on Magnetism, Geology, Meteorology, Steam, Mock Moons under the Microscope, Light, Astronomy again, Food in the Arctic Regions, Arctic Plants, Hydrostatics. Indeed it is difficult to conceive that more could have been done to enable the expedition to pass as cheerful a winter as possible under the circumstances.

Altogether Capt. Markham's work is a thoroughly interesting and instructive narrative of a memorable expedition. The numerous illustrations and the maps add considerably to its value.

ON THE STRUCTURE AND DEVELOPMENT OF THE SNAKE

IN my paper on the skull of this type (see abstract *Proc.* Roy Soc., January 10, 1878, pp. 13-16) I spoke of the snake as "lying at the very base of the *gill-less* vertebrata, and possessing a skull at once the simplest and yet the most curiously specialised," of any of the many kinds I have worked out.

As far as existing forms of reptiles are concerned, the snake does lie at the *very base*, yet, on the whole, I am inclined to add it to the other limbless lizards, such as the blind-worm and the amphisbæna, and to consider it, therefore, as a lizard which has had its limbs starved out for special purposes.

Much of the cranio-facial axis of the snake remains in a very primordial condition, but the outworks of the skull are modified to such a degree that "the power of nature could no further go."

I have not yet worked out the skull in the amphisbænidæ, but I expect to find it to have many things in common with that of the serpentiform amphibia, the "Cœcilians."

But the "Anguidæ," taking the common blind-worm (*Anguis fragilis*) as an example, are merely "Scincoids" that have dropped their limbs but retained their limb-girdles: they are *lizards* to all intents and purposes, and the native kind only differs from its quadrupedal relatives, in possessing an additional segment ("mesopterygoid") in the "pterygo-palatine" arcade, a segment common in osseous fishes and birds, but suppressed, as a rule, in the scaly reptiles.

As to that which is *archaic*, the chameleons so common in Africa, and the unique New Zealand Hatteria (*Sphenodon*), these outliers of the lizard tribe are evidently more generalised than the serpents.

But all these forms—snake, tropical lizard, legless lizard, and old aberrant lizards—all these come as close to the bird as the *pupa* of a dragon-fly does to the *imago* of the same insect.

With regard to the earlier stages and to the mode of development of the embryo, generally, I have stated in my paper (pp. 9 and 10), that "As to the general embryological study of the snake's embryo, it may be remarked that it is almost exactly that of the birds. Comparing my own observations on this low type with the results given in the study of the chick in Foster and Balfour's excellent work, I find that few paragraphs in it would need any material alteration, and that the figures would mostly serve very accurately if in that work the word *chick-* were to be exchanged for that of *snake*-embryo. The development of the vesicles of the brain, the organs of special sense, the rudiments of the cranium and face—those things that come across my path, to say nothing of the rest of the growing germ, all are developed similarly in the snake, below, and in the bird, above."

If this be so, the modifications undergone afterwards, in the specialisation of the skull and skeleton generally, and in the appearing and packing of the enclothing muscular masses, those "cunning machines" that do the gymnastics of the body—the development and endless modifications of these parts must be of the greatest interest.

I must refer to Professor Huxley's paper "On the Classification of Birds" (Zool. *Proc.*, 1867, pp. 415-418) for a comparison of the bird with the reptile, and for the reasons existing that have led modern anatomical zoologists to put the reptiles and birds into one group, viz., the "Sauropsida." [1]

With regard to the loss of limbs it is not a little remarkable that, on the theory of the "Ratitæ" being parental to the "Carinatæ," in the bird class, that pair of limbs which was to be most metamorphosed was not quickened into new life until it had died. Morphogically, the wingless *Dinornis* stands directly beneath the whole of the "winged fowl" known to us, and the steps and stages from that monster up to the sun-bird and the humming-bird are very gentle and gradual.

But there were *reptiles* in the olden times "that spread their limber fans for wings," and there were true birds also which had evidently only just escaped from the reptilian territory, as the *Archæopteryx*, for instance, and these are seen to be actually modifying the paw into a *wing*.

Perchance the birds grew out from many a kind of old generalised reptile; yet, be this as it may, the eagle himself is not a more powerful or beautiful creature than a python or a boa, nor is there much more to wonder at in

[1] That account of the "Sauropsida" needs a little modification in the light of newer discoveries. I have given such an *improved* account in my article on the Anatomy of Birds in the ninth edition of the "Encyclopædia Britannica," vol. iii., p. 278.

the manner in which the morphological force has enclothed a vertebrated animal in the case of the bird than in that of the huge "creeping thing."

Certainly the skull is in some respects much more simple in the serpent than in the bird, for the bird having built up its skull with all the old reptilian architectural elements, afterwards blots out their distinctness for the most part, and only leaves marks here and there of the early subdivision of the parts.

But this is due to "the hot condition of their blood" and, especially in the higher kinds, the "altrices," the life-vessel of a bird almost literally boils over; in a few short weeks the shapeless embryo of a swallow or a swift is able to join the "airy caravan" of its migrating parents

Head of Embryo Snake, 1 inch long, magnified 8 diameters.

and relations "high over seas," and in far distant countries seek for perpetual summer.

The great serpent, I ween, took a century or two to finish in its fulness his huge bulk; time, so important to the "turtle, and the crane, and the swallow," could be of no importance whatever to pre-Adamite boa-constrictors and pythons. Was not the whole jungle theirs, and theirs also every kid and fawn, to say nothing of the luckless unwary bird?

That the spinal column is as complete and beautiful a piece of machinery in a boa-constrictor or ordinary snake as in the bird there can be no doubt.

Talk of specialisation! Why, Prof. Owen's terms for the parts and processes of a snake's vertebra would take

Embryo of Snake, ¾ inch, magnified 8 diameters.

up half a column in a scientific glossary. I will give a few of his terms:—"Neural-spine," "neurapophysis," "post-zygaphophysis," "præ-zygapophysis," "zygosphene," "zygantrum," "procœlous," articular cup of "centrum," posterior ball for next cup of "centrum," "neural canal," oval articular head for ribs on each "diapophysis," and oval concavity on head of rib.

Four hundred vertebræ, most of which have all these parts! Surely this creature was made by Nature herself, and by no "'prentice hand."

The sinuous cylindroidal facets, fore and aft, on the bird's centrum are not a whit more perfect than the cup-and-ball of the snake's vertebra; and in all respects the articulation of the serpent's spine is so exquisitely perfect as to beggar all human inventions of joints and hinges.

Only just a little motion of joint on joint is allowed, each joint set into the other, so that nothing can part them without crushing them entirely; and yet a most perfect and delicate motion of cup in ball, wedge in cavity, and of the oblique overlapping facet on the oblique facet beneath it—all these are harmonised together, and just allow a gentle bend of bone on bone, and a gentle rolling of vertebra on vertebra.

Multiply by 400 this limited motion, this arrested curve, and you get a motion such as would, if likely to be applied to you, personally, make "all your safety to lie in remotion, and your best defence absence." The curve, so small as made by one joint bending on another, would, in its sum total, be sufficient to engirdle a luckless anatomist several times over.

In the bird's head nearly all the fair details of its early architecture are plastered over by *periosteal* bone, by the ruthless processes of a steady *ankylosis* that removes landmark after landmark.

Not so in the serpent, although, with a wise prevision (enough to satisfy the most craving teleologist, who, wondering, asks you if you see no *design* in Nature), ankylosis comes in to perfect the "strong box" in which this wise [cunning] creature keeps its limited brain.[1]

The organ of its mind is thus safely lodged so that no foot may crush or wild beast break its casket; thus with its "cruel venom" the adder "bites the horse heels so that the rider falls backwards," and is in no fear of that heaviest of all feet, the foot of the *soliped.*

The (relatively) deaf adder has its ear-organs encased in adamant; they with the cranial bones are "shut up together as with a close seal. One is so near to another that no air can come between them. They are joined one to another; they stick together that they cannot be sundered."

So much for the cranium proper; but how about the *face?*

The *face* is a loose framework of bones tied together into one piece of work by an infinite amount of "yellow elastic tissue," and the opening of the capacious "maw" is surrounded and defended by bars of ivory-like bone, many of which are beset with *retral* teeth pointed like needles and sharp as lancets.

Your serpent, with all his wisdom, does not "mouth" his words; he only hisses; but he *mouths* his prey as no other creature does; and the "shirt of Nessus" was not a more dreadful robe to wear than the distensible body of a python, inclosing, ingulphing, suffocating, and digesting its limp and helpless prey.

With regard to the relation of the snakes to the existing lizards, it is a remarkable fact that, whilst they have no tympanic cavity, in which character they agree with *Sphenodon* and the chamæleons, yet a small *cochlea* buds out from the vestibule, and there is to it a "fenestra rotunda." The chamæleon is void of this structure, and thus in that respect is as low as a frog.

The lower jaw and its pier (quadrate) was altogether directed forwards in the early embryo of the snake; afterwards the pier and the free mandible are articulated at a very acute angle, the squamosal touches the temporal regions by its apex, and to its base the long rib-like quadrate is articulated.

The quadrate thus is made to pass over the "columella auris," which also is directed backwards; on that rod there was a small "stylo-hyal"; the quadrate picks up this *useless remnant*, and glues it, by partial ankylosis, to its inner face.

Thus the counterpart of the human "styloid process" is ankylosed to the bone that answers to the *head* of the "malleus."

W. K. Parker

[1] I am frequently asked whether I believe in *design*, and am always at a loss how to answer the question, it seems to be to me so perfectly gratuitous. If the questioner would but give me time, I would promise to write him a book upon the fitnesses to be seen in a *frog* or even in a *flea* that should be as large as a family Bible.

A FOSSIL SPARROW-LIKE BIRD

WE recently referred to a new genus and species of Passerine bird, described by Mr. J. A. Allen from a specimen found preserved in the insect-bearing shales of Florissant, Colorado. We give an illustration of these remains, which consist of the greater part of a skeleton, embracing all of the bones of the anterior and posterior extremities (excepting the femora). Unfortunately, the bill and the anterior portion of the head are wanting, but the

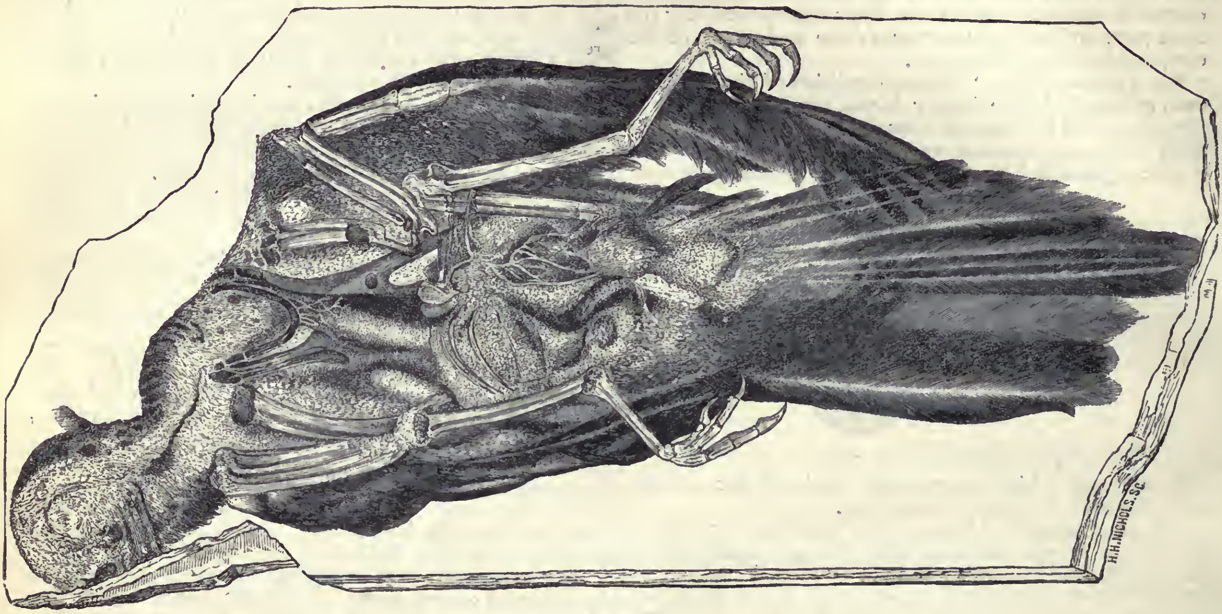


FIG. 1.

outlines of the remainder of the head and of the neck are distinctly traceable. The bones are all *in situ*, and indicate beyond question a high ornithic type, probably referable to the Oscine division of the *Passeres*. The specimen bears also remarkably distinct impressions of the wings and tail, indicating not only the general form of these parts, but even the shafts and barbs of the feathers.

In size and in general proportions the present species differs little from the Scarlet Tanager (*Pyranga rubra*) or the Cedar-bird (*Ampelis cedrorum*). The bones of the wings, as well as the wings themselves, indicate a similar alar development, but the tarsi and feet are rather smaller and weaker; and hence in this point the agreement is better with the short-legged Pewees (genus *Contopus*). These features indicate arboreal habits and well-developed powers of flight. The absence of the bill renders it impossible to assign the species to any particular family, but the fossil on the whole gives the impression of Fringilline affinities.

It is called *Palæospiza bella*. Its wings are rather long and pointed; the tail is (apparently[1]) about two-thirds the length of the wing, rounded or graduated, the outer feathers (as preserved) being much shorter than the inner. The feet and toes it will be seen are strictly those of a perching bird, and the proportionate length of the bones of the fore and hind limbs is the same as in ordinary arboreal *Passeres*, especially as represented by the *Tanagridæ*.

The most remarkable feature of the specimen is the definiteness of the feather impressions. Both the shafts and the barbs are shown with great distinctness in the rectrices, and the tips of the primaries of one wing are also sharply defined, overlying the edge of the partly-expanded tail. The tip of the opposite wing can also be seen beneath the tail. The feet are so beautifully preserved that even the claws are perfectly distinct (Fig. 1).

Another very imperfect specimen from the same locality, and probably representing the same species, consists of the tip of the tail and about the apical third of a half-expanded wing (Fig. 2). In this example the tail is also pointed and graduated.

FIG. 2.

The larger specimen, that first described, is divided into

[1] The character of the tail must be given with reservation, since it is not quite certain that the whole of the tail, or that the exact form of the terminal portion, is shown, especially as the preserved impression is somewhat unsymmetrical.

an upper and a lower half, the greater part, however, adhering to the lower slab. The bones adhere about equally to the two faces. The drawing is made from the lower slab, with some of the details filled in from the upper one. The feather impressions are about equally distinct on both, and where in either case the bones are absent exact moulds of them remain, so that the structure can be seen and measurements taken almost equally well from either slab.

The species here described is of special interest as being the first fossil Passerine bird discovered in North America, although birds of this group have been known for many years from the tertiary deposits of Europe.

The author is indebted for the opportunity of describing these interesting specimens to Mr. S. H. Scudder, who obtained them during his last season's (1877) explorations of the Florissant insect-beds. The specimens are now the property of the Boston Society of Natural History.

NOTES

A TELEGRAM from Sydney, dated June 17, announces the death of the Rev. W. B. Clarke, the eminent Australian geologist. Mr. Clarke was a Fellow of the Royal Society.

AT Gotha a monument erected in memory of the well-known naturalist, Prof. Johann Friedrich Blumenbach, who died at Göttingen, in 1840, was unveiled on May 19. It consists of a gigantic block of stone bearing a portrait of Blumenbach and an inscription, and was executed after the design of the eminent architect, Herr Eelbo.

THE next session of the French Association for the Advancement of Science will be held at Paris from August 22 to 29. The presidents of sections have been appointed by the general committee. Among them we find the names of MM. Cornu, Quatrefages, Bertillon, Maunoir, Wurtz, Hervé-Mangon, Baron Thenard. It is stated that for the first time each of these presidents will deliver an introductory address on the work of his section, after the example of the British Association.

TWO Japanese astronomers Janagi and Issono, are busily engaged in studying the equipment of our European observatories, and the best methods of conducting observations. At present they are visiting the Seeberger observatory at Gotha. After an extensive summer tour they intend to spend the autumn in Berlin, a city for which Japanese students in various branches of science seem to have a peculiar liking.

THE scientific demonstrations, which we announced as being organised in connection with the Paris Exhibition, were commenced on June 17 by the Anthropological Commission. Scientific explanations will be given four times a week, from ten o'clock by three professors of the Anthropological School of Paris: Monday and Thursday on Prehistoric Anthropology by M. de Mortillet; Tuesday, on Demography, by Dr. Bertillon; and Friday, by Dr. Topinard, on General Anthropology. The General Association for Lectures and Promenades has been authorised by the Minister of Public Works to complete its organisation, and its programme will be published soon. No fee is taken beside the charge of the usual admittance ticket, 10 deniers, collected at the gates of the Exhibition.

THE Committee of the Meteorological Congress, which will take place in Paris at the end of August, under the presidency of M. Hervé-Mangon, have issued their programme of questions.

THE first of the International Congresses arranged for by the French Government has taken place at the Trocadero. The Société des Agriculteurs de France took the initiative under the presidency of the Marquis de Dampierre, the Prince of Wales and Lord Lyons being present. But the attendance was very limited, not more than five or six hundred persons being present in a room fitted to accommodate many thousands. The number of delegates of French and foreign agricultural associations was 112, a large proportion belonging to English societies. The General Secretary delivered an elaborate address in which he reviewed the condition of agriculture in the world generally and principally in England, which may be considered as the home of modern scientific agriculture. The ordinary meetings of the Congress take place in the Pavillon de Flore, Tuileries, and the concluding sitting will be held in the large hall at the Trocadero. The same organisation has been adopted for all the congresses belonging to the Exhibition. The *Journal Officiel* has published their dates and details of organisation.

THE Paris Prefect of Police has granted the authorisation for the creation of a club of students (Cercle des Écoles). This institution is organised by a committee of *bonâ fide* students and professors of the several Government schools and universities, among them being MM. Littré, Hervé-Mangon, Acarias, Wurtz, Robin, Paul Bert, &c., &c. The Minister of Public Instruction has sent his approbation. Social, political, and religious discussions will be strictly forbidden in the institution. It is the first time, at least during the present century, that such an authorisation has been given in Paris.

WE learn, with pleasure, that at a meeting held at Barrow-in-Furness, on June 3, the Committee of the Naturalists' Field Club belonging to that town determined to organise a scheme for sending representatives (artisans, if possible) to the Paris Exhibition, with the view of collecting information in connection with the various branches of science which are there practically illustrated, one of the conditions being that the result of the observations should be imparted to the club in the form of lectures during the ensuing winter. Promises of substantial support have been received from several of the leading men in the district, and the scheme is expected to be shortly in working order.

WE have often had occasion to refer to the progress of science in New Zealand. Our contemporary, *The Colonies and India*, has, in a recent number, an article on education in New Zealand, from which we gather the following facts:—It seems that upwards of 600,000 acres of land is now set apart to provide funds for these educational establishments. Our contemporary may well ask, "Compared with this, what are the endowments made in this or in any other country in the Old World? What may not be hoped from such a commencement, and from a people possessed of such foresight and liberality?" There is a university established with a Royal Charter whose degrees are recognised as equal to those of the English universities. As yet it is only in its infancy. Having no examiners of its own it has still to conduct the examinations for degrees, through means of the professional staff of the colleges which are affiliated to it. The Canterbury College is thus united to it, where the course i cludes classics, mathematics, modern languages, history, English literature, natural philosophy, political economy, and jurisprudence. This college has received as an endowment 350,000 acres of land, judiciously selected in various districts, and producing a rental of several thousands per annum. In the course of years this will no doubt prove to be of enormous value. "It is open to purchase, at any time, at the rate of 2*l.* an acre; 700,000*l.* is therefore the maximum at which this endowment can arrive. In addition to this there is also a landed endowment for educational purposes, including not only the elementary schools but those of technical science, for classics and superior education, a museum and library, a college of agriculture, and a normal school for the instruction of teachers, a most useful idea." Besides these there is the

Canterbury museum and public library, and various similar institutions in the country towns. Lectures are given in the museum; and it is hoped that in course of time the library will become as large, or at least as useful, as those of Melbourne and Boston. Twenty scholarships of 40*l*. a year, tenable for two years for students of schools, colleges, or under private tuition, have already been founded by the Board of Education, and it is intended to increase the number. At Dunedin, the capital of Otago, which is chiefly a Scotch settlement, the same eagerness for education prevails. There is a university and a school of art, a boys' and girls' high school, and district grammar schools; besides which there are athenæums and public libraries in nearly all the country villages. "Here, as at Canterbury, large landed endowments have been made for the above-named objects. Two hundred thousand acres have been settled upon the university. The buildings have already cost 30,000*l*.; they are handsome and well-situated. As yet the number of students does not exceed eighty, to instruct whom there are five professors in addition to one of moral and mental philosophy, endowed with 600*l*. a year by the synod of Otago. A valuable library is attached, which it is intended shall be utilised as a free public library. Although this has been styled a university, it can only be looked upon as a college affiliated to the University of New Zealand. A Royal Charter has been refused to it, and its degrees are not recognised. Nearly one thousand of the elder pupils at the other schools receive, at the school of art, instruction in freehand drawing, painting from copies, from nature, and from the human figure, designing, practical geometry, perspective, mechanical and architectural drawing. In the provinces of Wellington, Nelson, and Auckland there are collegiate bodies affiliated to the University of New Zealand, and there are also provisions for elementary instruction. The general dissemination and desire for knowledge, it is said, is "laying a sure foundation of a people able to conduct their own affairs, and giving promise of a bright future in what has well been termed the Great Britain of the south."

WE understand that Mr. Thomas Denman, Lecturer on Physiology at the Birkbeck Institution and Physical Science Lecturer at the Working Men's College, has compiled a Glossary of Biological, Anatomical, and Physiological Terms, which will shortly be published in a small crown 8vo volume by Messrs. Griffith and Farran.

THE Chinese coast was visited by a terrific cyclone on April 12. It appeared to take its origin about fifty miles from Macao, and moved directly northwards, devastating everything within a path of about 700 feet in width. The European settlement on the Island of Schameen was reduced to a ruin, and the havoc created by the storm in Canton and the neighbourhood is beyond calculation. The loss of life is estimated at 6,000 to 8,000. An eye-witness states, in a letter to a Vienna journal, that the cyclone was immediately preceded by a hail-storm, the temperature being at 80° F.

MR. TALFOURD ELY, the Secretary of University College, London, asks us to state, to prevent misunderstanding, that the admission of women to classes in that College does not apply to the Faculty of Medicine, but only to the Faculties of Arts and Law, and of Science.

DURING the past year the Austrian Educational Department has maintained a party of geologists in Northern Greece for the purpose of preparing a reliable geological chart of this part of the kingdom, a district which, until within late years, has been almost entirely closed to scientific examination. A portion of the results have been submitted to the Vienna Academy recently in the form of a paper on the "Geological Structure of Attica, Bœotia, Locris, and Parnassus," accompanied by a number of barometric measurements of the heights of Greek mountains.

IN 1866 the Swiss government took active measures to preserve the numerous erratic boulders scattered over the country, and its efforts have been so ably seconded by the cantonal natural history societies that the most important of these silent witnesses to ancient glacial action have been carefully sought out and protected from destruction. The geologists of France have, as we intimated some time ago, lately awakened to the necessity of making a similar provision for the numerous erratic masses in the departments adjoining the Vosges, the Alps, and the Pyrenees, many of the most valuable of which have already been appropriated for building or other purposes. It is but lately that the immensity of the glacial action in eastern France has been comprehended. For the past ten years the two geologists, MM. Falsan and Chantre, have been occupied in a thorough study of the great movements which once took place in the valley of the Rhone. Their results are embodied in six large maps, on a scale of an inch to the mile, which give a careful reproduction of the striæ, marking the progress of glaciers over the rocks in the valley of the Rhone. From their investigations it appears that the ice in the neighbourhood of Grenoble possessed a thickness of over 3,000 feet, and that the glacier formed an enormous fan-shaped mass, bounded on one side by the alps of Savoy and Dauphiné, and on the other by the mountain ranges of Beaujolais and Lyonnais, and extended beyond Thodure. For the careful mapping of the movements of the Rhone glacier not only the abundant heaps of pebbles and the striæ have rendered the chief service, the erratic blocks have at every stage played a most important *rôle;* and it is to be hoped that the efforts now set on foot will preserve to coming geologists the means of thoroughly tracing the paths of the great glaciers in other parts of the country.

IN the last Annual Report of the Prussian Commission for the Scientific Examination of the German Sea-coast, we notice an interesting comparison of the relative results obtained from equal areas of (1) fish ponds, (2) grazing districts in Schleswig-Holstein, and (3) fishing grounds off Hela. The latter covered a surface of 7,200 hectares, and supplied, in the course of a year, 456,000 pounds of fish as the result of 3,405 expeditions. As contrasted with each other, per hectare, the land yielded 167 lbs. of meat, the fish-pond yielded 153 lbs. of fish; and the sea-fisheries yielded 63 lbs. of fish annually. This is the first effort to establish a comparative estimate of the value of fisheries, and affords some idea of the sources of wealth at the disposal of maritime nations, even when contrasted with the adjoining land.

THE Geographical Society of Vienna has conferred the title of honorary member upon Prof. A. Bastian and Dr. Brehm.

On May 16 a meeting of friends of natural history was held at Dresden, when a resolution was passed to found a society for the establishment and maintenance of an aquarium in that city.

THE *Électricité*, a scientific paper which was started two years ago by Count Halley Darroz for promoting a special electrical exhibition, will resume its publication on July 1 next, to promote a similar project to be executed at the Paris Palais de l'Industrie in 1879.

A NEW work on Russia, entitled "Das malerische Russland," is about to be published by B. M. Wolff, of St. Petersburg. The editor is Herr P. Semenow, Chief of the Russian Statistical Department. The work will consist of four volumes, and will contain over 500 illustrations.

M. DE FONVIELLE writes that he has learned by private letter from Philadelphia, and from a design published by the *New York Daily Graphic* that Prof. Ritchel succeeded in directing balloons in the interior of the permanent exhibition building on May 22 last. About the same time M. de Fonvielle witnessed an experiment by Capt. Annibal Ardisson in the Paris hippodrome, which

was successful also so far as demonstrating the possibility of motion; but the apparatus was so imperfect that the balloon moved very slowly indeed, and another apparatus has to be made by the French experimentor. Instead of using common lighting gas, Prof. Ritchel resorted to pure hydrogen. His balloon had only 3,000 cubic feet measurement whilst Capt. Ardisson's wanted about 11,000. Capt. Ardisson's motor was composed of two very imperfect fans worked with the hand. Prof. Ritchel used a screw propeller moved with both feet, so that he had his hands free for working a horizontal fan, for ascending and descending at pleasure. Instead of constructing a spherical balloon, Prof. Ritchel had prepared a cylindrical one similar to the balloon *Delamare* tried fifteen years ago without success, in the open air. It is stated that Prof. Ritchel's success was very great, and the experiment will be tried again in Philadelphia, and probably soon in Paris. These experiments, M. de Fonvielle thinks, disprove the scheme advocated by the head of the French balloon service, Col. Laussedat, who, in a paper recently referred to in NATURE, suggested that the motive power should be applied to the balloon instead of being annexed to the car.

A VALUABLE sketch of the development of the natural sciences in Holland, has lately appeared in Leyden from the pen of Dr. B. van Haan.

THE late investigations of Count Wurnbrand, on the *loess* formations of the Danube in Moravia, lead him to the opinion that these deposits are entirely of an alluvial origin, and not due to diluvial disturbances. A large variety of fragments of charcoal, carved bits of bone and horn, flints, &c., accompanying the collections of animal remains found in these strata, point with great certainty to the existence of mankind at the time of their formation.

AN interesting archæological discovery is chronicled by the Berne papers. A forest in the neighbourhood is found to grow above a buried Roman town. Numerous edifices have been laid bare, and the various remains which have been unearthed show it to have been inhabited by the officers of the Roman forces, who occupied the strong defensive positions on the river Aar.

AMONG the more important scientific novelties in the German book trade during the past month, we notice the following works:—"Die Dolomit-Riffe von Südtirol und Venetien," 1ste Lief., Dr. E. von Mogsisovics (Vienna); "Die Reptilien und Fische der böhmischen Kreideformation," Prof. A. Frie (Prague); "Die Erdrinde und ihre Bildung," J. Lippert (Prague); "Vorträge über Geologie,". F. Henrich (Wiesbaden); "Die Geologie und ihre Anwendung auf die Kenntniss der Bodenbeschaffenheit der oesterr.-ungar. Monarchie," F. von Hauer (Vienna); "Exkursionsflora für Mittel- und Norddeutschland," Exkursionsflora für Suddeutschland," Dr. M. Seubert (Stuttgart); "Taschenbuch der deutschen und schweizer Flora," E. Hallier (Leipzig); "Flora von Deutschland," Prof. A. Garcke (Berlin); "Die Schule der Physik," J. Müller (Brunswick); "Grundzüge der Elektricitätslehre," W. von Beetz (Stuttgart); "Lehrbuch der Physik," F. J. Pisko (Brünn); "Sonne und Monde als Bildner der Erdschale," J. H. Schmick (Leipzig); "Ueber Meerströmungen," E. Witte (Pless); "Anleitung zum Experimentiren bei Vorlesungen über anorganische Chemie," Prof. K. Heumann, III. (Brunswick); "Anleitung zur quantitativen chemischen Analyse," Prof. C. R. Fresenius, II. 2 (Brunswick).

We have upon our table the following books:—"Outlines of Physiology," by Dr. McKendrick (Maclehose, Glasgow); "Choice and Chance," third edition, by W. A. Whitworth, M.A. (Deighton, Bell, and Co., Cambridge); "A Library Map of London and its Suburbs," by J. B. Jordan (Stanford); "A Geological Map of England," by Prof. Ramsay (Stanford); "A Geological Map of Ireland," by Prof. E. Hull (Stanford); "Grundzüge der Electricitatslehre," by Dr. W. von Bertz (Stuttgart); "A Candid Examination of Theism," by Physicus (Trübner and Co.); "A School Flora," by Dr. Marshall Watts (Warne and Co.).

THE additions to the Zoological Society's Gardens during the past week include a Black-faced Spider Monkey (*Ateles ater*) from East Peru, an Ocelot (*Felis pardalis*), a West Indian Rail (*Aremides cayennensis*), a Black Tortoise (*Testudo carbonaria*), a Common Boa (*Boa constrictor*) from South America, presented by Capt. J. Moir; a Himalayan Bear (*Ursus tibetanus*), an Indian Crow (*Corvus splendens*) from India, presented by Capt. J. S. Murray; a Rufous Rat Kangaroo (*Hypsiprymnus rufescens*) from New South Wales, presented by Mr. Thos. Wickenden; Six Herring Gulls (*Larus argentatus*) European, presented by Mr. Arthur Clarke; two Black-crested Cardinals (*Gubernatrix cristatella*) from South America, an American Thrush (*Turdus migratorius*) from North America, presented by Mrs. Arabin; a Black Saki (*Pithecia satanas*) from the Lower Amazons, a Spotted Cavy (*Cœlogenys paca*), a White Ibis (*Ibis alba*) from South America, purchased; a Chimpanzee (*Troglodytes niger*) from West Africa, deposited; a Reeves's Muntjac (*Cervulus reevesii*) born, six Upland Geese (*Bernicla magellanica*), a Brazilian Teal (*Querquedula brasiliensis*) bred in the Gardens.

THE MICROPHONE[1]

A LATE member of the present ministry, at a dinner given by the institution whose hospitality we experience in this hall, implied, on the authority of one of the leading members of the engineering profession, that invention, like cocktails and Colorado beetles, had taken root in America and had deserted old England. It is therefore to me, as I am sure it is to you, a great gratification to have brought before us an invention which is the offspring of British soil. During the last few months the science of acoustics has made marvellous and rapid strides. First of all we had the telephone, which enabled us to transmit human speech to distances far beyond the reach of the ear and the eye. Then we had the phonograph, which enabled us to reproduce sounds uttered at any place and at any time; and now we have that still more wonderful instrument, which not only enables us to hear sounds that would otherwise be inaudible, but also enables us to magnify sounds that are audible; in other words, the instrument which I shall have the pleasure of bringing before you to-night, is one that acts towards the ear in the same capacity as the microscope acts towards the eye.

I may point out, in the first instance, that the telephone and the phonograph depend essentially upon the fact—and a great fact it is—that the mere vibration of a diaphragm can reproduce all the tones of the human voice. In the telephone the voice is also made to vibrate a diaphragm, which, by completing an electric circuit, or by varying a magnetic field, or by altering the resistance or electromotive force of the circuit, produces effects at a distance which result in the reproduction of the motion of the diaphragm. But in this new instrument diaphragms are cast aside, and we have the direct conversion of sonorous vibrations, or sound waves, into forms of electrical action.

Now, if it had been the habit or the custom of this Society to give to the papers and discussions delivered here sensational titles, I should have been inclined to call the few remarks I am going to make to-night, "A Philosopher Unearthed." Prof. Hughes is well known to us all; he has been more or less associated with this Society since its first inception. Whenever he is in London he is amongst us. His instrument is well known to us as one of the most exquisite pieces of mechanism ever invented; and his works, though few, are known because they are sound. The chief characteristic of this philosopher whom I have succeeded in unearthing, is his extreme modesty. If he had been left to himself, I do not think we should ever have had the microphone here; but, by a lucky chance, he admitted me into his secret, and following, as I have done, all his steps, I am

[1] A lecture given before the Society of Telegraph Engineers, on May 23, by W. H. Preece, Vice-President Soc. T.E., Memb. Inst. C.E., &c., &c.

enabled to-night to bring before you the results of his labours, and they have been labours indeed. For months and months he has been working and straining at the ideas which at last he has elaborated into the microphone.

Now the chief characteristic of the apparatus I am going to introduce to you to-night is its great homeliness, its uncouth roughness, and its absurd simplicity. With common nails, with small pieces of wood, with halfpenny money-boxes, with plain sealing-wax, with the ordinary apparatus which every child has at its command, he has been able to attack nature in her stronghold—to ask her questions and receive back answers, and lay bare to us facts and thoughts which, though they have existed from time immemorial, are brought to light now for the first time.

Now, let us in the first place ask ourselves this question: What is sound? It is a very difficult question to answer in the short time at my disposal; but it is necessary that I should first say something to you about the nature of sound, and then say something about the nature of electricity, and show you how the one can be converted into the other.

Now, what is sound? While I am speaking to you I am setting the air in this room into vibration. The air of this room is composed of an infinite number of infinitely small molecules; every molecule is set in motion, and vibrates to and fro, backwards and forwards, like the bob of a pendulum, and between my mouth and every one of the ears in this hall there is a rapid but short excursion to and fro of every single molecule that comprises the atmosphere of this room; and it is the impinging of these molecules against the drum of the ear that produces that sensation called *sound*. But more than that, not only is the air of this room in this marvellous state of motion, but every piece of wood, every wall, every picture, everything in this hall at this moment, is almost, I may say, alive, trembling away, moving backwards and forwards, forming what are called sonorous vibrations. If the sound be loud enough, and the note deep enough, we can distinctly feel these vibrations. Sound is therefore the vibration in particular periods and particular phases of matter.

Now what is electricity? Faraday, the greatest electrician perhaps that ever lived, was asked that question, and he said the more he studied electricity, the more he unravelled its mysteries, the more mystified he became as to its cause and its origin; therefore it seems an act of impudence on my part or the part of any one else to attempt to answer the question, What is electricity? But great strides have been made since the days of Faraday; we know a great deal more now of the internal molecular action of bodies; we know that light, and heat, and sound are the mere action of those molecules of which matter is composed, and we feel sure, from the facts brought to our notice by the delicate apparatus of the present day, that electricity is simply a mode of motion, nothing more or less than the simple play of the molecules of matter. The truth of this will be made evident to-night by the wonderful connection which exists between sound—which we know to be a mere mode of vibration—and electricity, which will reproduce to us the effects of sound. To make this evident to us we must have a detector which will render apparent to us any electrical action that shall result in sound, and it fortunately happens that this marvellous telephone is an instrument of such extreme delicacy that it has made us acquainted with currents of electricity hitherto unknown, though their presence has been suspected. The telephone which Prof. Hughes has employed in his researches is as simple in its construction as all his other apparatus. It consists of two rough pieces of board clamped together. There is half the coil of an electro-magnet that probably has been in his possession since his early experiments to judge from its appearance. The magnet is a piece of steel rod that has been magnetized. The wire used, and which he has found extremely useful, is wire that was originally made for very different purposes, viz., for ladies' bonnets, and in front of this is placed a piece of ferrotype iron, well-known by those who have experimented with the telephone.

But what is the source of sound? It was necessary in making these experiments that he should have a source of sound. His source of sound was a small mantelpiece clock of French manufacture, which cost originally three or four francs. It has been in use many years, and has been in many parts of the world. It is repaired with great lumps of sealing-wax, but nevertheless it has, or ought to have, a pendulum, which gives a succession of beats, and those beats form a source of sound. Now, with this source of sound, and his beautiful scientific apparatus or detector, he started upon one of Sir Wm. Thomson's discoveries, viz., that wires alter their electrical condition when they are placed under strain. He took a piece of wire, applied weight to it, connected the clock with it, and heard nothing. He was not disconcerted, he applied weight after weight till he reached the breaking strain of the wire, and at the moment when the wire broke, he heard a rush or sound which he thought was an indication of what he was searching for, so he took the two ends of his wire and laid them together, placed his source of sound above them, and to his intense delight heard—what imagination perhaps assisted him in believing to be—a tick. He thought he was on the right track, and he then manufactured with a flat piece of brass for a lever, a pin for an axle, sealing wax for cement, and black wax for solder, and the uncovered bonnet wire for binding, a little apparatus which enabled him to apply constant pressure to the thing he was experimenting upon; in fact, by this means, he was enabled to produce what electricians call a "bad joint." To his intense delight he found that with this bad joint he was able to obtain sonorous effects. But this contrivance, simple as it appears, was a great deal too elaborate and complicated for his purpose, so he took two little nails—the little bright nails so much used in France—laid them side by side, not touching each other, and bringing the ends of the wire in contact with them, and laying between, or across them, a third and similar nail, he was able to reproduce, almost perfectly, the sound of the clock, and more than that he began to get indications of the sound or tone of the voice. He then used chains; he took my gold chain and put it beneath his little compressor, and with that we were able to speak with great ease. From that he tried filings, and found with matter in a finely divided state, that he was able to reproduce all effects of sound. At last he made little glass tubes about two inches long, filling them with white bronze powder which artists use, which is a mixture of zinc and tin, and he was able to exactly reproduce the tones of the voice. But in his experiments with carbon he was able to make what may be called quite an independent discovery. The carbon he experimented with was the common carbon used by artists in sketching their drawings, and this carbon he found to be a non-conductor of electricity. The idea struck him that this non-conductor of electricity might be made a conductor, and by various processes he at last arrived at a plan of boiling or heating this carbon in quicksilver. Carbon so heated in an atmosphere of quicksilver itself becomes permeated through and through with quicksilver, and by that means we get the mercury subdivided into an infinitely fine state. Probably mercury in this state as closely approaches the molecular as anything can do.

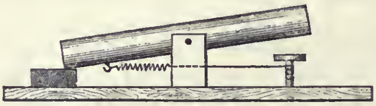


Fig. 1.

There is no apparent indication of mercury under the microscope, and yet we know that the carbon has been mercurised, because it is converted from a complete insulator into a conductor, and it has a metallic ring when it falls. Now then, having by these processes arrived at a substance which is remarkably sensitive to all the variations of the sound of the human voice, his next task was to construct these things into such a form as to make them telephonic transmitters. For that purpose he brought to his aid a very cheap kind of apparatus, a halfpenny money-box; inside this he placed his carbon transmitter, and as this discovery is not fenced in by fear of the patent or any other law, I am quite sure you will be glad to know how to make a Hughes transmitter. First he takes a piece of quarter-inch board about two inches long and one inch broad, and he raises upon that two thin brass bearings with a hole worked through by means of a pin for the support of the axis. He then takes a piece of carbon which has been mercurized about two inches long, which has a pin cemented to it near its centre, and which acts as an axis, and makes it into a lever. On the board he places a small piece of carbon, similarly treated, and upon this rests another similar sized piece of carbon, the two being connected by a piece of paper.

This is the end of one wire, and that the end of another wire; and on this diagram the arrangements of the circuit

are shown. S represents the source of sound, which I have shown on the black board. B represents the battery, and T the telephone. Now the battery is another remarkable speci-

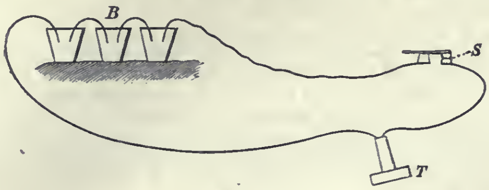


FIG. 2.

men of scientific manufacture. Three little glass tumblers are taken; at the bottom of each a coil of copper wire is coiled spirally. The copper wire is covered with a little sulphate of copper. The tumbler is then filled with moistened clay, and upon the top of the clay is placed a piece of scrap zinc. The three cells are placed in a cigar-box. S is what is called the box-transmitter. The tube-transmitter is shown on this diagram.

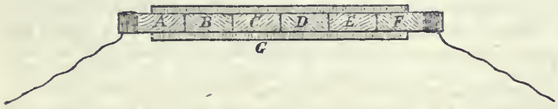


FIG. 3.

A is a glass tube about two inches long, and one quarter inch in diameter, inside which several pieces of mercurised carbon are inserted, touching each other with a pressure regulated by a screw fixed to each end. In that drawing there are six pieces of carbon, acting in this little transmitter; here (pointing) there are seven or eight, but that makes little difference, and the size of the carbon appears to be of little consequence. He has produced effects with carbon not larger than a pin's head. We shall show this by and by, but rather than disturb the order of these researches, I think it advisable in the next place to show you how this principle has been carried a little farther to produce what he calls the "microphone." This apparatus, drawn upon the board, is extremely sensitive. It will give evidence of nearly every sound; but in the microphone itself, which I have here, this extreme sensitiveness is carried to a still further extent. In point of fact, this is a microphone; but in this particular instance the pressure bearing between the two carbons is regulated by a spring fixed in this way, and it is so regulated that the transmitter is independent of any position in which it may be held. It is free to be moved in any direction in consequence of the pressure of the spring, but in one form of instrument this spring is dispensed with, and the pressure between the carbons is reduced to its greatest sensitiveness by making the two arms of this lever as short as possible. In the first machine he used, a piece of carbon was fixed on the top of an upright board, and a smaller piece was fixed down below. A cup-shaped hole was made in the upper piece of carbon, and a similar one in the bottom piece. Resting in these holes was a lozenge-shaped piece of carbon; and this lozenge-shaped piece of carbon rests with the greatest nicety upon its lower support, and is just in that position of equilibrium that the slightest atmospheric disturbance produces the effects which we are now about to show you. I think it desirable to tell you that you must not to-night expect distinct articulation. We have made a violent effort to make these experiments evident to you all. (Illustrations were given of speaking, singing, &c., &c.)

Now, the effects you have just heard have been produced by a transmitter similar to that drawn on the board. We will now repeat the effects with the machine on the table; and in order that you may judge of the effect—for Prof. Hughes desires that you should see there is no deception—we will connect this up, and use his old friend the clock to make its ticks, if it will, evident over the whole room. One of the greatest effects which this instrument produces is to render evident the tramp of a fly; and we have some nice little captives with which we will demonstrate that effect at the close of the meeting. (Illustration with clock.) To show that that is not due to the clock itself, Prof. Hughes will lift up the clock, when all traces of sound will have disappeared, and on putting it down again the sounds will be produced; so that the sound you hear is the sound of that clock which has been magnified. (This was so.)

Now, we have here a common quill pen, and Prof. Hughes will do as they do on the stage, pretend to write a letter; and I have no doubt if you listen attentively you will hear the scratching of his pen. (Illustrated.)

There are some peculiarities in this apparatus that are very striking. In the first place, though the sounds produced are very great, they do not interfere with each other. If you have a friend at the other end speaking to you, you can hear his voice distinctly working through your voice; and the result is you get a duplex action. Two or three persons can talk to each other without impediment or confusion.

Yet another point is, that the articulation is absolutely perfect. One of the great difficulties, both in the telephone and the phonograph, is getting the sibilant sounds reproduced—such as "s," and "c," and "sh," &c., which are produced by such extremely minute variations of the sonorous vibrations that they are lost in those instruments. Thus, if through the telephone you ask a person to "waltz," it will come out "walk," and names like my own, with the sound of "s" in it, would come out "Pree," not "Preece." In this transmitter one of its chief peculiarities is the fact that all sounds are faithfully reproduced; and it tends very much to upset the notion—Helmholtz's theory—that vowel sounds and other sounds are due to the superposition of waves upon waves of tones and over-tones. This apparatus shows almost unquestionably all these different properties; all these effects of intonation are due to differences in the form of the curve sent. Another peculiarity is this. I have told you that all in this room, every one's body while I am speaking, is alive with sound. If you take this transmitter and place it in front of your mouth, or put it on your forehead, or on the top of your head, or put it into your pocket, or upon your breast, it will still transmit sounds to distant places. Put it in a room, it does not matter where, it will reproduce the sounds. Put it anywhere in a drawing-room where there is a piano, you will hear the sounds of the piano faithfully reproduced. It is as you see a marvellously rough affair. You may throw it up, kick it about, or do what you like with it, it will always act. Here is the identical box that Prof. Hughes made two or three months ago. It has never been touched, it has been always at work, and never needs repair.

These are some of the peculiarities of his instrument, and I daresay some of you would like to know a little about its theory. We have here two points in contact, and those two points in contact complete an electrical circuit. The electric current that flows through that circuit depends for its strength entirely upon the obstacles or resistance in that circuit to the flow of the current; an alteration in any shape or form in the resistance of that circuit will result in the increase or decrease of the strength of the current flowing, and upon this diagram I have made a rough attempt to give you an idea of what occurs.

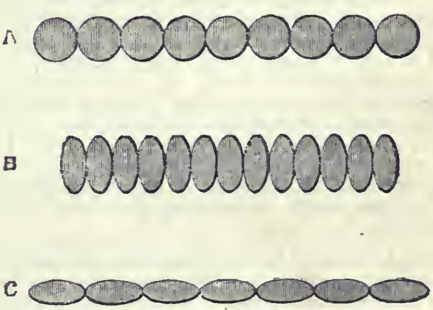


FIG. 4.

You must not conceive these round balls are molecules themselves, they are merely meant to represent the sphere of action of each molecule. In a normal state the molecules rest against each other, as shown by the upper line. When from any cause pressure is increased, they are contracted, as shown in the second line; when from any cause the pressure is decreased they expand, in the form shown on the other line. While I speak to you, the air of this room is thrown into vibration, the mass of air being subdivided into molecules in compression and molecules in extension. In a long wire these successions of compressions and extensions compensate each other; but when we break up a body into infinitely small parts, when we make less contact between two bodies, as shown there, and isolate the

portion of the sonorous wave in compression from that in extension, the result is that we have a variation in the resistance of the line. Now this variation in resistance depends upon the compression and dilatation of the molecules. They depend upon the tone of the voice, and the result is the resistance of the current varies with its variation of pressure, and at the distant end we have currents varying exactly as the voice varies, and reproducing on the telephone all the effects which we have seen. Hence follows the action of the microphone, and the action of the transmitter is one which depends upon the variation produced in bodies by the sonorous vibrations of the voice. As I am now speaking at that telephone, all the molecules of that transmitter are thrown into this elaborate series of compressions and dilatations. The current is varied; it goes to the room below, and is reproduced upon the telephone, as we have heard. Hence the effect is due to the difference of pressure, as is proved by using atmospheric pressure, and applying heat; and any large increase of pressure results in sound being reproduced.

No one has ever been nearer a great discovery than Mr. Edison. His telephone is based on the variation of resistance due to pressure. He used carbon and finely divided matter, but he worked on the idea that the difference in pressure was produced by the vibrations of a diaphragm. Had he thrown away his diaphragm he would have forestalled Prof. Hughes in this respect, and found that the sonorous vibrations themselves produced this difference of pressure. The great secret of Prof. Hughes's discovery is that sonorous vibrations and electrical waves are to a certain extent synonymous.

Now as to the uses to which this instrument is capable of being applied. It has been applied to surgical purposes in the form of the stethoscope. Though it does not show very markedly the beats of the heart, because they are more mechanical thumps than sonorous vibrations, yet it will show the injection and ejection of air in the lungs, and for many other surgical purposes it must become a valuable instrument. It admits us to some of the mysteries of insect life, and by its means we can hear sounds emitted by insects which have never been heard before. Going further it has enabled the deaf to hear; deaf persons who never heard a telephone before have been able to hear distinctly. It has enabled us to hear the physical operation which goes on in the process of crystallization of bodies and other things which before were wholly inaudible; and in fact it is impossible to say to what uses it may not be put.

It is rather remarkable that in an excellent paper read before the American Electrical Society, the author, Mr. Pope, makes these curious remarks:—

"The most striking results are to be looked for in the direction first pointed out by Mr. Gray, for the reason that if an effectual method of controlling the resistance of the circuit by means of atmospheric vibrations can be discovered, the source of power, which in this case is the battery, may be augmented to any required extent. It is not to be denied that the problem thus presented is one of exceeding mechanical difficulty, but there is no reason to suppose that it may not be successfully solved. It is to the development of this variety of the speaking telephone rather than to that of the magneto instrument that inventors will find it most advantageous to turn their attention, for I hazard little in saying that the latter has already reached such a surprising degree of efficiency, as to leave comparatively little more to be done within the necessary limitations which have been pointed out."

Mr. Pope throws out what has been done with the exception of the supposed mechanical difficulty, and that has been got over by a halfpenny money-box.

Now one very pleasing and gratifying circumstance attaches to this discovery of Prof. Hughes: he has thrown it open to the world, and by that means he has no doubt checked that species of immorality—I don't know what else to call it—connected with the infringement of the patent law, as regards the telephone. He allows us all to manufacture microphones for ourselves, but even he has been subjected to rather a peculiar incident. One impulsive and active gentleman who was present at the Royal Society the other night when Prof. Hughes first described his invention, went home and made himself a microphone, wrote a description of it and sent it off post haste to Paris. A short time afterwards Prof. Hughes himself with great care prepared a paper to be read before the French Academy, but to his great surprise he found that he had been forestalled, a description of his instrument had already appeared in the Paris prints from the gentleman in question.

There are lessons to be learnt from this discovery, and the principal lesson is—we can all of us with the means at our disposal cross-question nature and find out her secrets, and there are many secrets which yet remain to be divulged. We learn the wonderful connection which exists between all the physical forces: heat, and light, and electricity, and magnetism, are all co-related, and it has come to this, that what boys have said in joke has come to pass in earnest. We have been able to convert electricity into light, and light into electricity. We are now able to convert electricity into sound, and sound into electricity, and thus we are enabled to see the thunder and to hear the lightning.

THE SCIENTIFIC AIMS AND ACHIEVEMENTS OF CHEMISTRY[1]

MORE than a generation has passed away since my predecessor in the chair of Chemistry, Prof. Bischof, who was so full of merit in the domain of chemical geology, held the high office which the friendly confidence of my colleagues has entrusted to me for the ensuing university year. Since that time chemistry has undergone important changes, and its position upon the German high schools has also become an essentially different one.

At that time a general discouragement had taken root amongst the most eminent chemists. It was believed that all speculation had to be dismissed from the field of chemistry, and particularly that all atomistic considerations had to be discontinued, because whole categories of facts could not be made to agree either amongst each other, nor with the general theoretical views of that time.

At our high schools at that time chemistry was only taught from the chair; very often by teachers who were essentially appointed for other subjects. At most of the universities the students could be admitted to practical work only by favour of the teachers, and even Liebig's laboratory at Giessen, the first of all educational laboratories, only just then received its interior arrangement.

How different now! Well aware of its task and its aims, scientific chemistry, in close connection with physics, advances slowly, it is true, but with self-reliance and a certain confidence.

Each university has its special chair of chemistry, many indeed have several. Richly furnished laboratories, and very often luxurious edifices, are at the disposal of chemical students in nearly all German universities, and the chemical lectures are the best frequented ones almost everywhere.

All this and also the circumstance that it is just a chemist who is able to day, as a representative of the entire university, to speak to the entire university from this place, proves, doubtless, that our science is now generally recognised to the extent it merits. But as on many sides it is over-estimated, it is yet more frequent, on the other hand, that its scientific right of existence is doubted. While outsiders who may occasionally have seen a chemical experiment, or may have heard of the grand applications of chemistry to practice, declare chemistry to be the finest science of all, although they may not be able to form an idea concerning its scientific aims, other one-sided representatives of so-called humanistic studies, who also mix up the applications of chemistry with its scientific task, tend towards the unjustifiable view that chemistry ought really to be taught only at polytechnic schools, but not at the "universitas litterarum."

The propagation of such erroneous conceptions renders it the duty of the chemist to appear as the defender of the science he represents, and it will doubtless be considered fully justified if to-day I try to explain to you the scientific position of chemistry and its participation in the great progress of universal science.

Chemistry has often been designated as the sister of physics, and both subjects are in reality so nearly related, their domains are so contiguous, that the layman cannot understand the difference, and that even the scientific man can hardly fix the limits.

Chemistry and physics together form that group which may be designated as *general natural science*, inasmuch as the occurrence of their materials of study is unessential, and the laws recognised by them are valid everywhere. Astronomy, geography, geology, botany, and zoology (the latter including those more special subjects treating of man, and which form the scientific part of medicine), all these, which ought to be comprised under the

[1] Address delivered on assuming the Rectorate of the Rhenish Friedrich-Wilhelms University of Bonn, October 18, 1877, by Prof. Aug. Kekulé.

name of *special natural science*, are tied to certain circles of objects of study, and the truths they have established are valid only for these circles. Even that generalisation of the so-called organic natural sciences, which is designated as biology, cannot very well be called a general natural science, because if indeed anywhere else than upon the earth there exists something similar to what we call life here, there is yet but little probability that the laws of terrestrial life may be applied to that life of other worlds.

The common task of *general natural science*—of physics and chemistry, therefore—is the investigation of matter, of its properties, its changes, and of the laws of these changes; and the laws recognised by them must be applicable wherever there is matter at all.

Now with regard to the *difference* between *physics* and *chemistry*, it strikes the superficial observer that modern physics treats, in a more general manner, of the properties and changes of properties of bodies, and in doing so that it contemplates the separate bodies only as the bearers of the properties; while chemistry studies the separate bodies differing with regard to their material, and that it touches their properties only inasmuch as they appear necessary for distinguishing the bodies. One might be inclined to found a definition of the two disciplines upon these differences. But when we enter more deeply into the subject we shall see that the essential differences must be looked for elsewhere.

Of all conceptions which the human mind could form regarding the essence of matter, only the hypothesis of *discrete* mass-particles, the atomistic hypothesis therefore, has led to an intelligible explanation of facts. Even if nobody who has followed the scientific discussions of the latest time, can deny that the tendency of natural scientific reflection just now again lies in the direction of reducing the differences of materials to dynamic causes, yet we must certainly own that *at present* the observed facts can be deduced as necessary consequences from the atomistic theory only. On this point physicists and chemists are no doubt agreed. And if even modern representatives of speculative philosophy concur in the view that all natural knowledge leads to the mechanics of atoms in the last instance, then we may doubtless use the atomistic theory *preliminarily* as the basis of further reflections in the domain of natural science and, *for the present at least*, found upon it the definition of the separate branches of natural science, be it only in order to render a clearer account to ourselves of their tenour and of the limits of their domains. Now the sum total of all knowledge obtained with regard to matter has led to the following *maxims of the atomistic theory*.

We must imagine that matter consists of small particles, uniform in their material and not further divisible, not even by chemical processes,—of *atoms*. These atoms accumulate in consequence of forces inherent in them or acting upon them, and thus produce systems of atoms, or *molecules*. In the gaseous state these molecules move about in space as isolated beings, in the other aggregate states an attraction of molecules also becomes apparent, and thus the *masses* originate which are able to act upon our senses directly.

If this conception of the essence of matter is taken as a basis then we may define chemistry as the *science of atoms*, and physics as the *science of molecules*, and it lies near then to look upon that part of modern physics which treats of *masses* as a separate discipline, and to reserve for it the name of *mechanics*. Thus mechanics appears as the fundamental science of physics and chemistry, inasmuch as both are obliged to treat their molecules or atoms respectively as masses in certain considerations, and particularly in calculations. Mechanics, physics, and chemistry, however, are the bases of all special natural sciences, because it is evident that all changes, no matter whether they occur, in the great cosmos, or in the microcosmos of the vegetable or animal body, can but be of a mechanical, physical, or chemical nature.

Now from the fact that chemistry has to do with the study of atoms, of the building stones, therefore, of which the molecules are constructed which physics treats as a whole, it results directly that the theoretical investigations of chemistry offer more difficulties than those of physics, and that theoretical chemistry can progress in certain directions only after theoretical-physical knowledge has sufficiently advanced. The comparatively low state of theoretical chemistry thus seems not only pardonable but natural, and it becomes clear why for the present theoretical-chemical investigation has principally turned its attention to those questions which are more or less independent of physics. Thus we understand why *chemical dynamics* is as yet an almost uncultivated field upon which the materials, which are heaped up in immense profusion could, up to the present, not find a theoretical treatment, while on the domain of *chemical statics* ripe, or at least partly-developed fruits, were reaped in plentiful quantity.

It will not be difficult to show that chemistry and chemists have, in this direction, materially contributed towards the progress of the general doctrine of atoms, therefore towards the progress of our knowledge of the nature of matter.

Since the (as far as we know) first foundations of the scientific observation of nature were laid by Democritus, the most elementary maxims of the theory of matter have remained the same. "From nothing nothing can come; nothing that is can be annihilated; all change is only combination or separation of particles." But the atomistic theory of antiquity was more a precursor of the views which we now designate in physics as the molecular theory; it contained, even in its further development, no fundamental thought of a specially chemical theory.

The first fundamental maxim of scientific chemistry was pronounced towards the end of the seventeenth century by the chemist Boyle, who was first to define the conception of the *chemical element as that which is not further divisible into materially different parts*. It will not matter whether many or perhaps all the bodies which we now consider to be chemical elments may be found to be further divisible in the progress of knowledge—although there is at present no real indication for this—the idea of the chemical element will always remain unaltered.

With this idea of the chemical element that old conception of the indestructibility of matter was then connected, and thus the further fundamental maxim of chemistry originated, of the invariability of elements, which has not further been questioned since Lavoisier's celebrated experiments on the often-pretended change of water into earth, and which finds its confirmation in all chemical facts.

From these views the *chemical atomic theory* arose at the beginning of the nineteenth century, and the English chemist Dalton is with right regarded as its founder. While, after Democritus, the difference of all things is caused by the difference of their atoms in number, size, shape, and order, a *qualitative* difference of atoms, however, does not exist, Dalton first in a definite manner supposed the existence of *qualitatively different* elementary atoms. He was first to ascribe to these qualitatively different atoms *certain weights* which are *characteristic* for the various elements; he first showed that these relative *atomic weights* may be determined by chemical study.

As the conception of the chemical element so will also the conception of the chemical atom, as *that quantity of elementary matter which is not further divisible by chemical processes*, remain for ever. For chemistry, the question whether the chemical atoms are originally units (*einheitliche*) and absolutely indivisible beings, is of no importance. Let the proof be given that the chemical atoms are formed of particles of a finer order, or let the theory of revolving rings founded by Thomson, or some other similar conception which understands atoms to result from continuous matter, be proved in the progress of knowledge, the conception of chemical atoms will not be altered or annihilated. The chemist will always welcome an explanation of his units, because chemistry requires atoms only as a starting point, not as an end.

Dalton's atomic theory from the very first suffered from a certain imperfection which consisted in its speaking of the atoms of compound bodies as well as of those of elementary ones and thus did not distinguish the ideas of atom and molecule. For the first period, during which the foundations of chemical science had to be completed, no essential harm arose from this want of clearness, but later on, when the structure was to be developed further, it caused considerable confusion.

It is true that already in 1811 Amadeo Avogadro pronounced the maxim that gaseous bodies contain an equal number of molecules in equal spaces, and that even the molecules of elementary substances consist of several atoms, and that in 1814 the French physicist Ampère arrived at the same conclusions; but this idea, which was so fertile in the future, hardly attracted any notice at first. In its application it led to contradictions which seemed insolvable at that time, and it was therefore abandoned, although the great chemist Dumas had taken it for some time as the base of his considerations. More than that, it was forgotten until forty years later the Italian chemist Cannizzaro recalled to the memory of his colleagues the merits of his countryman.

In the meantime chemists first, and later on physicists as well,

had arrived at precisely the same conceptions, starting from new and perfectly independent points of view.

The chemists, with Laurent and Gerhardt as leaders, were led by *purely chemical* considerations and essentially by reasons connected with systematics, to distinguish clearly between the ideas of atom and molecule, and to find methods which, *in the perfection at which they have now arrived*, render possible the determination of the relative weights of the atoms and molecules, and even of the *absolute* number of atoms in the molecules, for all more perfectly examined substances, by the discussion of purely chemical facts. Amongst others they arrived at the result that the molecule even of elements consists of two atoms as a rule.

In physics, however, the *mechanical heat theory* caused a probability bordering upon certainty to be ascribed to the fundamental thought of Avogadro's hypothesis; and when our celebrated colleague, Clausius, in the course of his classical investigations, had arrived at the conception that even in elements the molecules consisted of several atoms, then he could express his satisfaction regarding the fact that chemists *before him*, on totally different ways, had already arrived at the same results.

After, in this manner, Avogadro's hypothesis on the nature of gases had obtained general recognition, and the relative weights of the gas particles could thus be deduced from the specific gravities of gases; after, on the other hand, we had learned to determine the relative weights of the chemical molecules by chemical considerations, then it appeared that both values were identical, and thus we arrived at the conception, which anyhow was probable on account of its simplicity, but which was not a necessary one previously, that the *gas particles* and the *chemical molecules* are identical, that heat therefore, is able to subdivide matter down to the chemical molecules.

The *chemical* part of the atomic theory was essentially extended some twenty years ago by that hypothesis, made by chemists, which has been designated as the theory of the *chemical quantivalence* of atoms. In its fundamental thought this hypothesis only says that *besides* the characteristic atomic weight which is the cause that the elements combine in certain *proportions of weight*, the *atoms* still possess a further fundamental property, which causes them to combine exactly *in those numbers* in which they do. As we could not, at first, arrive at a clear conception of this fundamental property, we simply ascribed a certain number of chemical attraction units to the materially different atoms, and accordingly called them uni-, bi-, tri-, or quadrivalent.

Now this hypothesis of the chemical quantivalence of elementary atoms of course still offers many dark points, but yet it has led to the recognition of a law which, not only for chemistry, but for the entire atomic theory is of fundamental importance, and which chemists call the law of the *connection of atoms* (*Verkettung der Atome*). *The separate atoms of a molecule are not connected all with all, or all with one, but, on the contrary, each one is connected only with one or with a few neighbouring atoms, just as in a chain link is connected with link.*

At the same time it is evident that the atoms within the molecules must be in constant *motion*, and if, indeed, nothing certain is known respecting the nature of this motion, yet it results from this very law of the connection that the intramolecular atomic motion must be of such a nature that the separate atoms move about certain positions of equilibrium without ever leaving them, as long as chemically the molecules continue to exist. The motion of atoms, therefore, is certainly similar to that of the molecules in the solid state, and thus it may be said that the *molecules of existing substances* are *solid aggregations of atoms*. A state of motion similar to that which the molecules of liquid bodies possess, occurs only with chemical changes, by which molecules of different atomic structure are formed, and then evidently only in a transitory manner and only for single atoms. A state of this kind doubtless plays an important part not only in fermentation phenomena, but also in the chemical processes occurring in living organisms. The nature of the motion of atoms is, as I said before, unknown at present. Perhaps it may be imagined as an oscillatory one in such a way *that the number of oscillations executed in the unit of time* exactly represents the chemical value, and that atoms engaged in functional oscillation, and perhaps striking against each other, appear in chemical combination. Then the chemical quantivalence of atoms would have to be considered as a *constant* one with even greater probability than hitherto. Anyhow one might imagine that polyvalent atoms, at temperatures which, for the substances in question, might be called *ultra-hot*, do not meet with another atom during one or even more oscillation phases, while adding a part of their motion-energy to the molecular motion; a conception which would correspond to the present conception of unsaturated affinities. We would have to think it probable, further, that upon raising the temperature still more, this intermediary state of partial dissociation would be followed by one of total dissociation, during which isolated atoms move in space, just as has already been proved for mercury vapour at temperatures of easy access.

The law of the connection of atoms based upon the hypothesis of chemical quantivalence, at present accounts only for the *chemical serial connection* (*Aneinanderreihung*) of atoms, without explaining their *position in space* and the *form of molecules* caused by it. But even now, from investigations on molecular volumes it results that the nature of the connection of atoms influences the *mean distances of atoms*.

The circumstance that with isomeric substances the boiling-point of that modification is the highest for which the law of the connection of atoms supposes a chain running in a straight line, while volatility increases the more ramifications the chain shows, the more compressed, therefore, the molecule appears from a chemical point of view; together with the maxim, probable in itself, that the position of the point of gravity and the moment of inertia of the rotating molecule must be of influence upon volatility, seem to indicate that the views on the chemical connection of atoms at the same time give us some notion on *their mean position in space*. The calculations made by Emil Meyer, of the molecular diameters, molecular transverse sections, and molecular volumes, also seem to support this view. Thus the probability of the hypothesis pronounced by Le Bel, and worked out further by Van't Hoff, of the *unsymmetrical carbon*, according to which the four affinity bonds of the carbon atoms, which had already been represented tetrahedrically, are imagined to exist *in space* in a tetrahedrical position, is increased. An hypothesis which may perhaps not merit the unconditional praise which Wislicenus has bestowed upon it, but which certainly much less deserves the bitter derision which Kolbe was inclined to throw upon it.

The hypothesis of chemical quantivalence further leads to the supposition that also a considerably large number of single molecules may, through polyvalent atoms, combine to *net-like*, and if we like to say so, *sponge-like masses*, in order thus to produce those *molecular masses* which resist diffusion, and which, according to Graham's proposition, are called *colloidal* ones. The same hypothesis in the most natural manner leads to the view, already pronounced by our genial colleague, Pflüger, that such an accumulation of molecules may go further yet, and may thus form the *elements of the form* of living organisms. Of these *mass-molecules* we may perhaps suppose further that they, through the constant change of position of polyvalent atoms, show a constant change in the connected single molecules, so that the whole—and of course under generation of electricity—is in a sort of living state, particularly since, through the same change of position, adjacent molecules are drawn into the circle of combination and newly-formed ones are expelled. To follow such speculations any further at present would, however, be equivalent to leaving the basis of facts rather too far behind us.

Really fertile hypotheses on the *nature of that force*, which brings about the combination of atoms, have not been made up to the present. The *electro chemical theory*, so ingeniously worked out by the great Berzelius, of which, during whole decades, it was believed that it would lead to a satisfactory explanation of chemical facts and to their combination with physical phenomena has proved completely insufficient. In all probability in a future period of the development of science it will again be taken up, and will then, in a modernised form, bear its fruits.

In any case besides the chemical quantivalence, which decides the *number* of combining atoms, the *specific intensity* with which this combination takes place, must also be considered. Here we must suppose that the atoms combined in a molecule, and therefore saturated with regard to their quantivalence, do not only exercise an attraction upon each other but also upon the atoms of neighbouring molecules, and that thus a *molecular attraction* results, which is caused by the attraction of the separate atoms and therefore depends on their quality. Only in this way we can explain the process of chemical decomposition and the existence of that infinite number of more complicated things which are supposed to be *molecular additions* or molecules of a *higher order*. Unquestionably the same cause plays a part in so-called *mass-effects* and in *catalytic* decompositions. The formation of solutions must also be ascribed to it, which hitherto were considered as chemical combinations in varying proportions, but

which are now more appropriately called *molecular mixtures.* The same fundamental cause further gives rise to the phenomena of cohesion, adhesion, and capillary attraction, and it seems therefore as if the supposition of special molecular forces is in no way necessary any longer.

Now as the attraction of atoms depends on their *quality*, it is also clear that the molecular attraction caused by such atomic attraction must, under favourable conditions, produce an *orientation* of all molecules combining with one another, and must thus lead to bodies of a regular molecular structure, therefore to *crystals*.

Lastly, the question whether the properties of atoms are dependent on their *weight* has much occupied the chemists of modern times. Positive results which could be rendered clear in a few words have not yet been obtained, but it seems, according to the observations made by Lothar Meyer and Mendelejeff, as if not only the *chemical* properties and specially the chemical quantivalence of atoms and the intensity of their mutual combination, but also the *physical* properties, which at present are still treated as *constants* for materially different objects, were a function and indeed a *periodic function* of the atomic weight. The mathematical form of this function is no doubt of a peculiar nature, but one thing seems certain, viz., *that the numerical value of the atomic weight is the variable by which the substantial nature and all properties dependent on this are determined.*

Thus there again seems hope that it will be possible to reduce *all* properties of matter, including gravity, to *one* and the same force.

The right of introducing all such speculations into the domain of exact science, has been questioned very much. It is generally conceded, indeed, that the setting-up of hypotheses on the domain accessible to exact investigation, as a method of investigation, is useful, inasmuch as it often may accelerate the progress of exact knowledge. But it is at the same time often believed that speculations beyond a certain limit are not admissible. The scientific value of all atomistic considerations particularly has ever, and also in the most recent time, been very much doubted. It has been pretended specially that the supposition of atoms did not explain any properties of bodies which had not first been ascribed to the atoms themselves.

We must own that such remarks contain many truths, but just for that reason it seems necessary that we examine the limit of their correctness.

It is generally acknowledged that the results of exact observation have the value of facts, therefore possess that degree of certainty which human knowledge can attain at all. It is further not contested that to all those laws which, independent of hypotheses on the nature of matter, are deduced from facts, nearly the same certainty must be ascribed as to facts themselves. It is just as incontestable, however, that the human mind in the positive understanding of facts does not find complete satisfaction, and that therefore natural sciences have to follow a yet further and higher aim, *that of the knowledge of the essence of matter and of the original connection of all phenomena.*

But the essence of matter is not accessible to any direct investigation. We can only draw conclusions regarding it from the phenomena which are accessible to our observation. And thus it is evident that there is a certain limit which, moreover, is influenced by the state of our knowledge at any given time, beyond which positive investigation loses ground and where the path is only open for speculation.

If, therefore, the single investigator, following the inclinations of his nature, rests satisfied with positive investigations and renounces all speculations, it is yet clear that to science as such this is not permitted.

By way of hypothesis, based upon what is known as facts, ideas must be formed on the nature of matter; the consequences of these ideas must be developed logically, and, if necessary, by the aid of calculation, and the results of these theories must be compared with the phenomena accessible to observation.

Of course, the *complete* truth will never be reached in this way, or there will, at least, never exist complete certainty that our conceptions are really identical with truth. But that conception which is simplest in itself, and which in the simplest manner accounts for the greatest number of phenomena, and finally for all, will have to be considered not only as the best and most probable one, but we shall have to designate it as relatively, and we may say, humanly, true.

By this the scientific right of existence of speculative investigation is no doubt proved, also for the so-called exact sciences, because beyond a certain limit these indeed cease to be exact.

Simultaneously, however, the scientific value of the present atomic theory is also proved, because it has not been contested that, even in its present and still extremely incomplete form, it accounts satisfactorily for an uncommonly large number of facts, better than any other conception.

It will certainly require further extension, and also a deeper fundamental structure; but at present there is very little probability that it will be completely superseded by essentially different conceptions.

There are other reproaches which have been made to chemistry specially, and still more to chemists, now and since the time of Lord Bacon; and even chemists cannot deny that they were not altogether undeserved.

It has been said that chemistry wilfully makes innumerable single hypotheses which are neither in connection with one another nor with the whole; that the value of hypotheses is over-rated by her disciples, far too great certainty being ascribed even to such as are only little justifiable, and that they are treated as if they had been actually proved; and finally, that her hypotheses are always gradually raised to articles of faith, and that everybody who sins against such dogmas is prosecuted as a heretic.

Recent times have also, in this direction, brought about a considerable improvement. The justification and the value of hypotheses are now recognised in chemistry, but at the same time the true value of hypotheses is also understood by chemists.

In chemistry also, as in all domains of science, blind faith in authorities has been crushed, and by this alone the danger of dogmatising is lessened. And should perhaps any one, who holds antiquated views, try to attach his dogma upon progressing science as a restraint, he will always find the striving young generation, the representatives of the future, ready to remove unjustified impediments. If others, in the fiery zeal of youth, should be inclined to look upon daring flights of fancy as scientific hypotheses and to give them out as such, then those who are more moderate by themselves or by the riper experience of age, will always feel it a duty to step in as regulators.

The school of independent, and at the same time quiet thinkers, is now so numerously represented also among chemists, that a constant development of the science may be confidently expected, and an overgrowth of weeds need no longer be feared. Also in chemistry we are now well aware of the continuity of human mental work; the present generation no longer looks with despising contempt upon the work of their predecessors; far from thinking themselves infallible, they know that at any time it remains to the future to continue the work of preceding generations.

ON THE CAUSES OF THE ASCENT OF SAP IN TREES[1]

THE question as to what forces cause water to rise to such a remarkable height (frequently) in trees has had very various answers given to it. But these have mostly failed to account adequately for the phenomenon.

Capillary action is perhaps the oldest cause adduced. The view was long popular that water rose in trees like oil in a wick, the connected vessels of the wood forming capillary tubes. This view lost force when it was known that the wood of coniferæ was without vessels; and it did not explain the weakening or stoppage of the rise of sap produced by amputation of the roots, nor the presence of air in the columns of sap.

Shortly after Dutrochet's thorough study of diffusion, this phenomenon was called in to account for the rise of sap. One grave objection to such a theory is the rapidity of the ascent of sap (it has been carefully measured) as compared with the slowness of diffusion, which depends simply on molecular motion; another is the inevitable consumption of the osmotic force of tension. So that other problematical forces had to be called in.

When Jamin found that the imbibition of water through fine porous substances (*e.g.* blocks of gypsum) took place with great force, and that the air could thus be compressed to several atmospheres, an effect of this nature was affirmed to occur in living plants, the cell membrane being considered a porous substance. But in fact the natural saturated cell membrane has no air-filled pores, but only pores already filled with water, and even the hollow spaces, bounded by the cell membrane, are partly filled with water; besides, the fact that a branch, immediately after being cut off, loses in great measure the power of raising water, is against this theory.

[1] Abstract of a recent paper in *Der Naturforscher.*

A few years ago yet another theory was started, based on M. Quincke's discovery of the tendency of liquid films to expand rapidly upon wetable surfaces. The only advantage of this lay in accounting for the rapidity of the rise of sap; otherwise it was open to all the objections of the Jamin theory.

A theory has lately been propounded, and thoroughly worked out, by M. Joseph Böhm, which is characterised by good consistency, and offers perhaps a more satisfactory explanation of the phenomenon than any that have been referred to. It is based, like the osmotic theory, on the cellular structure of all sap-conducting plants, and it attributes an important *rôle* to the *elasticity of the cells*. "When the surface-cells of a plant," says M. Böhm, "have lost a portion of their water through evaporation, they are somewhat compressed by the air-pressure. Like elastic bladders, however, they tend to take their original form. This of course is only possible by their drawing in either air or water from without. Since, however, moist membranes are little penetrable by air, the cells draw from cells further in a portion of their liquid contents. These again borrow from their neighbours further down, which contain more water, and so on, either to the extreme root-cells or to those parts of the stem which are supplied with water from below through root-pressure."

To illustrate the action M. Böhm constructed an artificial cell-chain. A funnel closed by a bladder represented the evaporating leaf; to it were connected below several glass tubes about two ctm. wide, closed at one end with a bladder, and joined together in series by means of thick-walled caoutchouc-tubing. In consequence of the evaporation, the membrane which closes the funnel-mouth is bent inwards, and when it has reached a certain tension water is sucked into the funnel out of the next lower cell, which covers its loss in like manner. Manometers, connected with certain cells of the apparatus, indicate the amount of suction at different heights. To avoid fouling of the membranes carbolic acid was mixed with the distilled water in the cells. Since bladder membranes, with a not very great height of liquid column over them, admit passage of water by filtration, these artificial cell-chains (it is pointed out) must act much more imperfectly than the sap-conducting cells placed over one another in living plants, which cells, by reason of their narrow aperture, retain their liquid column by capillary attraction.

It is shown that this theory is in harmony with sundry phenomena which are contradictory of the imbibition theory.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

It will be proposed to confer the degree of D.C.L. *honoris causa* at the ensuing Oxford commemoration, upon Dr. William Spottiswoode, M.A., of Balliol College, F.R.S.

The following awards for proficiency in Natural Science have been made at St. John's College, Cambridge:—Foundation Scholarships to F. J. Allen, Marr, Slater, C. M. Stuart; Exhibitions to Fleming, Hart; the Open Exhibition to C. H. O. Curtis, from the Royal School of Mines.

The plans for the new University edifices at Strasburg have just been completed. They provide for over 100 rooms to serve as auditoriums, museums, the inevitable German singing hall and fencing hall, &c., and will meet the needs of all sections of the university, with the exception of the medical faculty, which retains its old quarters, on account of the propinquity to the hospital. The attendance, which has fallen off during the past year, is now greater than ever before, the number of students for the present semester being 710.

SCIENTIFIC SERIALS

Journal de Physique, April.—In this number M. Vincent recommends chloride of methyl as a frigorific agent, and indicates an abundant source of it. He employs a cylindrical copper vessel having double walls, between which the liquid is admitted through a peculiar cock from an adjoining vessel. In the central part is put an uncongealable liquid such as alcohol. The outer wall is enveloped in cork. On opening the cock the chloride of methyl enters into ebullition, and the temperature of the alcohol bath sinks to $-23°$. By connecting with an air pump and making vacuum, a much lower temperature may be obtained. One pretty experiment with this apparatus is the crystallisation of mercury.—M. Gariel explains the new system of numbering glasses of spectacles, in which a unit called the *dioptrie* is used, this being the power of a convergent lens of 1m. focal distance. The number of dioptries for a particular lens is got by dividing 1m. by the focal distance reckoned in metres and decimal fractions of a metre, since the power varies in inverse ratio of the focal distance. Let N_D be the number of a lens reckoned in dioptries and f_m the focal distance in metres, then $N_D f_m = 1$m., which gives one of the quantities when the other is known.—M. Pellat contributes a mathematical paper on the specific heats of vapours, and the phonograph occupies some attention.

Memorie della Società degli Spettroscopisti Italiani, January, 1878.—Prof. Tacchini contributes a long paper on the appearance and constitution of the sun, based on the photographs of M. Jansen taken at Meudon; there is also another by the same author, giving the observations of the positions in which the magnesium and 1474 lines appeared on the limb of the sun in June, 1877. The appendix contains a paper by L. Gruber on the falling stars of the first part of last November.

February.—Notice of the death of Father Secchi, by the editor.—A paper by Prof. Rosetti on the temperature of the sun; a description of the thermopile and the necessary accessories, together with the results, is given at length.—A table showing the number of spots and protuberances, and the heights of the latter during the first half of the year 1877, and drawings of the chromosphere for the months of November and December made at Rome, by Prof. Tacchini.

March.—A note and table by Prof. Tacchini showing the position on the sun's limb when the magnesium and 1474 lines were seen during June, 1877. Also a summary of the positions of the same during the first half of the year 1877.

SOCIETIES AND ACADEMIES

London

Royal Society.—"Note on the Specific Gravity of the Vapours of the Chlorides of Thallium and Lead," by Henry E. Roscoe, F.R.S., Professor of Chemistry in Owens College, Manchester.

Experimental difficulties of so serious a nature surround the attempt to ascertain the specific gravity of vapours at a high temperature that, in spite of the interest which attaches to this subject, but few additions have been made in our knowledge in this direction since the researches of Deville and Troost.

The present experiments, of which this notice contains the first results, have been made with the object of so simplifying the process as to render it easy to determine the specific gravity of the vapours of bodies possessing high boiling points with a degree of accuracy sufficient for the purpose of controlling their molecular weights.

The method consists in vaporising the substance under examination in long-necked glazed porcelain globes of known capacity placed in a muffle raised to bright redness. The temperature of the globe is ascertained by a calorimetric determination made with heavy platinum weights placed in the muffle, this determination being checked by the simultaneous insertion in the muffle of a second globe containing mercury.

The porcelain globes having a capacity of about 300 cub. centims., and containing from three to nine grams of substance, are closed by loosely-fitting stoppers of baked clay, and then gradually introduced in the muffle. After remaining there until no further escape of vapour is observed, and until the temperature has become constant, the globes are quickly withdrawn from the muffle and their contents removed and analysed, the temperature being in each case ascertained by the calorimetric method at the time of withdrawal of the globe. The following determinations of the specific gravity of mercury vapour serve to show the reliability of the method:—

	Temperature determined calorimetrically.	Specific gravity of mercury vapour.
	°	
Experiment I.	1019	6·92
,, II.	894	6·75
,, III.	815	6·91
,, IV.	972	5·77
,, V.	1047	7·05

the calculated specific gravity (Hg = 198·8) being 6·728.

Before determining the specific gravity of the vapour of thallium chloride it was ascertained that this compound does not

give off free chlorine when volatilised at a red-heat, and that the sublimate contains thallium and chlorine in the atomic ratio of equality.

In each experiment the total amount of thallium and of chlorine remaining in the globe was determined by analysis, and the specific gravity calculated from their sum.

	Temperature determined calorimetrically.	Specific gravity of the vapour of thallium chloride.
Experiment I.	859	8·15
,, II.	828	8·28
,, III.	1015	8·06
,, IV.	859	7·43
,, V.	1026	8·75
,, VI.	852	8·60
,, VII.	837	7·84

The specific gravity of thallium chloride vapour calculated upon the supposition that the molecular weight of the compound is 238·07, and its formula TlCl, is 8·49.

Four determinations of the specific gravity of mercury vapour made simultaneously with four of the above experiments gave as a mean the number 6·0 instead of 6·728.

The specific gravity of the vapour of lead chloride was made in a similar way, but the temperature required for complete volatilisation is much higher than that needed in the case of the last compound. The residue left in the globes was completely soluble in hot water, and contained lead and chlorine in the proportion of one atom of the former to 2·08 of the latter.

	Temperature determined calorimetrically.	Specific gravity of the vapour of lead chloride.
Experiment I.	1046°	9·12
,, II.	1089	9·72
,, III.	1077	9·51
,, IV.	1070	9·64

The specific gravity calculated from the formula $PbCl_2 = 277·14$ is 9·62.

I hope before long to be able to lay before the Society the results of specific gravity determinations of the vapours of other compound and elementary bodies, together with the whole of the experimental details.

Anthropological Institute, May 14.—Mr. John Evans, D.C.L., F.R.S., president, in the chair.—Capt. Dillon exhibited a series of flint implements, collected in the neighbourhood of Ditchley, Oxfordshire, and a number of others, from the drift gravel of the sea valley near Clapton, were exhibited by Mr. Worthington G. Smith. The following papers were read by the author, Prof. Rolleston, M.P., F.R.S.—Description of a male skeleton found at Cissbury by Mr. J. Park Harrison. The paper was illustrated by a semidiagrammatic of the pit whence the skeleton had come; the principal parts of the skeleton itself, some bones of ox, goat, pig, and red deer, and finally, a large quantity of worked flints and some lumps of iron pyrites were upon the table. Much help had been received as to the preservation of the skeleton from Dr. Kelly, the Medical Officer of Health for the district. There was no doubt the skeleton had belonged to a man with a markedly dolichocephalic skull, the length-breadth index being 71, but not tapeinocephalic, the length-height index being 76; his stature had been something under 5 feet, either as calculated from the long bones or by simple measurement of the skeleton as laid out and increased by the addition of one inch for calcaneal and cranial integuments. The age had been something between 25 and 30, the absence of wear on the wisdom teeth being deceptive owing to the non-development of one of these teeth and the small size of another. The owner of the skeleton had suffered from infantile cerebral hemiplegia, the right humeous being half an inch longer, and the right radius $\frac{8}{10}''$ longer than the corresponding bone on the left side, whilst the femur were equal in length, and the right tibia only $\frac{2}{10}''$ longer than the left. This pathological condition, however, did not account for some very striking characters of the limb-bones, which were equally prominent on both sides of the body: these being the platyenemy of the tibiæ, the anterior convexity, and from side to side flattening of the humeri, and the curved upward end of the illux. Altogether the osteologial peculiarities of the skeleton were as distinct evidences for its antiquity as its mode of burial.—On the excavation of three round barrows at Sigwell, near South Cadbury, in the parish of Compton, Somerset. These three round barrows belonged to the bronze age, no trace of iron, except such as had been accidently, and demonstrably so, introduced, being found in any of them. The interments in them had been in the way of cremation, and in one case the ashes had been gathered into a bark coffin and a bronze dagger placed with them. In one barrow no interment was found; in another the ashes occupied an area of only an inch in diameter; and in both cases the bones had been carefully picked out of the embers of the funeral pile and interred apart, though, in neither case, in an urn. Fragmentary pieces of coarse pottery, however, were found here and there throughout the mass of the barrows, and, though there were no flints to be found in the immediate neighbourhood, great abundance of chipped flints and some scrapers were found, and notably one very beautiful one by the Rev. J. A. Bennett, to whose association very much of the success of the exploration was due.

Physical Society, May 11.—Prof. W. G. Adams, president, in the chair.—The following candidate was elected a Member of the Society: Rev. P. Magnus, B.A., B.Sc.—Mr. J. Norman Lockyer, F.R.S., read a paper on some recent researches in solar chemistry, a report of which is deferred for the present.—Sir William Thomson, LL.D., F.R.S., described and exhibited the apparatus he has employed in recent researches on the influence of stress on magnetisation, a detailed account of which he has submitted to the Royal Society; he also, in part, described them at the Royal Institution on May 10, but attention was not then directed to the experimental details now brought before the Society. The rod or wire under examination was surrounded by two co-axial wire helices, the outer of which was connected with the battery, and the inner with a ballistic galvanometer, that is, one that acts with regard to electric impulses just as Robins' ballistic pendulum. It was some years ago discovered by Villari that a longitudinal pull augments the temporary induced magnetism of soft iron bars or wires when the magnetising force is less than a certain critical value, and diminishes it when the magnetising force exceeds that value; in either case the residual magnetism is augmented when the force is applied and diminished when it is removed. Sir W. Thomson has found the critical value for soft iron to be about twenty-four times the vertical component of the earth's magnetic force. It is therefore approximately 10 C.G.S. units. In the case of some bars of nickel and cobalt specially prepared for him by Mr. Wharton, of Philadelphia, he finds opposite effects. With the amounts of magnetising force used the effect of pull was to diminish magnetisation, but the amount of this effect was less with the highest magnetising forces than with a certain degree of magnetising force which was found to make it a maximum with probably or possibly a critical value. But this value had not been reached by the magnetising force hitherto applied. The next branch of the inquiry had reference to the transverse stress obtained by water pressure within a gun-barrel, and it was ascertained to have opposite effects to those found by Villari in the case of longitudinal pull. The critical point in soft iron for transverse pull is at about 25 C.G.S. units. Sir W. Thomson has been examining the effect of torsion on a wire that is at the same time exposed to longitudinal pull, confining himself in his first set of experiments to magnetisation under the sole influence of the vertical component of terrestrial magnetism. His results showed, with every amount of longitudinal pull, a diminution of magnetisation produced by torsion in either direction, thus extending a conclusion arrived at by Matteucci, Wertheim, and Wiedemann, regarding the effect of torsion unaccompanied by longitudinal stress. But it now appears that this effect of torsion is very remarkably diminished by a large pulling force nearly reaching the limits of elasticity. In conclusion, Sir W. Thomson called attention to a very different and extremely interesting effect of torsion discovered by Wiedemann—the development of longitudinal magnetisation in an iron wire by twisting it while a current of electricity is flowing along it. This effect, he pointed out, is just what would result from the æolotropic susceptibility for magnetisation due to the æolotropic stress produced in the outer portion of the wire by the torsion, supposing the tangential magnetising force to be less than a certain critical value intermediate between the Villari critical value and the more than twofold greater critical value which Sir W. Thomson has found for transverse magnetising force. But he pointed out that another cause was also positively or negatively efficient in contributing to Wiedemann's result. This cause is the difference of electric conductivity in different directions

which may be inferred from Sir W. Thomson's early experiments and from Mr. Tomlinson's recent confirmations and extensions of the conclusions to which he was led regarding the effect of stress on the electric conductivity of metals. It is much to be desired that Mr. Tomlinson should continue his experiments; but in the meantime it seems probable that the electric conductivity in the outer parts of an iron wire twisted within its limits of torsional elasticity is maximum and minimum in the two spirals at 45° to its length, being minimum in that one of them which is of the same name as the twist, that is, the one in the direction of the maximum extension of the substance; and the conductivity is a maximum in the other 45° spiral which is the direction of maximum contraction of the substance. The effect of this æolotropic conductivity, if it exists, must be to cause the electric currents to flow in spirals of opposite spirality to that of the twist and to produce a corresponding amount of longitudinal magnetisation. The effect of this is to develop, at the end by which the current enters, a true south pole when the twist is right-handed, and a true north pole when left-handed, which is opposite to Wiedemann's result. And if the tangential magnetising force exceeds the critical value, the effect of the æolotropic magnetic susceptibility also is opposite to Wiedemann's result. This is a subject of great interest, and requires further investigation.

Photographic Society, May 14.—J. Glaisher, F.R.S., president, in the chair.—Papers were read by Capt. Abney, R.E., F.R.S., on photography at the least refrangible end of the spectrum, and on some photographic phenomena, by W. England, on dry plate processes, and by T. S. Davis, F.C.S., on a tourist's preservative dry plate process.—Capt. Abney in his paper described the means by which he obtained a photograph of the spectrum beyond the B red line by using one of Rutherford's reflection gratings containing 17,280 lines to the inch, which gives a double spectrum outside a central white light, the resulting negative contained 130 perfectly defined lines, many never yet seen by the human eye, the wave length of the lowest lines being about 10,000 tenth metres.

Rome

R. Accademia dei Lincei, Mar. 3.—The following, among other papers, were read:—Geological and palæontological studies on the middle cretaceous of Southern Italy, by M. Sequenza.—On the Italian expedition to Equatorial Africa, by M. Correnti.—On pensile shoots, with measurements of the vertical and horizontal angles, by M. Robert.—Prehistoric Calabrian objects, by M. Ruggeri.—Graphic determination of the forces in reticular woodwork, by M. Favero.—Statistics of the mortality, diseases, and reforms of the Italian army from 1860 to 1875, compared with those of other European armies, by M. Sormano.—On the nummulitic horizon near Castelnuovo dell'Abate, in the province of Siena.

Paris

Academy of Sciences, June 10.—M. Fizeau in the chair.—The following among other papers were read:—On the cubes or prisms of M. Rohart for the destruction of phylloxera, by M. Chevreul. He finds they contain about thirty per cent. sulphide of carbon. Their efficacy surprises him.—On the large number of joints, mostly perpendicular to each other, which divide the meteoric iron of Santa Catharina (Brazil), by M. Daubrée. In a weight of 23 kilos. were found 1,350 small fragments of iron, each about 17 grammes weight; this would give 25,000 for the 500 kilos. which have come to Europe.—On the source of excito-sudorsal nerve-fibres of the anterior limbs of the cat, by M. Vulpian. They come principally from the spinal nerve, with the spinal roots of the upper thoracic ganglion; but some come directly from the cord by the roots of the nerves forming the brachial plexus.—Experiments proving that the nerve-fibres, whose excitation causes dilatation of the pupil, do not all proceed from the cervical cord of the great sympathetic, by M. Vulpian. Some come directly from the encephalon, mixed probably with fibres of cranial nerves, whose branches enter into connection with the ophthalmic ganglion.—M. Lecoq de Boisbaudran was elected Correspondent for the Section of Chemistry, in room of the late M. Malaguti.—On the geographical distribution of Mexican Graminese, by M. Fournier. He has brought the number up to 638. He divides them into two groups, the one special to Mexico, or partly common to the Andine and northern regions, distinguished by slenderness of leaves and panicles; and the other expanded in the tropical region and noted for larger size. The former inhabit, by preference, mountainous and dry parts; the latter the banks of rivers and moist parts.—On the artificial production of natron or natural carbonate of soda, by the action of carbonate of magnesia on chloride of sodium, by M. Cloez. This is done at ordinary temperature. The author thinks the phenomenon may occur in nature, explaining at once the production of natron and the large quantity of chloride of magnesium found in solution in the water of salt lakes.—On modifications produced in the animal system by various albuminoid substances injected into the vessels, by MM. Bechamp and Baltus. They experimented on dogs both with solutions of natural albumen and with pure albumens of known rotatory power. The latter were not, or were only partly, eliminated.—Influence of the physical state of gallium on its electro-chemical *rôle*, by M. Regnault. He made a small couple (about 489 mm.) of which the two metallic elements were solid and liquid gallium, and were connected by a layer of neutral sulphate of gallium dissolved in water. This caused, in a fine wire galvanometer, constant deflections of more than 40°, in a direction showing that the sheet of liquid had negative tension, while the solid plate had positive. This proves the influence of heat of constitution of a simple metallic body on the energy of its chemical properties.—On starch, by MM. Musculus and Gruber. They give a list of substances produced at expense of starch under the influence of diastase or diluted and boiling sulphuric acid.—Action of fluoride of boron on certain classes of organic compounds, by M. Landolph. Fluoride of boron combines indefinite proportions, equivalent for equivalent, with aldehydes, with acetones, and with carbonyles.—Researches on the peptones, by M. Henninger. These researches seem to indicate that the peptones result from a fixation of water on albuminoid matters, and they thus confirm a hypothesis enunciated by M. Dumas more than thirty years ago that pepsine causes the liquefaction of azotised matters by a phenomenon similar to that of diastase on starch.—Anatomical observations on certain cutaneous excretory glands in the fluviatile tortoises of China, by M. Rathonis. These glands are distinct from those formerly described by Owen and others; their physiological *rôle* is unknown.—Presence and *rôle* of ammoniacal salts in modern seas, and in the saliferous strata of all ages, by M. Dieulafait. All mineral waters, whether sulphurous or not, whether thermal or not, must contain anomalous quantities of ammoniacal salts.—Experimental proof of the incomplete crossing of the nerve-fibres in the chiasma of the optic nerves; longitudinal and median section of the chiasma not followed by blindness, by M. Nicati.

CONTENTS

THURSDAY, JUNE 27, 1878

HENFREY'S BOTANY

An Elementary Course of Botany, Structural, Physiological, and Systematic. By Arthur Henfrey, F.R.S. Third Edition by Maxwell T. Masters, M.D., F.R.S., &c. Illustrated by 600 woodcuts. (London: Van Voorst, 1878.)

IN reviewing a work of this kind, by acknowledged masters of science, the question naturally comes to the front: For what special class of students is it intended? and when this has been settled, a second question: Is the plan which has been adopted the best conceivable for the purpose? The first of these questions appears to be answered in the preface, by the editor's quotation and adoption of Prof. Henfrey's remarks, written in 1857, where he makes special reference to the needs of medical students, who seldom devote to the study of botany more than the summer term of their first session. As to the mode, Dr. Masters also refers with approval to Henfrey's plan of keeping the anatomical and physiological departments of the subject very much in the background, and training the student first of all in morphology and the rudiments of classification; on the ground that by this plan the evil is avoided of "directing the attention of the student to a series of isolated facts and abstract propositions," and of "loading the memory with second-hand information, of no use whatever outside the walls of the examination-room, and indeed of but little service in practical examinations." We may venture to question whether the plan adopted in this work is altogether the best for securing this desirable result, whether, for example, the pages devoted to phyllotaxis[1] do not include a number of "isolated facts and abstract propositions," and whether a longer or shorter description of the characters of considerably over 200 natural orders of flowering plants—when those of twenty-five or thirty are all that would be likely to be of any use to the medical student—may not fairly be open to the charge of "loading the memory with second-hand information;" since it is very few, even of the most experienced botanists, whose personal observation has embraced so wide a range. It is true that this portion may be skipped by the beginner; but then, why include it in a work specially intended for beginners? Fortunately, the day of "Complete Guides to Knowledge" has altogether gone by. The teacher no longer calculates on getting the outlines of every conceivable science within a single pair of boards. This tendency must advance still further, and our text-books must gradually divide themselves into two classes:—one giving primary instruction in the outlines of the entire science; the other, for the more advanced student, entering into the fullest details of special branches. In the science of botany we have numerous admirable text-books and primers which might be included in the first category; in the second, English literature is not yet so rich as French or German. The book before us seems to occupy an intermediate position between the two; it is needlessly bulky and expensive for the medical student who looks to nothing but keeping himself abreast of a three-months' course of lectures; it will not suffice for one who aims at becoming a scientific botanist.

After this criticism on the plan of the work—a point on which it is inevitable that different experiences and different modes of looking at the subject will lead to different conclusions—the manner in which the plan is carried out claims all but unqualified approval. In particular is this edition a great advance, in both completeness and accuracy, on that which preceded it; and the editor may be congratulated on having got together a larger amount of trustworthy and accurate information than can be found in any similar work of the same size. Here and there the terminology is hardly abreast of that of the most approved recent writers, as where the term "perisperm" is still used as synonymous with "albumen," to signify any nutritious tissue intermediate in the ripe seed between the testa and the embryo; instead of being confined to the case where this tissue is developed out of that of the nucleus, as in the Nymphæaceæ, Piperaceæ, and Cannaceæ, in contrast to the much more common "endosperm" which originates primarily within the embryo-sac. But it is remarkable how very few instances there are of defects of this kind, so liable to occur in new editions of old standard works. We notice with satisfaction the tendency to anglicise certain terms, the foreign aspect of which is repulsive to the beginner. Why should not "epiderm" and "parenchyme" become universal, instead of "epidermis" and "parenchyma"?

It is difficult to decide which part of the work is in itself the most satisfactory, that on morphology, on classification, or on physiology, each of these constituting a clear and admirable treatise. The plan advocated in some text-books, of giving the authority of the actual observer for every statement, is not adopted here, and we think wisely. The beginner must take facts which he is not able to verify for himself on the authority of his immediate teacher; it is only when the beginner becomes a student that he has any occasion to trace every statement to its source, or is able to form any judgment on the relative value of different authorities. The advanced text-book of our second category should, therefore, be copious in references; the primer is better without them.

In the department relating to the classification of Cryptogams, Dr. Masters has had the valuable assistance of Mr. George Murray, of the British Museum; and this portion is enriched with a large amount of new and excellent matter. We see, however, here some of the inherent defects of a triple authorship, in the occasional want of harmony of different portions. Thus, while in the general introduction to classification the latest arrangement, that of Sachs's "Lehrbuch," fourth edition,[1] is given, the system actually adopted is substantially that of the second edition of Henfrey's book; Algæ and Fungi are still maintained as separate groups, and the former are divided into Characeæ, Rhodospermeæ,

[1] Evidently by an error of the press, the continued fraction of which the most common angles of divergence are successive convergents, is given as $\frac{1}{2}+\frac{1}{1}+\frac{1}{1}$, &c., instead of $\cfrac{1}{2+\cfrac{1}{1+1}}$, &c., a correction needful to render the sentence intelligible to the student.

[1] Sachs is, however, erroneously credited with locating Eugleneæ unde Protophyta. He has never, as far as we are aware, claimed for *Euglena* a position even in the vegetable kingdom

Fucaceæ, Phæosporeæ, Confervoideæ, and Diatomaceæ – an arrangement which will scarcely bear the light of modern science. The beginner will be likely to be set wrong by finding the term "reproductive organs" sometimes used for the organs between which a sexual process takes place, sometimes for the result of such process; and by reading that Algæ are reproduced by spores which are the result of the action of the antherozoids, while under the head of Fucaceæ the spores are the unfertilised germ-cells, and elsewhere the term appears to be confined to non-sexual reproductive cells which directly reproduce a plant resembling the parent. But these defects do not seriously detract from the value of the work.

Altogether those who want a thorough grounding in the elements of botany, as well as to be taken a little beyond the threshold in the various avenues which open out to the view of the student, will find a very useful and trustworthy guide in the last edition of this old standard.

ALFRED W. BENNETT

PAYEN'S INDUSTRIAL CHEMISTRY

Industrial Chemistry; a Manual for Use in Technical Colleges and Schools and for Manufactures. Edited by B. H. Paul, Ph.D. (London: Longmans, 1878.)

DR. PAUL has unquestionably rendered some service to the cause of chemical technology in this country by his translation of Payen's well-known work; nevertheless we think the service would have been still greater had he essayed to present us with an entirely original production. The fact is the translation has been made from a translation; it comes to us from the German through Stohmann and Engler's edition. As a consequence we miss much of what is good in Payen, whilst some things that are bad—notably faults in arrangement and inaccuracies of statement—remain. One is reminded of Macaulay's assertion concerning Johnson's Dictionary, which has been so altered by editors that its author would hardly recognise it. Whenever Dr. Paul is on his own ground he is excellent; the supplementary chapters on the chemistry of the metals, for example, are all that could be desired in such a work. The metallurgical portions, more particularly of the more important metals, are especially well done; we question if our language can show anything better on the subjects as regards clearness and conciseness and accuracy than the accounts of the operations involved in the extraction of lead, silver, and iron. But when the editor has to trust to French and German descriptions of technical processes errors crop up. For example, by far the greater portion of the phosphorus which the world requires is made near Birmingham and in Lyons, but neither of the two establishments which thus practically enjoy the monopoly of the manufacture carries out Nicolas and Pelletier's process as described in this work. Britain also furnishes practically all the bichrome of commerce, but the method described on p. 523 is not an accurate description of the present mode of production. The time-honoured cut on p. 181 no longer represents the method by which iodine is manufactured; nor is sulphur obtained by distillation from the traditional pots sacred to the memory of Morgiana and the Forty Thieves, which almost every compiler of an English text-book has sedulously copied. Saxony produces more than 90 per cent. of the bismuth which is found in commerce, but the liquation process described on p. 505 is no longer in use there. The article on "Friction Matches" is, also, scarcely up to date; the old operation of sulphuration is described in detail as if it were an essential feature in the manufacture; the reader is, indeed, told that the splints are now often dipped in stearin or paraffin, but he would certainly infer from the description that sulphur is generally employed; whereas it is only to meet the demands of lamplighters and sailors who specially need a match less easily extinguished by the wind than the ordinary varieties that a very few establishments continue to use sulphur. The composition of the inflammable paste used in France and Germany may, possibly, be represented by some or all of the eight formulæ given on p. 159, but the "compo" of the English manufacturer is altogether different from these. It is certainly remarkable considering the widespread use of lucifer matches, that so little should be known of their mode of manufacture; it takes quite as many persons to make a match as a pin, and the details of the making are equally interesting.

In the portions treating of pure chemistry inaccuracies are unfortunately scarcely less frequent. We willingly pardon the statement that "hydrogen is an elementary substance known in the free state only as a gas which has not yet been condensed by the greatest cold and pressure combined," even when the book makes its appearance several months after the great triumphs of our continental brethren; but the results of Pebal's work on chlorine peroxide ought certainly by this time to be part of the general stock of chemical knowledge. The statement that bromine solidifies at $-7^{\cdot}3$ is probably based on Pierre's inaccurate observation made more than thirty years since: Baumhauer has shown that the true freezing-point of this liquid is about $-24^{\circ}\cdot5$. The commendatory statement that "the bromine obtained from Stassfurt has the advantage over all other kinds of commercial bromine, that it is entirely free from iodine" (p. 179), is scarcely just to our own product: the bromine turned out by the Scotch makers actually merits this reputation, whereas there is evidence that the German product, to say the least, has not always deserved it. A distinguished German chemist, in studying the action of bromine on ethylbenzene, was, in fact, led astray by the use of the Stassfurt product, which he assumed to be pure: it was subsequently shown that the bromine used by him contained iodine, and the interesting fact was elicited that the action of this iodised bromine on the hydrocarbon is entirely different from that of the pure substance. The statement of Wollaston that our atmosphere does not extend beyond a height of forty-five miles above the sea-level (p. 54) is scarcely in conformity with current opinion: the observations of Herschel and of Secchi have certainly disproved the assertion as regards this particular limit, whilst the reasoning of Clausius has rendered it highly probable that in reality no limit exists.

A few more errors of commission and omission might be cited, but as it is very far from our desire to disparage a work which, by judicious revision, might fairly claim a very high place in our chemical literature, it is hardly

necessary to point them out at length, as a second edition will certainly see the greater number corrected. The book unquestionably supplies a need: it attempts to do for industrial chemistry what Mr. Watts's well-known work does for the theoretical part of the science, and we can wish it no higher measure of success than that it should meet with the favour which that work so deservedly enjoys. T.

OUR BOOK SHELF

Annual Report of the Superintendent of Government Farms. (Madras, 1877.)

THIS report of Mr. W. R. Robertson is one of great value; it comprises an account of the present state of native agriculture in the district of Coimbatore, and a statement of the work carried out at the experimental farm at Sydapet.

The information respecting native agriculture was obtained during a three months' tour of inspection. The general condition of the country is clearly most deplorable, and unless improved methods of farming are adopted there is apparently nothing but starvation and ruin before the majority of the ryots. The land irrigated from rivers and tanks forms about 1-25th of the area under cultivation; this land receives scarcely any manure save that supplied by the water; it nevertheless maintains good crops, and its money value is 20—25 times greater than that of land unirrigated. Mr. Robertson complains of the great waste of water: an ordinary crop of paddy will receive during its growth about twelve feet of water. If the crops were manured, far less water would suffice. A still greater saving would be effected by growing crops requiring less water; four or five acres of wheat or maize could be produced with the water required for one acre of paddy. Irrigation by means of wells is employed to some extent; the wells being private property, the water is used with far greater economy than is the case with river irrigation. Mr. Robertson strongly recommends an improved form of water-lift known as the "double mhote;" by this a single bullock can raise as much water as, on the native plan, is accomplished by four bullocks. Facilities for sinking wells should also, he thinks, be increased.

The unirrigated land has of late years very considerably decreased in fertility, and the number of cattle per acre is now only about one-half the number maintained in 1838. "The curse of Indian agriculture" is the employment of cattle manure as fuel, and this custom increases as the jungle is destroyed and brought under cultivation. The author strongly recommends the compulsory planting of fuel trees throughout the country; these would improve the climate as well as furnish the much-needed fuel. A striking feature of the unirrigated land is the entire absence of weeds, a true indication of the poverty of the soil. The greater part of this land is never manured, and is cultivated chiefly for grain crops, *Penicillaria spicata*, *Sorghum vulgare*, and *Eleusine coracana;* fodder crops and pasture are rarely met with. Were fodder crops more largely grown, the live stock increased in proportion, and the cattle manure all returned to the land, a great increase in fertility would be effected. The addition of organic manures to the soil, or the ploughing in of green crops, would also considerably increase the power of the soil to retain moisture, humus being of all the ingredients of the soil that which possesses the greatest water-holding power. Artificial manures are never employed: saltpetre may be purchased at a low price, but it is all exported, and never applied to the land.

It is pleasant to find, towards the conclusion of the report, that a School of Agriculture has lately been opened at Sydapet. Now that the causes of the agricultural depression have been clearly pointed out, we may hope that active steps will be taken to provide a remedy.

R. WARINGTON

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

The Size of the Indian Tiger

IN a book recently published entitled "Thirteen Years Among the Wild Beasts of India," by Mr. G. P. Sanderson, of Mysore, at pages 272, 273, the following remarks occur regarding the size of the tiger, and in reference to certain measurements of that animal given in a small volume entitled "The Royal Tiger of Bengal: his Life and Death," published in 1875.

The author (Mr. Sanderson) says,—"Regarding the size of the tiger, once a much disputed point, all careful observers are, I believe, agreed in accepting Dr. Jerdon's view ('Mammals of India') as thoroughly correct. He says, 'The average size of a full-grown male tiger is from 9 to 9½ feet,' but I fancy that there is little doubt that occasionally tigers are killed 10 feet in length, and perhaps a few inches over that; but the stories of tigers 11 feet and 12 feet in length, so often heard and repeated, certainly require confirmation, and I have not myself seen an authentic account of a tiger that measured more than 10 feet and two or three inches. I know," continues Mr. Sanderson, "two noted Bengal sportsmen who can each count the tigers slain by them by hundreds whose opinions entirely corroborate Jerdon. My own experience can only produce a tiger of 9 feet 6 inches and a tigress of 8 feet 4 inches as my largest. Of course writers start up now and again, as the author of the 'Royal Tiger of Bengal' did two years ago, and give us something like the following:—'The full grown male Indian tiger may be said to be from 9 to 12 feet or 12 feet 2 inches, the tigress from 8 to 10 feet, or perhaps in very rare instances 11 feet in length.' It is only fair to the author to state, however, that in the next paragraph he looks with doubt upon Buffon's tiger of 15 feet, and would only with greater hesitation accept the recorded statement that Hyder Ally presented a tiger to the Nawab of Arcot that measured 18 feet.

A portion only of the paragraph in my book is quoted; the most important, the first part, being omitted; it is as follows:—"The statements as to the length they (tigers) attain are conflicting, and errors are apt to arise from measurements taken from the skin after it is stretched, when it may be 10 or 12 inches longer than before removal from the body. The tiger should be measured from the nose to the tip of the tail as he lies dead, before the skin is removed. *One that is 10 feet by this measurement is large, and the full-grown male does not often exceed this, though no doubt larger individuals (males) are occasionally seen; and I have been informed by Indian sportmen of reliability that they have seen or killed tigers over 12 feet in length.*"

This account of the size of the tiger really, therefore, substantially agrees with Dr. Jerdon's, except that he says, "The stories of tigers of 11 feet and 12 feet in length, so often heard and repeated, certainly require confirmation." This confirmation is supplied. The following examples may be adduced:—

Lieut.-Col. G. Boileau killed a tiger at Muteara, in Oude, in 1861, that was over 12 feet, the skin, when removed, measured 13 feet 5 inches.

Sir G. Yule, K.C.S.I., has heard once, at least, of a 12-foot tiger fairly measured, but 11 feet odd inches is the largest he has killed, and that twice or thrice.

Col. Ramsay killed a tiger in Kumaon, 12 feet. I have myself seen and killed tigers over 10 feet, and have notes of some: one, for example, killed in Purneah, in 1869, 10 feet 8 inches in length.

Gen. Ramsay mentions the skin of a tiger partly killed by himself near Benares that measured over 12 feet. This had no doubt been stretched, but it was a very large tiger.

Col. J. Sleeman does not remember having killed a tiger

measuring more than 10 feet 6 inches in the skin. He saw the skin of one at Dinagepore, over 12 feet in length; this was also no doubt stretched. Col. J. Macdonald has killed a tiger of 10 feet 4 inches. He says: "I do believe tigers have exceptionally reached 12 feet."

The Hon. R. Drummond, C.S., killed a tiger 11 feet 9 inches in length before being skinned.

Col. Shakespeare killed a tiger of 11 feet 8 inches.

In regard to the allusion to Buffon's tiger of 15 feet, and Hyder Ally's of 18 feet, I refer to but to express my distrust of them.

It is needless to adduce further evidence. I repeat that though male tigers over 10 feet may be uncommon, they do *occasionally* (and I said no more) attain the greater size.

June 17 J. FAYRER

Zoological Geography

IN the annual address of the President of the Geological Society of London, just issued, an extract is given by anticipation from the forthcoming work of Dr. Günther, on the gigantic land-tortoises, wherein that naturalist discusses the question of the geographical connections by which the tortoises of the Mascarine region may have been related to those which are found in the Galapagos Islands.

As neither in this extract nor in the presidential discussion do I find any allusion to the circumstance that, according to the paper of Dr. Litton Forbes, in the *Journal* of the Geographical Society for 1877, *Didunculus strigirostris*, a near congener of the Mascarine *Dodo*, is living in Upolu, one of the islands of the Navigator group, I venture to call attention to it. Since this island lies in 14° S. lat., and is distant 130° of longitude, in a direct line eastwards from the Mauritius, towards the Galapagos, the presence on it of this ground-bird seems to show that the ancient geographical connection from the Mascarene to the Galapagos Islands was eastwards across the Indian and Pacific Oceans, rather than, as Dr. Günther thinks, westwards by way of Africa, the Atlantic, and South America; for by it more than three-fifths of the 210° of longitude, which in the easterly direction separates the Mascarine region from the Galapagos, and presents a difficulty to Dr. Günther, are bridged over.

The Navigator group, together with a multitude of other islands in the South Pacific, which extend to within 40° longitude of the Galapagos, appear to be small remnants of that continent of very remote geological age of which Australia, New Guinea, and New Zealand constitute larger remnants; and perhaps I may be allowed to observe that the suggestion made by Dr. Günther in reference to the extinction of the gigantic tortoises, viz., that they may be supposed to have once spread over the whole of the large area which connects the places of their present occurrence, but to have been unable to survive the arrival of man or the large carnivora, is precisely that which, many years ago,[1] I offered as the explanation of the extinction of the great wingless, or ground-birds, wherever they were unprotected from these enemies by insulation. This formed part of the argument by which in 1860 I endeavoured to show to the Geological Society that most of the land-tracts of the southern hemisphere were remnants of an ancient continent, which had become insulated at different times during the secondary or mesozoic period.

SEARLES V. WOOD, JUN.

Martlesham, Suffolk, May 27

Time and Longitude

IN NATURE, vol. vii. p. 68, the Rev. J. Pearson asked, "In what part of the globe and in what meridian does October 20 end and October 21 begin?"

The question was answered by several correspondents. Still the following may be interesting as a matter of fact in connection with Mr. Latimer Clark's letter in your issue of May 9.

The date was fixed in many of the Pacific Islands by the early missionaries, who, sailing eastward from Australia, kept the date of the eastern hemisphere after they had crossed the meridian of 180°. This imaginary boundary line cuts through the Fiji Islands, the principal islands of the group being in the eastern hemisphere. It would, of course, be inconvenient to have two dates in one group of islands, especially as the meridian of 180° passes through the north-east point of Vanua Levu, the island of Taviuni, and another small island. Such an arrangement might possibly lead to the necessity (if a stickler for strict accuracy should build his house across the line) of a person going from one day to another by passing from one part of the house to another. It would, to say the least, be awkward to sleep during the night of October 20–21 and on arising on the morning of October 21, by simply walking into the breakfast-room, to cross the boundary-line, and find oneself back into the beginning of October 20.

The Tongan and Samoan Islands are a few degrees east of the meridian of 180°; consequently they ought to be a day behind the neighbouring Fiji group. But hitherto their chief commercial intercourse has been with Australia and New Zealand; and this, for the sake of convenience, has led to the date of the eastern hemisphere being retained up to the present time, although a change has been advocated in Samoa more than once.

In consequence of the present arrangement these little islands have the honour of leading the world in the matter of time, whereas they ought, according to their geographical position, to wind up the rear. There is, however, one drawback to the honour: all our dates, when compared with those of the rest of the world, need to be put back twenty-four hours. This should be remembered in connection with observations of natural phenomena. To obtain local time we add 12 hours 33 minutes to G.M.T. instead of subtracting 11 hours 27 minutes.

S. J. WHITMEE

New Lunar Crater

I was much interested in the account which your last number (vol. xviii. p. 197) contained of the presumably new lunar crater discovered by Dr. Klein in the Mare Vaporum. Is it really necessary to ascribe the formation of such a crater to *present* volcanic action? It seems to me that this singular phenomenon of the birth of a new crater may be more likely owing to such action having, in long-past ages, left (as in all probability it would leave) extensive caverns beneath the visible surface of our satellite. Such caverns might, in consequence of the gradual changes which the action of the sun's rays, alternating with intense cold, must produce on the lunar rocks, occasionally give way. A crater-like cavity would then be caused on the moon's surface by this subsidence, such as are not unfrequently seen in mining districts where old workings have fallen in. The fact that the new crater is elliptical, and not round, seems to add to the probability of its having been caused by some such "settling" process. If the crater were produced by active volcanic agency, it would surely be circular, or nearly so. I do not find this mode of quasi-crater formation suggested in Nasmyth's book, nor, so far as I can remember, in any other. Is it not, however, a possible cause of change on the surface of our satellite?

EDWARD GREENHOW

Cardiff, June 22

Opening of Museums on Sundays

I AM delighted to find from my friend Prof. Dyer's letter tha I was mistaken in the belief that the Maidstone Museum was "the first and only scientific museum that has yet been opened on Sunday in the United Kingdom."

Still the fact that "the Botanical Museum of the Royal Gardens, Kew," is not closed when the Gardens are opened to the public on Sunday, a fact which I had overlooked, although important enough in itself, is not for a moment to be compared with the deliberate opening of the Maidstone Museum by the town authorities.

Had the Botanical Museum not been attached to the Royal Gardens there can be no doubt that it would still be closed on Sunday, as the British and South Kensington Museums are; the opening of one of these institutions would be a parallel case to that of Maidstone.

I am happy to be able to state that a motion for rescinding the resolution under which the Maidstone Museum was opened on Sunday has been defeated in the Town Council.

May I express a hope that there are many towns which will not long remain behind Maidstone in this matter.

W. H. CORFIELD,
Chairman of the Committee of the Sunday Society

10, Bolton Row,
Mayfair, W.

P.S.—I have just been reminded that the Natural History Museum in Dublin has been open to the public on Sundays for

[1] *Quarterly Journal of the Geological Society* for 1860, p. 329; *Phil. Mag.* for March, April, and May, 1862.

some months past, a fact of which the members of the British Association may take advantage this year. It is now Scotland's turn. W. H. C.

Ophrys muscifera

On the afternoon of June 2, 1878, I observed some new facts, which, I think, are of importance in elucidating the hitherto mysterious fertilisation of the Fly-Orchis. In sunny weather and under normal conditions the labellum secretes fluid, and a broad central longitudinal stripe of its surface is covered with small drops. Of fifty fresh flowers I found the labellum in thirteen covered with drops, in twenty-five shining with adhering moisture, in twelve without any conspicuous trace of fluid. The two small shining projections on each side of the base of the labellum (the sham-nectaries of Sprengel) were quite dry in all the flowers. In one flower I saw a fly (Sarcophaga sp.) sitting on the labellum and licking the drops. Its head was directed towards the base of the labellum. On my approaching it flew away before having reached the sham-nectaries, and the flower visited by it was found without pollen on the stigmas, and with both pollinia in their cells. Nevertheless, it is most probable that this fly, if not disturbed by my approach, would have stepped forward on the labellum, and, trying one of the sham-nectaries, would have removed one of the pollinia and perhaps transferred to the stigma of another stem, in the manner described by Charles Darwin ("Fertilisation of Orchids," p. 47).

For observing the fluid secreted by the labellum it may be essential to examine plants in their native habitats, not plucked ones. Hermann Müller

Lippstadt

The Jura

In the midst of the enjoyment of quiet and beautiful scenery I cannot refrain from writing, in the interest of geology, to attract attention to the facilities for the study of the Jura range afforded by a railway recently opened from Bâle, *viâ* Délémont and the Münster Thal, to Bienne. It crosses the range at, relatively to the anticlinal, a considerable angle, necessitating no less, as I am told, than twenty-five tunnels great and small (I did not count them myself).

Consequently, in a short morning's railway ride the traveller sees a vast deal of Jurassic structure, added to which the Münster Thal, formerly a rather tiring day and a half's drive, is replete with rock, forest, and pasture scenery of very great beauty.

Travellers thus crossing the Jura on their way to the Alps and returning from Lausanne by Vallorbes to Paris, will thank me, I think, for pointing out what, if only from a scientific point of view, are two recently-developed routes, far more interesting than the customary approaches to this land of wonders. I repress poetic and mountaineering sympathies.

Pension Mounoud, Veytaux-Chillon, Canton Vaud, June 21 Marshall Hall

THE TRANSIT OF VENUS PHOTOGRAPHS[1]

The photographs which have been measured were taken with the five photoheliographs made by Mr. Dallmeyer for the Transit of Venus expeditions, on "patent plates" 6 inches square, the images of the sun being very nearly 3·9 inches in diameter. The dry process of Capt. Abney was used throughout.

The measuring instrument, the determination of the errors of its glass millimeter scale, and the method of obtaining the optical distortion of the photoheliographs, have already been described in the Society's *Proceedings*. It has been found by an elaborate investigation that the lines of equal distortion were sensibly circles concentric with the centre of the field. The actual correction for distortion for that zone of the field in the points to be measured generally fell, was exhibited on the board, and was almost identical for all five instruments.

Before commencing the measures of a negative, the position of the line of centres was marked upon the film by a simple mechanical process. This operation has been performed independently by Mr. Burton and myself, with no sensible difference. I have paid no attention to the marks left by Mr. Burton on the plates, and found that my own coincided with them in direction.

In placing the negative in the instrument the circular carrier was turned about until the line of centres was truly parallel to the direction of the sliding motion of the microscopes.

When the negatives are placed under the microscope with an amplification of only five or six diameters, the limbs of both planet and sun, even those which are pretty sharp to the unaided eye, become extremely indistinct, and the act of bisecting a limb with the wire or cross of the micrometer is mere guess-work. The deposit of silver fades off gradually to nothing, and the denser the film the broader generally is the zone of fading off and the more uncertain the measures. In many cases the difficulty is aggravated by ruggedness due to atmospheric disturbances, but the smooth and gradual fading off is the chief cause of uncertainty.

There is only *one* really sharp picture in the whole collection, including the Indian and Australian contingents, and that is one of Capt. Waterhouse's wet plates, taken at Roorkee with a Dallmeyer instrument precisely similar to the others.

It should be remarked that in these instruments the artist has attempted to unite the photographic and visual foci on the collodion film. No doubt some sharpness of the photographic image was thus sacrificed, but this has little or nothing to do with the unfortunate failure of the photography generally.

Each photograph has been measured six times by Mr. Burton and six times by myself. I am not able to include in my series of measures all the photographs measured by Mr. Burton, for the reason that when some of them were viewed through the microscope I could see nothing to bisect, either from the extreme faintness of the film, or from its too gradual fading off.

Mr. Burton generally employed a cross of webs, but I have preferred a single very fine web, the breadth of which was eliminated in the mean by the mode of bisecting.

It had been suggested that the measuring instrument should possess the power of rotating the sun's image about a mechanical centre. This would be useful in some cases of rugged limbs when the sun's image was not rendered elliptical by refraction, but in my opinion would make no material difference in the accuracy of measurement. The rotation could only be applied to the limbs of the sun, whereas, perhaps, the greatest difficulty had been at the limbs of the planet.

From the measures, corrected for distortion, were obtained the photographic diameters of the sun and of *Venus*; the former presumably enlarged, the latter diminished by irradiation in a sensibly equal degree. The sum of the measured diameters in millimetres was compared with the sum of the tabular diameters, subject to errors, for the scale value, and thus every photograph furnished its own scale.

The measured distance of centres affected by errors of semi-diameter was then compared with the tabular distance affected by errors of parallax, right ascension, and north polar distance. From each photograph was formed an equation involving all the unknown quantities, of which the errors of parallax and of semi-diameters were the more important.

The rigorous solution of the equations resulting from Mr. Burton's measures is,

$$\begin{aligned} \text{Mean solar parallax} &= 8''\!\cdot\!165 - \cdot 209\,(dR + dr) \\ d\,\text{R.A.}\ \ldots &= +\,5\cdot 38 + \cdot 287\,(dR + dr) \\ d\,\text{N.P.D.}\ \ldots &= -\,5\cdot 10 - \cdot 882\,(dR - dr). \end{aligned}$$

The parallax deduced being absurdly small—altogether inadmissible, indeed—the Astronomer-Royal suggested that the quantity $(dR+dr)$, or the sum of the corrections to the tabular semi-diameters, should be considered the

[1] Paper read by Capt. Tupman at the meeting of the R.A.S. on June 14, on the measurements of the Transit of Venus photographs.

only unknown, and that approximate values of the true solar parallax and of the errors of R.A. and N.P.D. should be substituted in the equations. This was done, the mean solar parallax being taken at $8''.85$, d R.A. as $+5''.81$, dN.P.D. as $-5''.33$, which values resulted from the general solution of the whole of the contact observations, and the following values of $dR + dr$ were obtained:—

Station.	By Burton's Measures.	Number of Photographs Measured.	By Tupman's Measures.	Number of Photographs Measured.
Luxor	$-0''.96$	11	$-0''.13$	12
,,	-1.18	11	-1.61	11
,,	-2.21	11	—	—
Honolulu	-1.29	11	-0.21	12
,,	-1.75	11	-0.54	12
,,	-0.71	10	—	—
Rodriguez	$+1.19$	11	$+2.44$	9
,,	$+0.23$	10	$+2.49$	6
,,	$+2.10$	10	$+2.74$	8
,,	$+0.14$	11	$+1.27$	10
,,	$+1.46$	11	$+2.31$	9
Burnham, N.Z....	$+0.68$	13	$+1.89$ / $+1.38$	3 / 10
Kerguelen	$+1.51$	8	—	0

The above is perhaps the best way to exhibit the nature of the discordances. They might also have been shown as apparent errors of the tabular distance of centres.

The discordances of any one station are too large to admit of the measures being employed with advantage for the determination of the solar parallax. They are due to inherent defects of the photographic images. The reason why at the two northern stations the signs are all *minus*, while at the three southern they are all *plus*, is at present obscure, and I am not prepared to offer any suggestion as to the cause.

THE NORWEGIAN NORTH ATLANTIC EXPEDITION

I SEND you inclosed a clip from the *Dagbladet*, containing the route of our expedition for the coming summer. I hope to be able to send you notes from our expedition during our several stays in Hammerfest. H. MOHN

"According to the plan of this expedition, the *Voeringen* was to start from Bergen on its third and last cruise on the 15th inst. It will probably have reached Tromsoe by the 19th inst., and, after taking on board a pilot acquainted with the northern waters, have immediately proceeded to Alten Fiord, mainly to inspect the meteorological station there, and to examine the animal and plant-life of the Fiord bottom. The magnetic observations required for regulating the compasses, &c., were to be made at Hammerfest between the 21st and 24th inst. The course was then to be set eastwards, in order to examine the relations of depth and animal life, &c., in two of the fiords of Finmark. After touching at Vardoe on the 27th, the voyage is to be continued to a point midway between Vardoe and Novaya Zemlya, in order to take soundings and determine the boundary of the ice-cold water in the East Polar Sea, which hitherto in these regions has only been observed at Bear Island by the well-known Austrian Polar explorer Weyprecht, in his excursion thither several years ago in the Tromsoe yacht *Samson*. This thorough examination of the sea off the north-east coast of Norway, towards Novaya Zemlya will be of special importance for the study of the migrations of the "lodde" (*Malotus arcticus*), as it is probable that it is there that this salmon-like fish has its abode whence in spring it makes its way in large shoals to the coast of Finmark to spawn, pursued by the cod, which follows it and is accordingly taken; while the so-called "lodde" fish, as is well known, is not fished for, because it is not suitable for human food, on account of its penetrating unpleasant odour.

This eastward cruise of the *Voeringen* will scarcely occupy more than ten days, as the sea is here so shallow that taking soundings, &c., need not occupy much time, and the *Voeringen* may accordingly be expected back at Hammerfest on July 7, to take on board coal, water, &c., for a new cruise to the westward in the navigable waters north of Jan Mayen, which the expedition visited last year; thence to the Greenland ice, where the seal fishing is usually carried on, in order to ascertain the boundary between the Greenland Polar current and the Gulf Stream. The stretch of sea that will be traversed by the *Voeringen* has not hitherto been surveyed, and here will doubtless be found, by means of the lead, the beginning of the great Polar sea-depth which runs in between Greenland and Spitzbergen. The *Voeringen* will then return to Hammerfest to make preparations for the third cruise.

This cruise, which will be the last, will be commenced on July 29, and be occupied with the survey of the navigable waters between Bear Island and Spitzbergen, where the well-known shark fishing is prosecuted, and the great sea-deeps off the west coast of Spitzbergen (76° to 80° N. lat.) which hitherto have only been surveyed, and that incompletely, by two of the Swedish expeditions. The *Voeringen* will go as far north as it can for ice, but there is certainly no great expectation that the Norwegian expedition will be successful in carrying off the prize in the competition with other nations to reach the North Pole, for the *Voeringen* will certainly soon meet with ice in the navigable waters on the north coast of Spitzbergen, and it is not fitted out for a North Pole expedition. Leaving it to the enterprising publisher of the *New York Herald* and others to endeavour to reach this goal, the *Voeringen* will, instead, after having turned southwards, survey the fiords and banks on the west coast of Spitzbergen. There the Norwegian fishermen, as is well known, carry on a not inconsiderable cod-fishing, the yearly catch numbering 300,000 to 400,000 fish. But if we keep in view the recent discovery of the great fishing bank off the Lofoten Islands, it will be seen that the fishermen need not undertake the long and troublesome voyage to Spitzbergen to catch cod. They will find superabundence of larger and better fish at the banks off Vesteraalen, so to speak, lying before their own door. But these Lofoten fishing banks are for the time being visited by the Norwegian fishermen as little as the bank abounding in fish which lies off the Froey Islands (north-west of the mouth of Trondhjem Fiord), although the latter was known to old fishermen. The surveying-steamer *Hansteen* has now mapped it. It is besides beyond all doubt that one of the practical results of the Norwegian North Atlantic Expeditions will be a better turning to account of the rich fishing banks of whose position, animal and plant life, more precise information has now been obtained.

The return from Spitzbergen will take place at the end of August, and the *Voeringen*, after having touched at Hammerfest or Tromsoe, and Bergen, where the members of the expedition resident there will land will

probably resume its course in the middle of September, terminate its voyage in the harbour of Horten."

The members of the expedition are the same this year as last, viz., Profs. Dr. H. Mohn, meteorologist; Dr. G. D. Sars, Dr. Danielssen, and Herr H. Friele, zoologists; candidate Tornœe, chemist; assistant-candidate Schmelck, physicist and chemist; and the landscape-painter Herr Schiertz, as artist. The *Voeringen* will be commanded this year, as formerly, by Capt. Wille of the Royal (Norwegian) Navy, the second in command being the sailing-master, Capt. Greig. The expedition carries with it several valuable new instruments for measuring more exactly the temperature of the water at great depths; some of them have, with great good will, been obtained from the members of the English *Challenger* expedition. As in the preceding years, Prof. H. Mohn will send to NATURE communications from the expedition.

PHYSICAL SCIENCE FOR ARTISTS[1]

VI.

THE diagrams given in my last article should have made it quite clear that the various sunset and sunrise colours are due to the absorption produced by different thicknesses of aqueous vapour; that the colours of clouds are due to light falling upon them after absorption by different thicknesses of aqueous vapour; and finally that the blue colour of the sky in the zenith is due to the fact that the pure gases in our atmosphere exist in that molecular grouping which vibrates in harmony with the short waves of light.

The blue sky, however, is scarcely ever a true blue. Between us and it there is ever a misty veil which reflects to us the white light of the sun, as an examination of it by a pocket spectroscope will prove to anybody. It is to the variation in the quantity of this misty veil that the difference in the colour in the sky at great and low elevations, in different climates, and in the same climate, when clouds are about to form and when scarcely the germs of clouds are present, is to be ascribed. The thickness of our atmosphere is so moderate that neither the hypothetical red nor the blue molecules of aqueous vapour are competent, except during thunderstorms, to influence its colour as they undoubtedly do near the horizon.

A glance at Fig. 4 in the last article will explain how it is that sometimes in the case of clouds we find the before-stated order of sunset colours reversed. If, for instance, we imagine a cloud lying along the curve $x s'$, an observer at o will see a cloud at x higher above the horizon than one at s', but the cloud at x will have received light through a greater thickness of atmosphere than the cloud at s'. The red, therefore, at x will be more *foncé* than at s'; the order of colour, though not of brilliancy, will be reversed.

So far we have considered these colours looking towards the rising or setting sun. Let us now turn our back on that luminary. It will be at once obvious that if, for instance, we take a point on the horizon, there will be an enormous increase in the thickness of atmosphere traversed by the ray; indeed, we may say that for this point the absorption will be threefold. Hence a considerable reduction of light, a ruddier tinge, due to the increased absorption of the more complex molecules, and a mingling of the ruddier light with the blue sky.

In the voyage which I made to India in 1871 I scarcely ever missed a sunrise or a sunset, and although the point of sunrise or sunset was almost always the scene of a succession of glories unsurpassed in beauty, the point opposite was, if possible, more interesting, the colours were more subdued, and of a more composite order, but the work of law went on there, as elsewhere. If any clouds happened to be overhead, their greatest glory, which, as I have already shown, can only be put on when the sun is below the horizon—and the sun rises or sinks much more rapidly there than with us—was the herald of the shadow of the earth on the illuminated sky, which crept on a gigantic, mysterious crescent. That the shadow of the earth could thus be seen was new to me, and I am the more glad, therefore, seeing that many may doubt it still, to substantiate my observation and its explanation by a quotation from Prof. Brücke, one of the most distinguished members of the Vienna University. Prof. Brücke has been doing on the Continent what I have been attempting to do in these articles, and just before my last one appeared I saw in *La Revue Scientifique* an extract from his forthcoming work "Principes Scientifiques des Beaux Arts." I am delighted to see how much at one we are, but for the moment I shall content myself by giving what he says on the point to which I have referred. Talking of sunset he writes:—

"We see on the horizon to the east a grey blue stratum rising higher and higher, and stopping at that portion of the sky coloured red: it is the shadow of the earth.

"The shadow of the earth must always encounter an unilluminated part of the atmosphere. As this shadow does not fall on a surface, but on a great number of particles spread abroad in space, it is material, that is to say, it has three dimensions, and we see it, foreshortened—in perspective.

"Sometimes the regions above it are divided in a radial direction into sectors, some of which are dark, like the shadow of the earth, others red. These resemble in the sky the rays of the aurora borealis, and often change their place and size; in French they are termed 'les rayons de crépuscule.' They are due to the fact that in the path of the solar rays there are masses of clouds which only give passage to isolated ones here and there, which make their presence felt by the luminous train which they leave among the particles of the atmosphere. Hence arise those red prismatic masses spread abroad in the air east and west. At the zenith we do not remark them, because the vision cuts across them, and the stratum of illuminated particles is not thick enough to render them sensible; but we see them painted on the eastern sky because we regard them obliquely in the sense of their length; we see them in perspective. By their nature and their mode of origin they do not differ from the beams which the setting sun throws between the intervals in the clouds, nor from those which it sometimes casts in the morning or afternoon through the clouds, when the peasants say that 'the sun is drawing water.' 'Voilà un bouillon qui chauffe.'"

This paragraph not only supports my view, but it opens up several very interesting points on which, if space permitted, there would be much to say; one or two words, however, must suffice.

The rifts to which Prof. Brücke has drawn attention do not always arise from clouds; in fact, they are not seen in their greatest vividness when they do. One evening I saw them thrown, in a perfectly cloudless sky (in fact, there had been no cloud all day), by the sky-line of Socotra, which island we had passed during the day, and which was below the horizon at the time. Capt. Parish, in command of the *Mirzapore*, to whom I appealed at the time, took the bearing of these rifts, which, in their sharpness and magnitude, were almost appalling, and put the question beyond all doubt.

With regard to the "sun drawing water," artists should note the absence of all colour and the radial direction of the beams, all meeting in the sun's place. For some reason or other many artists are not yet quite clear about this appearance, and compromise matters by making the beams look like a distant rain-shower. There are some notable examples of this in the South

[1] Continued from p. 157.

Kensington Museum. That phenomena so diverse in their origin and appearance should be mistaken for each other does not say too much in favour of the cultivation of the observational faculties of artists as a rule.

I shall next refer to two or three other questions which have been dealt with by Prof. Brücke in the article to which I have referred. Prof. Brücke is again with me to a certain extent in tracing the origin of most sky-colour to a defect of the blue light, but he does not make the attempt to run it to earth that I have done, by ascribing it to aqueous vapour; indeed he considers it rather due, I take it, to the presence of solid particles in the air. Thus, after pointing out that the dawn is generally orange, and the sunset redder, he states that at night the quantity of molecules capable of troubling the air is generally greater. For my own part, I should be inclined to ask whether, during the night, the molecules of aqueous vapour which absorb the blue have not been driven into higher forms—*dew* being one of them—owing to the reduction of temperature. This would at once explain not only the generic difference between sunrise and sunset colours, which is more marked here than in the tropics, but also the golden instead of red sunsets which accompany the formation of cloud.

Another point of difference. Prof. Brücke considers green sky as an effect of contrast produced by the quantity of red light which enters the eye. I cannot agree to this, first, because I have given a physical reason for the green; and secondly, because I have observed it without any strong contrast of colour to mislead the eye. The considerable darkening of the green after sunset is, I believe, purely physiological; and it is an effect of so curious a nature, that it raises several interesting questions with regard to the manner in which the eye grapples with the middle colours of the spectrum, namely, the orange, yellow, and green, which can be made to change to a certain extent according as the light is more or less intense, which does not happen with the other colours.

The changes in mountain scenery form the subject of several interesting remarks by Prof. Brücke. As long as distant mountains are illuminated by a high sun, their outlines are not very clear; because, as he well puts it, the reflection of this light from the lower strata of the atmosphere is then so great that the illumination at the horizon, where mountains are, is as strong as where they are not. He then points out that at night the setting sun fills the sky towards the west with a great brightness which renders the profiles of the mountains between us and the sun much darker. Their contours are neatly detached, but it is not only on the horizon that this is seen; the various chains are better distinguished, and appear one behind the other like the scenes in a theatre, because the light in which we see them does not come from them but from the interposed air. The sides of the mountains which we see are dark because the other sides are turned towards the sun, but the various thicknesses of air interposed between us and them reflect to us the sunlight; hence the atmosphere of a picture is truly the work of the air.

Here is what Prof. Brücke says about sunset tints; I do not follow him in all his explanations:—"When the sun reaches the horizon and the red tint is developed, the colours of the landscape change in their turn and the mountains themselves appear red when we regard no longer their shadows but the illuminated air which lies in front of them." It appears to me this gives too much work to the air; a rock surface is generally as capable of dispersing red light which falls upon it, as a molecule of aqueous vapour is; "still the tint has not the intensity of the alpine colour; it is a red less intense and more empurpled, which sometimes approaches even the violet or the lilac."

I shall have a word to say on this, but I will first give Prof. Brücke's explanation:—

"Two causes are at work in this latter case; the first is the mixture of red and blue light. At night when the sky is clear the shadows are coloured a strong blue. The shadow region is illuminated by the blue light of the sky, and appears more pronounced, owing to the contrast of the reddish-yellow light, as we have already seen. The illuminated air reflects the blue rays more abundantly than the red ones, and consequently the former have the ascendency. If not scientifically correct, it is at least practically so, to suppose the blue light in which we see the mountains bathed after sunset to be mixed with purple or lilac. The second cause of the violet tone in the distances to the west is to be found in the frequent contrast. In the west, in fact, a great part of the sky is illuminated by yellow light; often this yellow is a perfect sulphur-colour, which contrast makes objects even in the middle distance, which turn their dark sides to us, appear violet; thus, looking to the west, dark, unploughed earth appears violet when the majority of terrestrial objects turn their dark sides towards us."

An observation I made at Cannes last year leads me to think that the whole cause of this purple colour has not been stated in the foregoing. It was near the hour of sunset, and I was looking towards the south-west, delighting in the blue colour at the foot of Les Estrelles—while their crests were being gilded by the sunset—when, almost instantaneously, the valley to the north of these hills was enfiladed by a beam from the sun itself, which threw part of the aqueous vapour in the valley into a frenzy of gold. This gradually got ruddier as the sun got lower, and the amount of vapour lighted up between me and the blue vapour at the foot of the hills was at the same time reduced; the blue and the red then melted together into the richest and most beautiful purple that I, at all events, have ever seen.

We have only, then, to assume that, when we thus see purple, that colour is produced by a mixture of particles, some of which are reflecting to us the blue light of the sky, because they can do no other, while others, again, are reflecting to us the red light of sunset, because it is more powerful than the light from the sky.

J. NORMAN LOCKYER

AN ECLIPSE SPECTROSCOPE

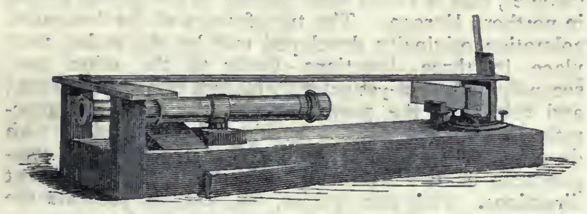
SOME little time ago I communicated to the Royal Society a suggestion for the use of Mr. Rutherfurd's reflection gratings in obtaining photographs of the coming eclipse. The plan suggested was that the grating should be placed short of the focal point of a telescope, and at right angles to its axis, and that the diffracted images of the chromosphere should be received on photographic plates adjusted for the different orders of spectra on either side the axis. I am glad to learn from Prof. Newcomb that the value of this method of observation will probably be tested by Prof. Young, who is in charge of one of the six expeditions already organised to observe the eclipse. The chief defect in this mode of observation lies in the difficulty of determining the position of the lines photographed, supposing the chromospheric spectrum to vary considerably from the ordinary solar one so far as the intensity of the lines is concerned; and as it seemed desirable that these gratings should be utilised for less serious attacks, I have recently been endeavouring to see if the method can be improved.

The annexed woodcut shows one form of the new arrangement, which has many conveniences. It is a rough model on wood, but will suffice to show the method of use.

The grating, which is free to rotate, is placed in front of a little telescope of low magnifying power and the stand which carries both is so placed and the grating so

adjusted that the image of the light source is seen in the little telescope reflected by the general surface of the metal grating. Supposing a circle and micrometer attached, represented by the wooden bar at the top, the reading would now be zero.

Next, and this is the new point, a piece of glass silvered on the front surface is fixed with its surface parallel to the surface of the grating, the side of which it covers. When this is in perfect adjustment the images produced by the movable grating and the fixed mirror are superposed. Let us suppose the light source to be a Geissler tube, we get a single image of it; the fixed mirror is then very slightly inclined, so that its image lies a little above or below the one due to the grating. We now by the movable arm at top rotate the grating, the grating image vanishes from the field of view, and in a little time, if the rotation is continued, the blue of the first order spectrum makes its appearance. Each coloured image in the spectrum can in turn be brought to coincidence, with the non-dispersed image of the tube thrown by the fixed mirror, and readings of considerable accuracy can thus be obtained. The illumination of the image due to the



fixed mirror can be easily regulated by changing its position with regard to the axis of the telescope prolonged; in no case of course should any part of the ruled surface of the grating be covered. With a ring slit illuminated by the vapours of different metals, the phenomena observed are very interesting and novel; with the fixed mirror slightly inclined, the image from the fixed mirror always in the centre of the field of view forms a capital point of comparison.

More light is gained by employing an object-glass of short focus and placing the grating and fixed mirror at such a distance inside the focus that the beam falls on the ruled surface and a small fraction of the fixed mirror.

I hope the suggestion does not come too late to enable it to be utilised by the outposts during the coming eclipse. If it helps in enabling us to determine the position of the chromospheric line near H, the time I have spent on the little model will not have been thrown away. I may add that I have found that a prism of 60° dense flint placed in front of the lens of an ordinary photographic camera will give us, if properly focussed, a most useful spectrum of the eclipsed sun.

J. NORMAN LOCKYER

OUR ASTRONOMICAL COLUMN

NEAREST APPROXIMATIONS OF SMALL PLANETS TO THE EARTH'S ORBIT.—Out of the 187 minor planets now known there are ten which approach the earth's orbit at their perihelia within 0·9 of her mean distance from the sun, and which may therefore afford the most advantageous opportunities for determination of the solar parallax by one or other method of observation of these bodies, already successfully applied: *Medusa* is omitted on account of uncertainty of elements. The nearest approach, 0·798, is made by *Clio*, discovered by Luther in August, 1865. *Æthra*, detected by Watson in June, 1873, makes the nearest approach to the sun 1·614; but the great depression of the planet below the plane of the ecliptic, at perihelion, prevents so near an approximation to the earth's orbit as in the case of *Clio*. The following is a tabular view of the distances in the ten cases referred to:—

	Perihelion Distance.	Heliocentric Latitude in Perihelion. ° ′	Distance from Earth's Orbit.
84. Clio	1·805	+ 1 57	0·798
132. Æthra	1·614	− 23 45	0·813
18. Melpomene	1·796	− 7 9	0·815
43. Ariadne	1·834	+ 0 48	0·818
12. Victoria	1·823	+ 7 40	0·828
80. Sappho	1·835	+ 5 53	0·842
8. Flora	1·856	− 5 45	0·874
33. Polyhymnia	1·890	− 0 53	0·882
42. Isis	1·890	− 6 53	0·892
50. Virginia	1·896	− 0 47	0·896

If we extended our limit to 1·0 we should include, in addition to the above, *Felicitas*, *Phocea*, *Euterpe*, *Thyra*, *Echo*, and *Feronia*.

While referring to the small planets it may be remarked that, between the perihelion of *Æthra* and the aphelion of *Hilda*, there is a difference of 2·98; and between the aphelion of *Flora* and the perihelion of *Hilda* 0·76, or upwards of three-fourths of the radius of the earth's orbit. The periods of *Flora* and *Hilda* being respectively 3·27 and 7·85 years exhibit a difference of 4·58 years. These are the extremes, as they result from the latest and most complete catalogue of elements.

According to the last Circular of the *Berliner Jahrbuch*, the following names have been proposed:—For No. 177, *Irma*, for 180, *Garumna*, and for 186, *Celuta*.

MEASURES OF DOUBLE STARS.—Many applications for copies of the earlier volumes of "the Leyton Observations" having been received after the edition had been exhausted, Mr. J. Gurney Barclay has issued a fourth volume containing the double star epochs from the commencement of observations at Leyton, with the addition of results to the end of 1877. This part includes also occultations and phenomena of Jupiter's satellites since 1865. The notes on the double-star observations comprise the principal epochs of other observers. The small companion of Procyon at a distance of about forty-five seconds, to which attention was first pointedly directed by Mr. Barclay in January, 1856, had the following position for 1863·23, angle, 294°·88, distance 45″·9, which, corrected for the proper motion of Procyon in the interval, gives for 1879·0 angle, 319°·3, distance 47″·3.

The *Astronomische Nachrichten*, Nos. 2196-99, contain measures of double-stars made by Dr. Doberck at the observatory of Col. Cooper, Markree Castle, Sligo, from the end of 1875 to the spring of 1878. The list includes most of the well-known binary systems. γ Coronæ was single in the Markree instrument in 1876-77.

Mr. Ormond Stone, Director of the Observatory at Cincinnati, writes with respect to a remark in a notice of the Cincinnati measures of double-stars, which appeared in this column, and which might be misunderstood as implying that the work carried on at the American Observatory is to a certain extent a duplication of that commenced some time since with the refractor at Melbourne. Mr. Ellery, however, has lately informed Mr. Ormond Stone that his observations are limited to stars south of 35°.

THE BINARY STAR α CENTAURI.—Mr. Maxwell Hall writes from Jamaica, on May 21, with reference to α Centauri: "Since my communication last year respecting this binary, the angle of position of the smaller star has rapidly increased at the rate of 60° per annum. I have lately taken measures in the same manner as before, few in number, but with the greatest care, so that their concordance gives them great weight."

Epoch 1878·38 Position 139°·1 Distance 2″·4

Mr. Hall adds: "There can be no doubt that the smaller star is variable: according to my estimates it has diminished during the last year; and I would therefore call attention to the subject"—and appends various estimates from 1½ (Powell, Jacob) to 4 (Dunlop), also a table of the measured angles and distances to 1878, which it is

unnecessary to give here, as they will be accessible to most readers who interest themselves on the subject of binary stars. For the interval 1864-76, in which Mr. Hall states he had not measures in his possession, the following may be cited:—

Powell	1870·10	Position 20°·45	Distance	10″·24
Russell	1870·75	„ 22·3	„	10·46
—	1872·47	„ 25·5	„	9·74
Ellery	1874·15	„ 30·5	„	8·00
Russell	1874·47	„ 30·0	„	7·97

On the question of the brightness of the components Sir John Herschel says:—"Individually their magnitudes have been very differently estimated by other observers from what I consider to be their correct values. All agree in assigning the first magnitude to the principal star, or that which follows in R.A. (1834-37); but whereas Lacaille, and after him Fallows, Johnson, Taylor, and Messrs. Dunlop and Rumker estimate the preceding star of the fourth magnitude, I have never estimated its magnitude as seen with the equatorial lower than 2·3, and the mean of all the magnitudes assigned to it with this instrument is 1·73, or 1¾ by a mean of eleven observations. . . . . On the whole evidence afforded by my experience I am disposed to assign to it a magnitude which may be deemed indifferently either a very low first or a very high second." Sir John Herschel further considered that "it is not necessary to recur to the hypothesis of variability to account for this difference of estimation," and gave his reasons for this opinion ("Cape Observations," p. 300).

BIOLOGICAL NOTES

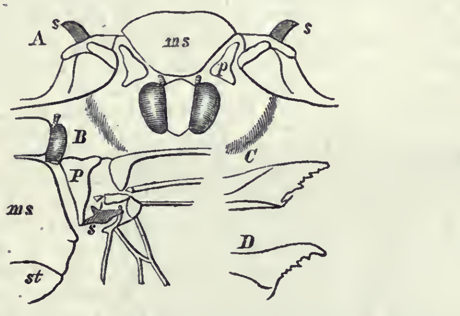
DECORATIVE COLOURING IN FRESHWATER FLEAS.—There is something essentially comic in the notion of a freshwater flea—a species of the entomostracous crustaceous Daphnoidæ—becoming beautifully ornamented with patches of scarlet and blue, for the purpose of seducing the affections of the opposite sex. If a scarlet coat is appreciated by the females of the very fleas of this great family to which we all belong, we ought not to be surprised at hereditary predispositions in favour of this colour, and should conclude on this ground, as on many others, that the civilian male Anthropini of western Europe have taken a foolish and unnatural step, within the last hundred years, in abandoning the use of brilliantly-coloured clothing, and giving over the exceptional advantages which it confers to soldiers and huntsmen. The figures given by Prof. August Weismann, in the *Zeitschr. wiss. Zoologie* (1878, Supplement 1), show us the water-fleas, Polyphemus and Latona, most gorgeously got up in blue and scarlet. Goethe, though he never saw them, foretold their appearance:—

"Es war enimal ein König, der hatt' einen grossen Floh,
Den liebt' er gar nicht wenig, als wie seinen eignen Sohn,
* * * * * *
In Sammet und in Seide, war er nun angethan,
Hatte Bänder auf den Kleide, hatt' auch ein Kreuz daran,"
&c., &c.

It is to the elaborate and ingenious studies of Prof. Weismann on caterpillars—worthy to be placed by the side of the most original of Mr. Darwin's own investigations—that we owe our knowledge of an exceedingly important cause of animal coloration, namely, that which is explained by the term "startling" or "terrifying" colouration (Schreckfarben). Just as in various human races the amorous of both sexes paint their faces and adorn their bodies in order to attract one another, so nature paints by sexual selection, and just as we dress ourselves up in wigs and gowns and spectacles, or tattoo our countenances in order to terrify evil-doers so (Prof. Weismann shows) does nature paint masks with staring eyes upon the feeble caterpillar's back in order that he may enjoy the privileges so usually gained by the ass in the lion's skin. Brilliant patches of colour occur only in a few Daphnoidæ (also in a few Phyllopoda), and after a very detailed investigation as to the variations which these patches of colour present in the different species, in the two sexes, and at different seasons and at different periods of growth, Prof. Weismann comes to the conclusion that they must be regarded as a decoration acquired by sexual selection which probably was first of all confined to the male sex, but subsequently, in most cases, became transmitted also to the other sex. Probably a reciprocal and alternating sexual selection favoured this transference to the female sex, the most brilliant females being chosen by the few males existing at the commencement of a sexual period, and the most brilliant males being chosen by the relatively few females existing at the end of such a period. The existence of these "sexual periods" is a well established feature in the life-history of Entomostraca, alternating with parthenogenetic periods. From the fact that neighbouring colonies of the same species have a constantly differing arrangement of colour, it appears probable that the development of these decorative colour-patches took place after the isolation of the colonies, that is to say subsequently to the glacial period in northern Europe. The transference of the decorative coloration originally developed only by the males, took place in three directions—firstly to the other sex; secondly to the not-yet sexually mature period of growth; and thirdly to the parthenogenetically produced generations. In the various species of Daphnoidæ with decorative coloration we find different degrees of completeness of the transference in these three different directions. Only one species, viz., Latona, presents the highest degree or complete transference of the coloration to both sexes, all stages of growth and all generations of the annual cycle. Prof. Weismann concludes that the Daphnoidæ afford a further case in favour of the hypothesis that secondary sexual characters can be converted into general characteristics of the species, and that they confirm Mr. Darwin's theory of the origin of the colour-patterns of butterflies' wings.

HOW LEPIDOPTERA ESCAPE FROM THEIR COCOONS.—The mode in which butterflies and moths free themselves from their chrysalides has been a subject of some controversy, but of very little recent observation. With regard to the silkworm moth, Malpighi asserted that the animal first wets the silk with a liquid calculated to dissolve the gum that connects the threads, and then employs its lengthened head to push them aside and make an opening. Réaumur, however, maintained that the threads of silk are not merely pushed aside, but are actually severed, and believed that the eyes, which are the only hard organs of the head, are the instruments by which the threads are divided, their numerous minute facets serving the purpose of a fine file. That the threads are actually cut is the general view; and the account of the breeding of silkworms, published in the *American Philosophical Transactions*, states that cocoons, out of which the moth has escaped, cannot be wound. On the other hand it is known to be a common practice with many moths the chrysalis of which is very hard, to discharge, immediately before issuing forth, a copious fluid from the mouth by which the shell is so softened that they are able to force their way through it. In an article in the *American Naturalist* for June, Dr. A. S. Packard, after reviewing our previous knowledge of the subject, gives an account of some interesting observations of his own. His attention being arrested by a rustling, cutting, and tearing sound, issuing from a cocoon of the large green swallow-tail silkworm moth, *Actias luna*, he discovered, on examination, a sharp black point moving to and fro, and then another, until both points had cut a rough irregular slit, through which the shoulders of the moth could be seen vigorously moving from side to side. The hole or slit was made in one or two minutes, and the

moth worked its way at once out of the slit. The wings at this time being very small and flabby, and the shoulders being alternately much raised, the points stuck up far enough to cut or saw through the cocoon. The wings were at first of a deep buff yellow, but in half an hour after they began to expand and to turn green. The black points can be detected when the wings are fully expanded, not being entirely covered by the hairs at the base of the wing. In this case no fluid was seen to exude from the mouth, and the cocoon was perfectly dry. The black points are seen, when magnified, to have the form of a rude saw, and Dr. Packard proposes for them the term *sectores coconis*. The cocoon-cutters were found in every other species of the sub-family *Attaci* that was examined; in *Telea polyphemus* they are large and well-developed; they are rather small in *Callosamia promethea*, *Platysamia cecropia*, *P. Gloverii*, *Samia cynthia*, an *Attacus* from Nicaragua, and *Attacus amazonia*, Pack., from Pebas,



Peru; large and well-marked in the European *Saturnia pavonia-minor* and *Endromis versicolora*. In *Bombyx mori* the spines are not well-marked, and they are quite different from those of the *Attaci*. They are three sharp points, being acute angles of the pieces at the base of the wing. No such spines are present in *Eacles imperialis*.

In the accompanying cut A represents a front view of a specimen of *Actias luna* which came out of the cocoon and died with the wings not expanded; the shoulders are elevated, and the rudimentary wings hanging down; *m s* the scatum, *s* the cocoon-cutter, *p* the patagium. B represents another specimen with fully-developed wings; *m s* the scatum, *s t* the scabellum of the meso-thoracic segment, *s* the cocoon-cutter, which is evidently a modification of one of the pieces at the base of the fore-wings; it is surrounded by membrane, allowing free movement. C and D are modifications of the spine or *sector coconis* considerably magnified, showing the five or six irregular teeth on the cutting edge, the spine being sharp, curved, and conical. It will be seen that it acts like a rude saw.

Fear of Snakes in Primates.—Mr. A. E. Brown has recently made experiments in the Philadelphia Zoological Garden, in pursuance of those of Mr. Darwin. He coiled a *dead* snake in a newspaper, so as to be easily capable of coming loose, and set it on the floor of a cage containing a great variety of monkeys. It was instantly carried off by a leading spirit, but in a few seconds the paper became unfolded and the snake was exposed. The monkey instantly dropped it and went away, but with a constant look behind. The other monkeys, perceiving the snake, approached, step by step, and formed a circle round it six or eight feet in diameter. None approached except one Macaque, who cautiously made some snatches at the paper. At this moment a string which had been attached to the snake's tail was gently pulled; the snake moved, consequently, and the monkeys fled precipitately, with great chattering and screaming. Some time after they gradually returned to their former position, and they continued for some hours showing both intolerable fear and a strange attraction. The same monkeys had no fear of a tortoise or a small dead alligator. The same snake was then shown to mammals of other orders, but none of them showed any especial interest. It is seen that the same dread of snakes is shared by the human species, especially women. Mr. Brown was able to trace, in the actions of a woman who was deaf and dumb, very similar fear, attraction, and repulsion to that shown by the monkeys. Is this a relic of early struggles for existence with an enemy whose bite produced results very different from that of other animals, and exposed mankind to a death lingering and horrible?

The Fertilisation of Eggs of the Lamprey.—We have frequently referred to the great progress of researches into the actual phenomena of fertilisation, especially those of Hertwig. Ernst Calberla, of Freiburg, is another most earnest pursuer of this subject, and he has followed the fertilisation of the lamprey. His views corroborate very strongly those of Hertwig, with some additional particulars. He finds a very distinct external micropyle, with a channel in the yolk leading into the ovinucleus (*Eikern*), which is the residuum of the germinal vesicle. The spermatozoon which is so fortunate as to find the micropyle, enters it and gives rise to the sperm-nucleus (*Spermakern*), which appears twenty-six seconds after the entrance of the spermatozoon into the micropyle. In a minute and a half altogether, the cleavage-nucleus (*Furchungskern*) is seen. After five hours the first cleavage furrow arises, at the spot where the micropyle was situated. In the *Zeitschrift für wissenschaftliche Zoologie*, vol. xxx. part 3, Calberla gives a most interesting account of his procedure and observations, and reviews the work of other investigators, giving a capital bibliography which is of value to those interested in such a rapidly expanding subject.

GEOGRAPHICAL NOTES

Admiral Sir George Back, F.R.S., died on Sunday at the age of eighty-one years. He entered the Royal Navy when twelve years old as a midshipman on board the *Arethusa*, and in 1818 joined a vessel under the command of Sir John Franklin, whom he accompanied on his expedition overland from Hudson's Bay to the Coppermine River, having already taken part under Capt. Buchan in his perilous voyage of discovery made to the neighbourhood of Spitzbergen. In the spring of 1825 Lieut. Back again accompanied Sir John Franklin on his second expedition to the Arctic regions for the purpose of co-operating with Capt. Beechy and Capt. Parry in their simultaneous efforts to ascertain from opposite quarters the existence of a north-west passage. Full details of this voyage will be found in Franklin's "Narrative of a Second Expedition to the Shores of the Polar Sea." Back was again appointed in the spring of 1833 to conduct the expedition fitted out for the purpose of seeking and relieving Sir John Ross, who had gone out nearly four years previously in quest of the north-west passage. A full account of the results of that hazardous enterprise, in the course of which he discovered the river which has since borne his name, Capt. Back gave to the world in his "Narrative of the Arctic Land Expedition to the Mouth of the Great Fish River and along the Shores of the Arctic Ocean in 1833-35." In 1836 Capt. Back sailed in command of another expedition to the frigid zone. The details of this expedition, in the course of which he reached Frozen Strait, almost within sight of Repulse Bay, were published by Capt. Back in his "Narrative of the Expedition in Her Majesty's Ship *Terror*, Undertaken with a view to Geographical Discovery, in 1836-37." In 1857 he obtained flag rank, but had not been afloat since that date. In 1837 Back had awarded to him the gold and silver medals of the Geographical Society. He also was honoured by the gold

medal of the Geographical Society of Paris, of which he was made a corresponding member. He was knighted in 1839, and elected a Fellow of the Royal Society in 1847.

IN the latter part of January and in February last Mr. G. J. Morrison, of Shanghai, made an interesting journey overland from Hankow to Canton. The distance in a straight line is about 525 miles, and he estimates that an ordinary route would be less than 700 miles, though by the route he took it was 860 miles. On the whole, Mr. Morrison does not appear to have experienced any very grave difficulty with the natives during his journey; the people in the southern part of the province of Hupei were very civil, and not very inquisitive; but as he got into Hunan, the population of which is notoriously turbulent, he remarked a great difference. The main portion of his land journey was through a district which had not been visited by a foreigner "within the memory of the oldest inhabitant," and the natives—as is always the case in out-of-the-way parts of China—were most anxious to see the stranger. Mr. Morrison's great trouble appears to have been with his maps, and this was especially the the case where the provinces of Hunan and Kwangtung meet. "The Chinese maps of this district," he says, "are very incorrect, and some foreign maps are worse. The fact that along the north of Kwangtung there is a range of mountains, but that this range does not form the watershed, has been puzzling to geographers. Ichang, which is on the south side of the pass, is still in Hunan, and is situated on the head waters of an affluent of the North River of Kwangtung. This affluent runs in a narrow gorge through the range above referred to." The country through which Mr. Morrison passed on his journey presented many points of interest. Near Wuchang, on the right bank of the Yang-tsze, the land is low and subject to floods, but a short distance to the south it becomes undulating. A little to the west of Puki, on the borders of the great tea-districts, as elsewhere in Hunan, a large quantity of tea-oil is made; the plants from which the seeds are obtained grow about eight or nine feet high, and are more straggling than the tea-shrub. The Siang River, which flows through Hunan, Mr. Morrison found to be in some places nearly a mile broad; but its usual width, when the water is low, is about one-third of a mile. At certain seasons vessels of considerable size are able to ascend as far as Changsha, the capital of the province of Hunan, which is a large and apparently prosperous place. Siangtan, a great trading-place further on, though only a third-class city, is larger than Changsha, and its population is estimated by the Chinese at one million, which, no doubt, is an exaggeration. In the neighbourhood of the borders of Kwangtung the country is bleak and uninteresting. The road over the Che Ling Pass, which is by no means steep, is crowded with traffic, tea-oil, tobacco, &c., going south, and salt and Canton goods going north. The absence of trees is very noticeable both in Hunan and Kwangtung; in the latter the traveller sees the hills for miles denuded of every tree, but in Hunan some attempts are being made at replanting. The part of Mr. Morrison's journey which interested and astonished him most, was the examination of the coal-fields of Hunan and Kwangtung; but it was with very great difficulty that he obtained permission to visit one mine. He noticed that there, as in all Chinese mines, the great want was a good road, which seriously interferes with the output of coal.

AMID all the disasters from flood and drought which have fallen upon China of late, the *North China Herald* says it is pleasant to learn that the great river which has earned the epithet of "China's Sorrow," has not this year justified its name. The Governor-General of the Yellow River reports that the unprecedented cold of the winter caused the upper waters to freeze, and that for more than a month all traffic was suspended, letters having to be forwarded overland by circuitous routes, a necessity which has not arisen for many years, while the pressure of ice in the upper waters caused a rise of one or two feet lower down.

WE have already referred to the fact that relics of the Franklin Expedition have been heard of as in possession of the Nechelli Eskimo away to the west of Hudson Bay. The schooner *Eothen* has left New York under Capt. T. F. Barry—who was in communication with these Eskimo last year—with a party to search for and bring back the relics—among which are said to be written records. The *Eothen* goes to Repulse Bay, whence a party will sledge west about 600 miles to a point near Cape Englefield, where the relics are said to be. The expedition is expected to be away two years and a half.

WE have before us a number of German geographical journals, the nature of whose contents we can only briefly refer to. Indeed the number of these journals in Germany, and the high quality and variety of their contents, are remarkable; they forcibly illustrate the often-repeated saying that geography has become the meeting-place of all the sciences. First, we have advanced sheets of the July number of Petermann's ever-welcome *Mittheilungen*. The first article is by Dr. van Bebber, on the distribution of rain in Germany during the four quarters of the year, and is illustrated by four maps. A remarkably picturesque preliminary, but lengthy, account of his travels in the Caucasus in 1876, is contributed by Dr. Gustav Radde, whose observations on the botany of the region are valuable. Dr. Wojeikoff has an important article on the results of the recent Siberian Surveying Expedition, and Dr. Brehm contributes his usual admirable monthly summary. From the Berlin Geographical Society we have Nos. 3 and 4 of the *Verhandlungen* and Nos. 74 and 75 of the *Zeitschrift*. In the former the principal paper is an account of Thielmann's recent ascent of Cotopaxi, and a long and learned paper by Baron von Richthofen on Prjwalsky's recent journey to Lob-nor. In the *Zeitschrift* (No. 74) is a paper (with map) on the distribution of rain in Europe, by Dr. Otto Krümmel, and a paper of great interest, also with a map, by Dr. Theobald Fischer, on the changes in level of the Mediterranean Coast; the map shows at a glance what parts of the coast are rising and what parts are sinking. No. 75, *à propos* of the recent jubilee of the Society, has a long and interesting account of the progress of geography during the past fifty years, especially in connection with the work done by the Society. This is followed by a paper, with map, on the ethnography of Epirus, by Dr. Kiepert. In the *Mittheilungen* of the Vienna Society the two principal papers also relate to the East; one (a continuation) being on the Turkish Vilayet of the Islands, by A. Ritter von Samo, and the other being an important contribution to Turkish ethnology by Herr Carl Sax, Austrian Consul at Adrianople. The paper and map of the latter show both the race and religion and language of the various divisions of the country, three items which are often confounded.

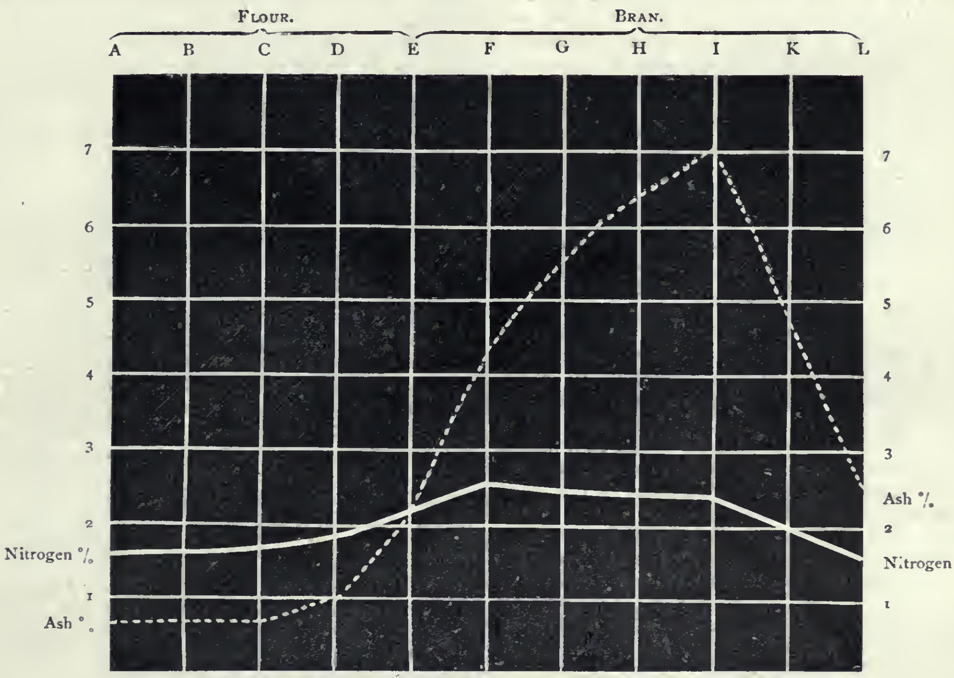
THE Japanese are certainly making great strides in the way of harbour improvement and the extension of means of inland communication, affording thereby a direct contrast to the exclusiveness and obstructiveness of the Chinese. The Doboku-Kioku (Bureau of Construction) now propose to construct a harbour at Samusawa in Miyagi, at a cost of 350,000 yen. It is also said that the Japanese Government desire to raise a home loan of 10,000,000 yen for the purpose of connecting Lake Biwa, in the province of Omi, by a canal with the river Uji, to bring the waste lands in the province of O-u under cultivation, and in order to connect Kioto with the Bay of Tsuga by railway.

REAL BROWN BREAD

NOTWITHSTANDING the labours of chemist and physiologist, the exact composition and nutritive value of the several products obtained in milling wheat have not been thoroughly determined. That fine flour contains less nitrogen, and leaves, when burnt, less ash than biscuit flour, middlings, or any variety of bran, is well known. The percentages of starch, of the mixture of cellulose and lignose known as "fibre," and of fat, in several series of samples of mill-products, have been ascertained. Moreover, there have been made many minute analyses of the ash of wheat and of the preparations derived from it. But we are still somewhat in the dark concerning both the chemical and physiological aspects of what may justly be regarded as the central feature of the problem under discussion. For we are not sure of the nature of the nitrogen compounds which exist in the several distinct parts of the grain of wheat; nor do we know how far the phosphates and such nitrogen compounds as may be ranked with the true albuminoids can be digested when intimately associated with fibre. Then, too, the mechanical condition of these coarser products from the milling of wheat is of considerable moment in estimating their actual value as nutrients.

Before endeavouring to reach some conclusion as to the comparative merits of white bread, brown bread, and whole-meal bread, I will offer in as compact a form as possible the more important and incontrovertible data which must form the starting-point of the discussion.

Firstly as to variations in composition in the grain itself. These variations, chiefly affecting the percentage of nitrogen, depend upon hereditary qualities in different strains of the wheat-plant; upon climate and season; and, to some extent, but not so largely as is often stated, upon cultivation, soil, and manure. The hard translucent wheats, *blés durs et glacés*, of high specific gravity, about 1·41, and, owing to their lengthened and wrinkled shape, of low weight per bushel, these wheats are rich in nitrogen. The soft opaque wheats, of less specific gravity, about 1·38, and, owing to their rounded and plump form, of high weight per bushel, these are poor in nitrogen.



The hard wheats grown in Poland, in Southern Russia, in Italy, and in Auvergne, are used in the manufacture of macaroni, vermicelli, semolina, and pâtes d'Italie. The softer and more starchy wheats are especially appropriate for the production of fine white flour. According to the most recent analyses, the percentage of nitrogen in different varieties and samples of air-dry wheat may range from 1·3 up to 2·5—numbers corresponding to 8·23 and 15·83, respectively, of gluten or flesh-forming substances. But the same variety of wheat may give a grain having 3 per cent. more gluten in a bad season than when matured in a fine summer. More than this, one may select from the same field, the same plant, or even the same ear, individual grains which shall show quite as wide a variation in gluten, as that just cited. For instance, a sample of Hallett's white rough-chaffed wheat of the harvest of 1865 contained many dense and translucent horny grains having 13·2 per cent. of gluten, while the white opaque soft grains from the same sample contained but 9·6 per cent.

It will simplify the consideration of the chemistry of mill-products if we confine our attention mainly to the nitrogen and ash of the grain. The following diagram represents the percentages of these two substances in a series of flours and brands derived from a good sample of English soft wheat. The figures are based in great measure upon the analyses made at Rothamsted by Dr. Gilbert. The mill-products termed A, B, C, are derived mainly from the central portion of the grain, and constitute "fine flour;" D is a biscuit flour known as "tailings;" E is intermediate between flour and bran, and goes under the name of "middlings;" F is "coarse sharps," G "fine pollard," H "coarse pollard," and I "long bran." K, or thin bran, is a product obtained in the process of decorticating wheat by attrition; while L is separated from the grain by moistening and then rubbing it, as in the method devised by Mège Mouriés. These two latter products may legitimately find a place in the series, since they represent the last terms as we proceed towards the outer coats of the grain.

The above table explains itself; we would remark merely that both nitrogen and ash are lowest in the four

flours, and that the former constituent attains its maximum in F, the coarse sharps, and the latter in I, the long bran. In K and L both nitrogen and ash are lower, these products containing much cellulose, made up as they are in great measure of the three coats which form the pericarp of the grain. But it must not be forgotten that all the mill-products included under "bran" contain much more cellulose than is present in flour, namely:—from 7 to 15 per cent., or even more, in lieu of 1 per cent., or less. And it would appear that while flour contains more than 90 per cent. of its nitrogen in the form of true albuminoids or flesh-formers, in some of the brans one-third of their nitrogen is in the form of non-albuminous bodies, of no recognised value as nutrients.

We have now to secure but one more datum before we proceed to the determination of the main question before us. How much flour and how much bran will 100 parts of ordinary soft wheat yield on the ordinary system of low-milling adopted in England? As the averages from an immense number of independent estimates we may put down the flour at a total of 80, the bran at 17, and the loss at 3. Thus, from an economical point of view, we appear to lose $\frac{1}{5}$, or twenty per cent. of our wheat by submitting it to the numerous treatments involved in the manufacture of flour. But is this really the case? We think not. For much of the nitrogen in the rejected parts is not in the form of flesh-forming matter, and much that does so exists in the bran passes unaltered and unused through the alimentary canal, because of its close incorporation with fibre. But on the other side we must not forget that bone-forming materials are clearly deficient in wheaten flour, and that those phosphatic compounds present in bran are readily soluble to a large extent, not only in the several digestive secretions with which they come in contact in the body, but also in pure water.

But in comparing and contrasting bread made from flour with that made from whole wheat we must consider other points. We shall find it impossible to make, by means of leaven or yeast, a light spongy loaf from whole wheat finely ground, the so-called *cerealin* of the bran inducing chemical changes which result in a moist, clammy, dense product. Even whole wheat merely crushed into meal, and not ground, partakes of the same defect. Fine flour, on the other hand, yields a bread which is light enough before mastication, but which, when masticated, possesses a marked tendency to become compacted into dense lumps which may never become penetrated by the gastric and intestinal juices, and which are a frequent cause of constipation. Whole meal bread cannot be charged with this defect; indeed it acts medicinally as a laxative, and by reason of its mechanical texture is hurried rather too quickly along the digestive tract, so that the full virtue of such of its nutrients as are really soluble becomes in part lost. Yet there is no doubt that for many persons, especially those who have passed middle age and are engaged in sedentary occupations, whole wheaten meal in the form of bread, biscuits, scones, &c., forms an invaluable diet.

The following analyses may present some of the foregoing statements in a clearer light and may add some additional particulars of interest. They represent, so far as a couple of sets of average results can do so, the percentage composition of ordinary white bread and of the whole meal bread made by Mesrs. Hill and Son:—

	White.	Whole Meal.
Water	40·0	43·5
[1] Albuminoids or flesh-formers	7·0	[2] 10·5
Starch, dextrin, and sugar	50·7	40·6
Oil and fat	0·6	1·6
Cellulose and lignose	0·5	1·8
[3] Ash or mineral matter	1·2	2·0

1 Calculated from total nitrogen present.
2 As much as 12·5 in some samples.
3 Includes common salt added.

It is clear from the above figures that if we could reckon the whole of the nitrogenous matter in whole meal bread as equally effective with that contained in white bread, we should possess in the former a far more perfectly adjusted food; for the ratio of flesh-formers to heat-givers is about 1 to $7\frac{1}{2}$ in white bread, while it approaches 1 to 4 in some samples, at least, of whole meal bread. Add to this the higher proportion of phosphates in the latter, and its *chemical* superiority over white bread becomes still more marked: its flavour, too, is far richer.

One word as to ordinary brown bread will suffice. It is a poor preparation at the best. By adding a dash of rather rough bran to flour we do not obtain a satisfactory or rich product: analysis demonstrates this fact clearly.

We cannot leave this subject without referring to the little pamphlet which Messrs. Hill and Son have recently issued,[1] on the subject of wheaten meal. Though its main purpose is necessarily a commercial one, it presents many interesting and important facts in a readable form. Messrs. Hill have certainly brought their specialty in bread making some way on the road to perfection. With a few of the opinions in their little *brochure* we cannot, however, wholly concur; nor do we see how their assertion can be maintained that the present system of white bread making involves the loss of 50 to 60 per cent. of the wheat devoted to that purpose.

The limited space at our command must be our excuse for the very imperfect treatment here adopted of the wide subject before us.

A. H. Church

THE LAND OF BOLIVAR AND ITS PRODUCTS[2]

Venezuela, or the Land of Bolivar, as Mr. Spence prefers to call it, has certainly received less attention from European travellers than many other less attractive and more explored parts of South America. The Andean ranges of the north and the llanos of the south of the republic alike merit attention, and now that mining enterprise has opened up several parts of the country and tinged it with European civilisation, we know of no more come-atable country to which the naturalist could turn his steps. Certain it is that he would find ample materials for investigation, and reap a good harvest of novelties in either fauna or flora.

Mr. Spence's main object in visiting Venezuela was, as it appears, the obtaining of a concession of the privilege of working certain deposits of mineral phosphates in the Roques Islands on the northern coast of the republic. During the eighteen months occupied by the delicate negotiations required for this purpose Mr. Spence seems to have lost no time. Although nominally resident at Caracas, in order to be in immediate communication with the ministers, frequent excursions were made to the most interesting of the surrounding districts. The coal mines of Nueva Barcelona, the Lake of Valencia, and the group of islands which were the seat of the wished-for concession, besides other localities of interest, were visited and explored. But the ascents of the Silla of Caracas and the still higher peak of Naiguatá, the crowning point of the Andean range between Caracas and the coast, appears to have been the principal expeditions to which Mr. Spence devoted his attention. The first

1 "The Whole Meal Bread Question." By W. Hill and Son, Bishopsgate Street.
2 "The Land of Bolivar: or, War, Peace, and Adventure in the Republic of Venezuela." By James Mudie Spence, F.R.G.S. 2 vols. 8vo. (London, 1878.)
"Estudios sobre la Flora y Fauna de Venezuela." Por A. Ernst. 4to. (Caracas, 1877.)
"Estudios sobre las deformaciones, enfermedades y enemigos del Arbol de Cafe en Venezuela." Por A. Ernst. (Caracas, 1878.)
"On Venezuelan Birds Collected by Mr. A. Goering." By P. L. Sclater, M.A., F.R.S., and Osbert Salvin, M.A., F.R.S. (*Proceedings* of the Zoological Society of London, 1868–75. Five articles.)

recorded ascent of the Silla was made by Humboldt at the beginning of the present century, since when it has been climbed by several enterprising Venezuelans and by some foreign visitors. Mr. Spence effected the ascent in April, 1872, in company with the German naturalist, Goering, who was at that time collecting in Venezuela, and several private friends. The Silla having been successfully stormed, the summit of Naiguatá, which rises about 800 feet higher, was the next object. From the Silla, Mr. Spence tells us, this high peak "rose boldly to view, and the walled-in appearance of its flanks provoked not only curiosity, but an enthusiastic desire to overcome its traditional difficulty of ascent." Now Naiguatá was reputed to be inaccessible; there was a firm belief in Caracas that its summit "would never be trodden by human foot." There was even an old tradition which "proclaimed its impregnability," and all those who had attempted to scale the height had been compelled to abandon the undertaking without success.

Nothing daunted by the objections of the good people of Caracas, Mr. Spence and his friends set out on their expedition on April 21, 1872, and arrived, after some little difficulty, at the desired summit about midday next day. The Grand Precipice (see our illustration, Fig. 1) would not perhaps appear very formidable to an Alpine-clubbist, but under the tropics people are not so active or so venturesome as in these cold climes, and the retreat was rendered rather severe from the want of water, and the fog which rose up in the evening and obscured the way, as shown in Mr. Spence's drawing (Fig. 2). However, the deed was done, and amongst a small collection of Alpine plants brought from the summit, which has been since described by Dr. Ernst in the *Journal of Botany*,[1] was a new species of bamboo, named, after its discoverer, *Chusquea spencei*, in commemoration of the occasion.

Besides the account of his various expeditions and of his life at Caracas, many miscellaneous subjects regarding

FIG. 1.—The Grand Precipice of Naiguatá.

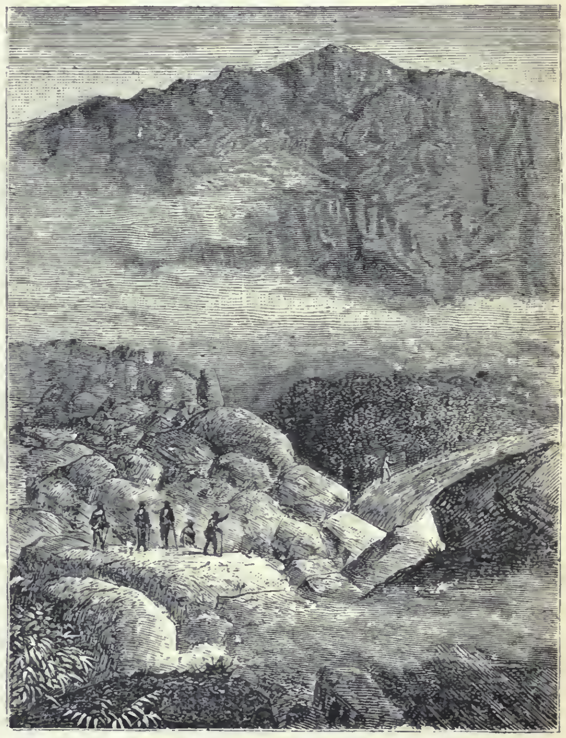


FIG. 2.—The Way lost on Naiguatá.

the "land of Bolivar" are treated of in Mr. Spence's volumes, and the appendix contains other details, amongst which is a synopsis of the orchids hitherto met with within the confines of the republic from the pen of Dr. Ernst. On the whole we may pronounce that Mr. Spence has done well in bringing the merits of a little-known part of the world's surface before the European public. Could Venezuela be persuaded to keep free from intestine dissensions, and to pay her debts a little more regularly, she might still make a figure among the American republics.

Along with Mr. Spence's volume two memoirs of Dr. Ernst, whose name we have already mentioned, lie before us. Dr. Adolf Ernst is, as his name betrays, a German who has deserted the Fatherland for Caracas, and is there labouring to grow science upon a somewhat uncongenial soil. In botany, zoology, and ethnology alike he has worked hard, and is the founder of the "Sociedad de Ciencias Fisicas y Naturales de Carácas," and, we believe we may add, the writer of the greater part of the memoirs of that learned association. His first "Estudios" contains general essays on the flora and fauna, and special catalogues of the ferns, orchids, birds, and land-molluscs of the republic. The second "Estudios" are devoted to a subject of primary importance in Venezuela, that is, to the maladies and enemies (animal and vegetable) of the coffee-plant—one of the staple-products of that part of America. This appears to have been written in answer to an appeal, from the scientific society above-mentioned, for the best essay on this absorbing question, and received the prize to which it was, no doubt, entitled, as having been written by probably the only individual in Venezuela who had more than empirical knowledge of the subject.

Finally we may remark that there is at least one

[1] "Notes on a Small Collection of Alpine Plants from the Summit of Naiguáta, in the Mountains of Caracas." By A. Ernst, Ph.D., &c. (*Jour. Bot.*, September, 1872.)

branch of the fauna of Venezuela that appears to have been pretty thoroughly worked at. Herr Anton Goering—the German naturalist, whose name has been already mentioned in connection with the ascent of the Silla of Caracas—sent all his collections of birds to this country, where they were examined and reported upon by two competent naturalists, who have devoted special attention to the neotropical avifauna. The results are given in the series of papers read before the Zoological Society of London, of which the titles stand last in our list of the subjects of this notice.

Mr. Goering's principal discoveries in the class of birds were made in the Andes of Merida, where some splendid novelties were obtained. And in this part of Venezuela, if we mistake not, there remains most to be done as regards both the fauna and the flora of the republic.

THE FISHERIES OF BRITISH NORTH AMERICA[1]

II.

THE careful inquiries that have been recently carried on by various able investigators in regard to the habits of our chief food-fishes—the Cod, the Herring, and the Mackerel—have now finally disposed of a large accumulation of popular fallacies on the subject of their migrations. On the European side Dr. G. O. Sars has added most to our scientific knowledge of the subject; and on the American, the United States Fisheries' Commissioner, Prof. Spencer Baird, and Mr. Hind, of the Halifax Fishery Commission, whose reports furnish a most valuable body of information as to the New England and Dominion fisheries.

It may now be affirmed with certainty that the notion of the long and distant migrations of these food-fishes is a complete mistake: the real facts being that they never range to any great distance from their ordinary *habitats;* that their migrations, which have reference to food on the one hand and to the deposit of spawn on the other, are simply from deep to coastal waters, and back again; and that these migrations are chiefly dominated by temperature.

Commencing with the *Cod,* we are informed by Mr. Hind that the total average weight caught in North American waters is about 185,000 tons, representing from 150 to 175 *millions* of fish, or between three and four times the produce of the whole Norwegian cod-fishery. Of this, the portion caught in the waters of the United States is only about one-fifth. "Winter cod" are taken on the southern coast of Newfoundland through the whole winter, while "summer cod" are captured through the summer months on the north-east shores of Newfoundland, the entire shore of the Gulf of St. Lawrence, and along the Labrador coast as far north as the Moravian missionary stations, Nain and Okak ($57\frac{1}{2}°$ N. lat.).

It seems now well established that the great body of cod-fish inhabiting the waters of the long North American seaboard is divided into numerous separate "schools," which vary in their habits according to the localities they respectively frequent, each keeping (for the most part, at least) within its own limited range. There is no specific or even varietal difference between the "winter" and "summer" cod; their movements towards the coast from the neighbouring deeps, in which they spend the remainder of the year, being determined by the climatic changes which make the northern shores afford the temperature most congenial to the species in the summer months and the southern in the winter.

The food which lures the cod towards the shore at stated periods varies with the locality and season, being for the most part the capelin in the colder seas and the herring in the warmer; and hence the movements of these fish exert an important influence over those of the cod. At other times the chief food of the cod consists of the Invertebrates of the sea-bottom; and according to the predominance of any particular species will be its share in their maintenance. Thus in some places the cod feeds chiefly (as is shown by examination of the contents of the stomach) upon bivalve or univalve Mollusks; in others upon crabs, shrimps, and yet smaller Crustaceans; in others upon sand-stars, brittle-stars, holothurians, and other Echinoderms. The resort of cod to "banks" seems essentially determined by the food they find there; this, again, being dominated by temperature,—for, as already pointed out, the water on these banks is colder than water at the same depths elsewhere: many sub-arctic species of shell-fish, &c., which serve as food to the cod, thrive there far south of their ordinary habitats (as has been observed by Dr. J. Gwyn Jeffreys on the Dogger Bank); and thus, as Mr. Hind remarks, these banks bear the same relation to the surrounding sea area with regard to certain forms of marine life, as do the oases in the desert to various species of land animals.

An impression has prevailed among fishermen, and even among naturalists, that the Shore cod, or cod generally caught in coastal waters, is specifically different from the Bank cod, which is taken on reefs and banks in comparatively deep water, and often at a considerable distance from land. But it has been conclusively established by the careful observations of the two Profs. Sars (father and son) that no such specific distinction exists, the difference being one partly of age and partly of *habitat.* The two and three-year old cod remain on the Norwegian coast all the year round, and it is usually not until they attain their fourth year that their reproductive organs are sufficiently developed for multiplication. The adult Norwegian cod, according to Sars, retire far from the coast when the spawning season (January to March) is over; and are found during the summer on the slopes of the Polar Deeps. So the cod which frequent the coasts of Labrador through a great part of the year, seem to be immature (though sometimes having their reproductive organs developed); and when they attain their full growth, which occurs in their fourth year, they change their habits, frequenting the outside banks, and only a portion of them visiting the coast during the capelin season.

According to G. O. Sars, the Norwegian cod has no regular spawning ground, but drops its spawn free in the sea at a considerable distance above the bottom. The specific gravity of the ova is slightly below that of sea-water, so that the spawn rises to the surface and floats there, unless the salinity of the surface-layer be lowered either by rain- or by river-water, in which case the ova sink until they reach more saline water. The same is the case with the milt of the male, which seems to be shed at a greater depth than the roe of the female, which is thus impregnated from beneath, the micropyle of the ovum being located at its lowest point. The time required for hatching is about sixteen days, but a further period of fourteen days is required for the absorption of the yolk-bag, up to the completion of which process the young fish has little swimming power.

On the North American coast the spawning of the cod is not confined to a particular season, the process taking place in one locality or another through nearly, if not quite, every month in the year, and being obviously dominated by temperature, for it appears that cod ova find the *coldest* surface-water, provided it be free from ice, the most congenial to their development. Hence, as Mr. Hind justly remarks, the zone of cold water of from twenty-five to thirty miles broad, which extends for hundreds of miles along the Labrador coast, within the line of banks on which icebergs ground, is a most

[1] Continued from p. 172.

valuable possession to us, as supplying the most favourable conditions for the development of the cod ova furnished by the South Labrador schools, and thus feeding the great fishing-grounds further south.

Of late years the salted roe of the cod has become an important article of export, and the preparation of it a considerable industry, the principal use to which it is applied being for bait. Now in so far as this utilisation turns to account what was previously thrown away as offal, it is clearly an advantage; but it now leads to a special search for the gravid fish, which are taken in large quantities in shallow waters by seine nets, and in deep by the "bultow." This practice is very strongly reprobated by the United States Commissioner, who justly remarks that it is "precisely equivalent to killing off all the mature hens in a farm-yard before they have laid their eggs, and then expecting to have the stock continued indefinitely." "As well," he continues, "might the farmer expect to keep up his supply of wheat year by year while he consumed all his grain, reserving none for seed, and without the possibility of obtaining it from any other source." It is obvious from what has been already stated that the fisheries of New England must be much more injured by such a practice than those of the Dominion, the recruiting-ground of the former being far smaller in proportion; and it is also clear that the concession to the United States fishermen of the right to carry on this industry in British American waters is a very valuable one, and that, if made at all it should be placed under conditions which may prevent its being used to the detriment of our own fisheries.

The habits of the *herring* are in many respects different from those of the cod; for while the latter is essentially a bottom-feeding fish, the former is an essentially pelagian fish, feeding and swimming either at the surface or at any depth at which it finds its best supply of food. This consists sometimes of smaller fishes—sometimes the young of its own kind, but generally speaking of more minute animals, especially Entomostraca and Radiolaria, of which small reddish-brown aggregations, known to Norwegian fishermen as *aat*, are often found floating in the waters frequented by the herring. (I have myself met with these in considerable quantity near the Shetlands.)

The old notion of the annual migration of the herring from polar to southern waters has been long since abandoned, in favour of that which recognises in its movements an instinctive direction towards shallower waters at the spawning season. The eggs do not float, like those of the cod; but sinking in virtue of their greater specific gravity, attach themselves by their viscid envelopes either to the bottom or to anything else with which they come in contact. Ropes drawn through herring-spawn, or merely lying where it is deposited, become so thickly coated with it as to resemble large cables; and nets let down upon the spawning-grounds become so thickly covered, that in cleansing them the decks of the fishing-vessels are often ankle-deep in spawn. This spawn is very attractive to cod, which are thus lured towards the shore by the abundance of bottom-food left by the spawning "schools" of herring, as well as by the opportunity of preying on the schools themselves.

The productiveness of the herring fishery of the British North American coasts has been rapidly augmenting of late years, and seems likely to undergo a yet larger increase; for while it has hitherto been prosecuted only when the fish approach the coast at the spawning season, the knowledge now acquired of its habits will guide the fishermen where to look for it at other parts of the year, and how to take it at different depths. The limit set by temperature to the southern range of the herring has been already adverted to; and the admission of United States fishermen to British American fishing grounds is likely to become an even greater boon to them, in allowing them to prosecute a winter herring fishery along the coasts of Nova Scotia and Newfoundland, than it is in enabling them to participate in the cod-fishery. "The fluctuations in North American waters," says Mr. Hind, "are small in extent compared with those astonishing changes which take place in Europe, sometimes causing the ruin of large commercial and fishing communities, and leading to general distress. But the permanency of the herring schools in British American seas, the comparatively small size of the schools, and their uniform extent of distribution over an immense extent of coast-line, give them a direct and individual value to our fisheries, greater than is enjoyed even in Norway."

The spawning of the herring on British American coasts takes place partly in May and June (this being known as the "spring spawning"), and partly in August and September (the "autumn spawning"). The spring and autumn "schools" appear to be quite different; the period being determined in each case by the temperature of the waters frequented by the school. The spring spawning takes place with great regularity on the breaking up of the ice. The young when hatched school together, rarely going out to sea as far as their progenitors, and wintering by themselves apart from the older fish; not being found in any numbers in the deep bays of the coast of Newfoundland, Nova Scotia, and the northern part of Maine, where the old herrings winter. It seems probable that they do not begin to spawn until they have attained their third or fourth year. The depth at which the "spawning grounds" lie varies considerably—on the Norway coast, according to Boeck, from 10 to 150 fathoms. And there is good reason to believe that the occasional abandonment of old spawning grounds is usually due to a change of temperature, and that the fish is to be found at no great distance, probably in deeper waters.

"It is an important result of scientific inquiry," says Mr. Hind, "to ascertain the extent of the movements of a class of animals which have suddenly disappeared from accustomed haunts, and thrown into hopeless confusion an enormous industry upon which hundreds of thousands are dependent for their daily bread. But what immediate relief does it afford, if the discovery establishes the fact that the small downward movement into deeper water, or outward movement into less accessible wintering or spawning grounds, has placed them within reach of fishermen provided with the requisite means of capturing them?" In adapting themselves to such new requirements, he considers that the United States fishermen show more energy than those of New Brunswick; but if the latter allow themselves to be beaten in this winter fishing, in spite of the advantages given by nearer proximity to the fishing-grounds, it is of course their own fault.

The Norwegian herring fishery has of late suffered such a decline, while that of the North American coast has been improving, that out of the million of barrels, to which the catch of the latter is said to amount, no inconsiderable amount is now carried to Sweden in United States vessels. Another source of profit in the capture of the herring is the manufacture of an oil pressed from the bodies of the fish, and the use of the residual "scrap" as manure, under the name of fish-guano. And a vast number of freshly-caught herrings are used as bait in the cod and halibut fisheries; the United States fishermen resorting for this purpose to the Nova Scotia and New Brunswick fishing-grounds, as they find a more profitable market at home for the herrings which they catch off the New England coast. There is reason to fear that, unless due attention is given to the preservation of the spawning grounds, the New Brunswick herring-fishery will decline as that of New England has done; so that the activity of the United States fishermen will not only greatly injure British interests, but will in time come to defeat

their own, if it be not placed under provident restraint. Not only the fishermen of the United States, but those of France also, are supplied with cod-bait from the Newfoundland herring-grounds; and from recent arrangements for storing the bait-fish in ice, the capture of herrings for this purpose is being carried on with increased vigour. "So urgent is the demand for bait, and so entirely dependent are the cod and halibut fisheries upon a sufficient supply, that the fisheries may be said to be altogether dependent upon its being available, either naturally or stored near at hand, in a fresh and suitable condition." "The importance of these facilities for procuring bait only stands out in its true relief, when compared with what would be the condition of affairs if the fishermen of the United States did not enjoy a sufficient supply."

The *Capelin* and *Launce*, also, though of comparatively little value as human food, are of great importance as bait-fishes; the former supplying the cod fisheries of Labrador (where they sometimes abound to such a degree that at the spawning season their shoals are often stranded along the shore), and coming south as far as the Grand Banks; whilst the latter often visit the Banks in such enormous numbers as to give to the sea quite a glittering aspect. The resort of capelin to the Newfoundland fishing-grounds is less regular than that of herring, and it has been found necessary, in order to prevent the destruction of this most important attraction to the cod, to prohibit the use of capelin as manure.

The *Mackerel* is another very important food-fish, which, though an inhabitant of the United States coast much further south than the herring, is especially abundant in northern waters, and has always formed an important component of the produce of the Dominion fisheries, the value of the catch in some seasons exceeding that even of the cod. The supposed migrations of the mackerel from warm southern waters to cooler seas during the summer months, like the mythical wanderings of the herring to polar seas during the winter season, or the equally fanciful migrations of the cod to spawning-grounds on the Norwegian coast, have disappeared before the test of rigid inquiry; the fact being that different schools of mackerel inhabit different parts of the western shore of the Atlantic, from Greenland to Cape Hatteras; wintering in deeper water, and approaching the shore in the spawning season. The time of this approach varies with the temperature of the locality, the fish making their appearance earliest in southern latitudes, and progressively later in the spring and summer in proportion as the latitude is higher and the temperature of the sea lower. The spawn is not deposited on the bottom like that of the herring, but floats on the surface like that of the cod; and the young, when hatched, seems to pass the earlier part of its life in coastal waters. Though the schools of mackerel wander a good deal in the summer months, their wanderings do not appear usually to extend far from their birthplace, and seem mainly to have reference to food-supply, which consists of small fish-fry, entomostraca, and other inhabitants of surface-waters, the relative abundance of which is greatly determined by prevalent winds, while the stratum in which they swim is mainly determined by temperature.

For this and other reasons not yet fully known, the fluctuations in the productiveness of the Mackerel fishery are much greater than those of the Cod and Herring fisheries, especially on the New England coast; and thus the unrestricted admission of United States fishermen to the Dominion waters is a privilege of great value, of which they have largely availed themselves. Mackerel-catching is a special industry, and requires sea-going vessels. The boat-equipment common throughout British-American waters is wholly unsuited to the pursuit of the mackerel, immense schools of which are frequently left unmolested in the Gulf and on the coasts of Newfoundland, in consequence of the fishermen being unprovided with suitable vessels and fishing-gear.[1] Hence the greater part of the mackerel fishery in these waters has hitherto been carried on by United States fishermen; but there is, of course, no reason, save a want of enterprise, why those of the Dominion should not prosecute it with equal success.

From all this it is clear that if the United States fishermen were limited to their own waters, they would speedily exhaust the supplies of the "commercial fish" required not merely for the supply of food to a vast population, but for the supply of bait, fish-oil, and fish-guano—together constituting a drain which far exceeds the natural resources of the limited area along the United States coast inhabited by the cod and other deep-sea fish, as is fully admitted by Prof. Spencer Baird, the United States Commissioner. And thus the free admission of United States fishermen to the fisheries of the Dominion, which are not only unexhausted but apparently inexhaustible (if only placed under reasonable restrictions), is a privilege of enormous value, which should be met on the other side in a spirit of fair reciprocity.

How far this spirit has been exhibited on the part of the Legislature of the United States—which, after agreeing to an arbitration for the settlement of the amount to be paid in compensation, is now raising technical objections to the award, and protesting strongly against its justice,—is not a matter for our consideration; but we cannot conclude without adverting to one point which seems to have received insufficient attention.

While the coastal waters of the United States are in great measure unfitted by temperature for the maintenance of the "commercial" fishes, they are peculiarly adapted for the natural growth and artificial production of different species of shell-fish; some of which are chiefly useful as bait, whilst the Oyster not only supplies the wants of American consumers, but has become a large article of export. The Oyster-industry in the United States now far exceeds in value the aggregate of the deep-sea fisheries; its head-quarters being Chesapeake Bay, "a magnificent basin in which Providence seems to have accumulated every necessary condition for forming an admirable locality for the fishery," so that the oysters inhabiting it do not need culture, but are at once fit for the market. The transport of these oysters to the Northern and Eastern States employs quite a fleet of schooners; and the amount of oyster-shells calcined for lime is almost incredible, the profit derived from the shells at Baltimore alone amounting in 1857 to more than 120,000 dollars.

Now the Treaty of Washington having limited the taking of shell-fish to the citizens of the nationality in which they are found, British American fishermen are completely excluded from the Oyster-industry of the United States, without possessing any corresponding advantage; for the temperature and other conditions of the Dominion coast are just as *un*favourable to the growth of oysters and other esculent shell-fish, as those of the United States coast are favourable; so that, as its produce has no commercial value, "the reciprocity is all on one side."

The different fisheries of the United States coast have been long pursued with the ability and energy which distinguish the American people; but it has been clearly pointed out by the officers employed both by the United States Government and by the several States' Governments, that a decline in the productiveness of the fisheries has of late been going on along the greater part of the coast, and that this decline is due to excessive capture, especially of spawning fish. Through the obstruction and

[1] It is worth notice that the abundance of mackerel on the north-east coast of Newfoundland was for many years so great, that the fish were not only used for manure, but gave such trouble to the fishermen engaged in the cod and herring fishery, that their subsequent diminution was attributed by the fishermen to their having been "cursed off" the coast.

pollution of the New England rivers, the lumberer and manufacturer have ruined the cod-fishery of that locality by destroying the anadromous fishes which attracted the cod thither; so that thus the "fish oil" and "fish guano" manufacturers, who are now enriching themselves, not only at the expense of the herring and menhaden, but of the other species which depend on these for food, will speedily, if unchecked, increase the depletion of the northern waters of the United States; thus increasing the value of the concession made by the Treaty of Washington, and rendering it still more important that laws should not only be made, but enforced, for the prevention of a similar depletion of the (at present) highly productive fishing-grounds of the Dominion.

WILLIAM B. CARPENTER

THE GEOLOGY OF LONDON[1]

ALTHOUGH the British Government have undertaken the geological survey of the country, yet the valuable results obtained by this survey are unfortunately allowed to remain almost unknown to the general public. A complete set of the publications of the geological survey costs, we believe, something like 130*l.*, and is, of course, quite out of the reach of all but great libraries and wealthy public institutions, and no authorised reductions of the maps have as yet been published. It is much to be regretted, too, that the illiberal parsimony displayed in some branches of our public service is most conspicuous of all in that scientific department of it, where its effects prove most injurious. While the publications of the American geological surveys are distributed in foreign countries with an open-handed liberality worthy of a great government, and the courtesy of the chiefs of those surveys, Dr. Hayden and Mr. Clarence King, is well known to everyone—it is notorious that the directors of our own survey are placed in the painful position of having to refuse to acknowledge the just claims of the largest and most important scientific institutions of their own and other countries. The directors of our national surveys are the more to be pitied, inasmuch as the position of grudging parsimony in which they are placed contrasts so strikingly with that course of wise and judicious liberality in making known the results of their labours which the officers of the scientific departments of the United States and some other countries are permitted to pursue.

Another matter calling for serious consideration on the part of those who manage the publication of the results of these national surveys, is the exorbitant prices so often charged for the maps and memoirs. We know not whether it be the result of mismanagement or something worse, but it is a fact that it would seem to cost this Government department three or four times as much to produce a map or memoir as a private firm would require to accomplish the same work. Surely these publications not being handicapped with the charges of authorship, ought to be alike marvels of cheapness and models of excellence, yet how very different is the fact! For an unmounted one-inch map of the district around London the public is charged thirty shillings; for very moderate-sized volumes printed on inferior paper and having the general aspect of mean blue-books put into cloth covers, the sum demanded is two pounds; and recently the geological survey has surpassed even itself by issuing a small paper-covered pamphlet at the price of seventeen shillings!

None suffer so much from the effects of this unwise parsimony and obvious mismanagement as the officers of the survey itself. Those among their number who are engaged in active scientific work see the results of their labours, after long delays and many vexations, placed before the public in an almost inaccessible form; and they are too often disappointed and discouraged by finding that they do not receive the credit which their persevering labours so well deserve. Possibly, as has frequently happened, an amateur observer working independently, and untrammelled by the chains of officialism, is able to forestall their results, by publishing in a scientific journal the most important of their conclusions. Have not the directors of these surveys yet learnt that the day is gone by, when scientific writings can with impunity be delayed for years in the press?

Fortunately the evils to which we have directed attention in the foregoing paragraphs have a tendency to work their own cure. Thus, though the English Government have not followed the wise example of Austria in publishing chromo-lithographed reductions of the larger maps, the director-general and the directors of the branch surveys have produced privately useful maps on a reduced scale of the areas of which they respectively have charge. Objectionable as it may seem in principle that Government officials should issue as private speculations these results of their labours, it is certainly better that they should be allowed so to do, than that the public should be altogether deprived of such important publications.

The map of which the appearance has prompted the foregoing remarks, is another example of private enterprise being allowed to take in hand what we might fairly expect to be accomplished by a national institution. At the Loan Exhibition of Scientific Apparatus, in 1876, a MS. map of the geology of the district around London, drawn on the scale of six inches to the mile, attracted much attention. Since that time this map, with a well-constructed model of the same area, has formed one of the attractions of the admirable museum at Jermyn Street. In this instance the wise course was adopted of publishing a cheap "Guide to the Geology of London," which was drawn up by Mr. Whitaker, one of the most active and efficient officers of the survey, and a geologist whose researches are well known to scientific men beyond its limits. We believe that this excellent little book has had the large circulation it so well deserves; and it is certainly much better calculated to attract the attention of the general public to the important work that is being carried on by the Geological Survey than some of the more ponderous volumes, of which only a few copies are sold at very high prices in each year.

But valuable as the information on this six-inch map clearly was to a large section of the public, its information has been allowed to remain unpublished, and now Mr. Stanford has had to step in to supply the deficiency. Taking advantage of his excellent and well-known library map of London, and securing the services of Mr. James B. Jordan, who has had so much experience in work of this character, he has issued the geological information in question in a very convenient form. The map embraces all the area from Finchley on the north to Beckenham on the south, and from Blackheath on the east to Shepherd's Bush on the west. The subdivisions of the superficial deposits are not so numerous as might possibly have been desired on a map of this large scale, and the work shows too evident traces of having been compiled from a variety of different sources, some of the areas having been carefully surveyed on the six-inch scale, while others are only enlargements of the one-inch map. Nevertheless, with all these drawbacks the map furnishes information not to be obtained from any other published source, and it will supply a want that was beginning to be extensively felt among the ever-growing population of the metropolis.

The colours of the map are exceedingly well chosen and tastefully combined. Until it is superseded by an authoritative Government publication on the same scale, it is sure to have an extensive circulation.

[1] Stanford's Geological Map of London and its Suburbs. The Geology compiled from the Maps and other Works of the Geological Survey of England and Wales by James B. Jordan. Size, 76 inches by 65. Scale, 6 inches to a mile. (London: Edward Stanford, 1878.)

NOTES

THE following is a list of the officers of the Forty-eighth Annual Meeting of the British Association, which will, as we have intimated, commence at Dublin on Wednesday, August 14, 1878. President Elect—William Spottiswoode, LL.D., F.R.S. Vice-presidents Elect—The Right Hon. the Lord Mayor of Dublin, the Provost of Trinity College, Dublin, His Grace the Duke of Abercorn, K.G., the Right Hon. the Earl of Enniskillen, D.C.L., F.R.S., the Right Hon. the Earl of Rosse, D.C.L., F.R.S., the Right Hon. Lord O'Hagan, M.R.I.A., Prof. G. G. Stokes, D.C.L., LL.D., Sec.R.S. General Secretaries—Capt. Douglas Galton, C.B., D.C.L., F.R.S., Philip Lutley Sclater, Ph.D., F.R.S. Assistant General Secretary—G. Griffith, M.A., Harrow. General Treasurer—Prof. A. W. Williamson, Ph.D., F.R.S. Local Secretaries—Prof. R. S. Ball, LL.D., F.R.S., James Goff, John Norwood, LL.D., Prof. G. Sigerson, M.D. Local Treasurer—T. Maxwell Hutton. The following are the presidents of sections:—A.—Mathematical and Physical Science.—President: The Rev. Prof. Salmon, D.D., D.C.L., F.R.S. B.—Chemical Science.—President: Prof. Maxwell Simpson, M.D., F.R.S. C.—Geology.—President: John Evans, D.C.L., F.R.S. D.—Biology.—President: Prof. W. H. Flower, F.R.S. Department of Zoology and Botany: Prof. W. H. Flower, F.R.S. (president), will preside. Department of Anthropology: Prof. Huxley, Sec.R.S. (vice-president), will preside. Department of Anatomy and Physiology: R. McDonnell, M.D., F.R.S. (vice-president), will preside. E.—Geography.—President: Prof. Sir Wyville Thomson, LL.D., F.R.S.L. & E. F.—Economic Science and Statistics.—President: Prof. J. K. Ingram, LL.D. G.—Mechanical Science.—President: Edward Easton, C.E. The first general meeting will be held on Wednesday, August 14, at 8 P.M., when Prof. Allen Thomson, M.D., LL.D., F.R.S.L. & E., will resign the chair, and William Spottiswoode, M.A., LL.D., F.R.S., F.R.A.S., F.R.G.S., president elect, will assume the presidency, and deliver an address. On Thursday evening, August 15, at 8 P.M., a soirée; on Friday evening, August 16, at 8.30 P.M., a discourse by G. J. Romanes, F.L.S., on Animal Intelligence; on Monday evening, August 19, at 8.30 P.M., a discourse by Prof. Dewar, F.R.S., on Dissociation, or Modern Ideas of Chemical Action; on Tuesday evening, August 20, at 8 P.M., a soirée; on Wednesday, August 21, the concluding general meeting will be held at 2.30 P.M. Excursions to places of interest in the neighbourhood of Dublin will be made on Thursday, August 22.

THE following are the presidents of the numerous sections of the French Association which meets at Paris August 22-29:—Sections 1 and 2. Mathematics, Astronomy, Geodesy, and Mechanics, M. Collignon; 3 and 4. Navigation, Civil and Military Engineering, M. L. Reynaud; 5. Physics, Prof. A. Cornu; 6. Chemistry, Prof. Wurtz; 7. Meteorology and Terrestrial Physics, M. Hervé-Mangon; 8. Geology, Comte de Saporta; 9. Botany, Prof. H. Baillon; 10. Zoology and Zootechny, Prof. de Quatrefages; 11. Anthropology, Prof. Bertillon; 12. Medical Sciences, Prof. Teissier; 13. Agriculture, Baron Thenard; 14. Geography, M. Maunoir; 15. Political and Statistical Economy, M. Frédéric Passy.

THE Paris Academy of Sciences has at last succeeded in sending a list of candidates to the Ministry of Public Instruction to fill the place vacated by the death of M. Leverrier. The Academy suggests, by a large majority, the appointment, in the first place, of M. Faye, but M. Faye persists in declining any appointment. In the second place the Academy places the name of M. Loewy, one of the astronomers of the Observatory. M. Loewy being an Austrian by birth, it cannot be said that the Academy has been influenced by any prejudice of nationality. The other candidates presented by the Council of the Observatory are, in the first line, Capt. Mouchez, and in the second MM. Loewy and Tisserand *ex æquo*. It is not yet known what the minister will do. He is at liberty to appoint any other astronomer who has shown himself qualified for the exalted position, as we have announced. M. Mascart has already taken possession of his post at the Observatory as being at the head of the meteorological bureau, but although the principle of separating astronomy and meteorology has been decreed, they are making at the observatory active preparation to fit up new offices for the meteorological bureau. Both services are to be separated, officially and financially, but are to be lodged in the same building as they were during Leverrier's rule. The formal opening of the Meteorological Pavilion at the Exhibition took place on Monday.

THE Anniversary Meeting of the Sanitary Institute will be held at the Royal Institution, Albemarle Street, on Wednesday, July 3, at 4 P.M., when an address will be delivered by Mr. Frank T. Buckland, on "The Pollution of Rivers and its Effects upon the Fisheries and the Water Supply of Towns and Villages." The Annual *Conversazione* of the Members and Friends of the Institute will be held on the same evening at 8 o'clock, at the Grosvenor Gallery, New Bond Street. The Autumn Congress and Exhibition of the Institute will be opened at Stafford on Wednesday, October 2, 1878. The members of the Institute have been invited to the International Congress of Hygiène, under the patronage of the French Government, which will be held at Paris during the first ten days in August, 1878.

WE commend to our readers a movement which has been set on foot for the presentation of a testimonial to Mr. P. Le Neve Foster, the secretary of the Society of Arts, upon the occasion of his completing his twenty-fifth year of service as chief executive officer. When Mr. Foster became its secretary the society numbered only about 1,000 members. At the present time it now numbers about 4,000. During the period of Mr. Foster's administration the Society has successfully dealt with many important public questions, including those of elementary and technical education, patent and copyright law reform, international exhibitions, public health, Indian and colonial and many other topics. Upon these grounds an appeal is made to the members of the Society and the public for their co-operation. An influential committee has been formed, with Lord Hatherley as president.

WE notice the death, in Nürnberg, on June 5, of Baron Ernst von Bibra, in his seventy-second year. Baron von Bibra presented an interesting instance of a cultured nobleman devoting himself entirely to science and letters, and attaining distinction in both branches—a type of character not altogether uncommon in England, but much more rarely encountered in Germany. After the completion of his university studies at Würzburg, he carried out at his castle in Franconia a series of chemical researches which, especially from a physiological point of view, attracted considerable attention. Among these were "Chemical Investigation of Various Purulent Matters" (1842); "Chemical Investigations on the Bones and Teeth of Mankind and the Vertebrates (1844); "Physiological Action of Phosphorus on the Workmen in Match Factories," "Action of Ether" (1847); "Chemistry of the Liver and Gall" (1849); and "Composition of the Blood of the Lower Animals" (1849). In 1850 he undertook an extensive tour through South America. On his return he published analyses of sea-water collected from a variety of points in the Atlantic and Pacific. These were followed in 1853 and 1854, by valuable monographs on the "Composition of the Brain, Spinal Marrow, and Nerves;"

"Action of Narcotics on the Human System;" and "Contributions to the Natural History of Chili;" and in 1858–60, by researches on cereals and coffee. At this period von Bibra turned his attention more especially to *belles-lettres*. The record of his travels in South America was followed by works of fiction, and in a short time he won a prominent place among the German novelists of the day. So fruitful was his pen that no less than fifty-one volumes of novels and tales appeared under his name from 1861–73. Despite this degree of literary activity, the claims of science were not entirely neglected. Papers appeared from him at intervals on various South American minerals, on the chemical composition of various German geological formations, on the properties of aluminium, on a bismuth tin-lead alloy nearly as fusible as Rose's metal, on methods for regaining silver from the solutions of the cyanid, &c. Of more importance were two chemico-archæological monographs "On the Bronze and Copper Alloys of Antiquity" (1869), and "Ancient Iron and Silver Work" (1873). A paper "On the Restoration of Ancient Manuscripts and Paintings" which appeared during the present year, was lately alluded to in these columns. Baron von Bibra was a corresponding member of the Vienna Academy, and several other German academies.

THE State Museum of Sweden has suffered a severe loss through the death of Prof. C. Stål, which occurred on the 14th inst., after a few days' illness; Prof. Stål was only 45 years of age. He was keeper of the Entomological Department of the Museum, to the maintenance of which he devoted an unusual activity and diligence. He is widely known in the scientific world as the author of many important papers on hemiptera and orthoptera, to the systematising of which orders he chiefly contributed. He has been snatched away before his time from other works unfinished and from a large circle of friends who deeply deplore the decease of the amiable and faithful man.

WITH the formation of international exhibitions like that now attracting the world's notice at Paris, there are placed on record, in the form of catalogues, lists of all, or nearly all, the contents of an immense building. These contents are, as it is intended they should be, of a very varied character. The catalogues themselves being the productions of different sections or departments and of widely different nations, consequently we might expect some difference of character in the preparation of these "Guides." Too often a bulky book is produced which is nothing more than a mere list of exhibitors' names and addresses, of no use to the visitor while in the exhibition, and of still less use for reference after. Thus, for instance, opening promiscuously the catalogue of the British Section of the present exhibition, our eye rests on the name of a well-known firm of manufacturing chemists, but all the information we obtain about their exhibits is "Pure Chemicals and Pharmaceutical Products." The Australian colonies have hitherto distinguished themselves in producing full and descriptive catalogues which have been worth a place in the library not only as records of each great show, but as books of reference on the products of the Colonies. We are glad to find that our Indian exhibits are being treated in a somewhat similar way, for we have before us a "Catalogue of the Raw Products of Southern India Collected and Forwarded (under orders of the Government of Madras) to the Paris International Exhibition of 1878." This Catalogue has been prepared by Dr. G. Bidie, the Superintendent of the Government Central Museum at Madras, and comprises substances used as drugs, for food, and in manufactures. Forest products, such as woods, are excluded from this catalogue for the reason that their collection and exhibition has been made a specialty by the Forest Department, a catalogue of which has been drawn up under the title of a "Catalogue of Specimens of Timber, Bamboos, Canes, and other Forest Produce from the Government Forests in the Provinces under the Government of India and the Presidencies of Madras and Bombay." Returning to the first-named catalogue we have an exceedingly well drawn up handbook of 136 pages divided into three great divisions of drugs, food substances, and substances used in manufactures, each being lettered in red on the margin for easy reference. These primary divisions are subdivided into products of the vegetable, animal, and mineral kingdoms, and, in the case of the drugs, again subdivided into such as are officinal in the Pharmacopœia of India, those not officinal, but described in the Pharmacopœia, and those not included in the Pharmacopœia. Again, amongst foods we have agricultural produce, such as cereals, pulses, &c., fruits and seeds, substances used in the preparation of drinks, &c., and so on through each great division. The genera of plants are arranged under each natural order, and, being printed in black letters, are very easily found. After the Latin name follows the English, French, German, and other vernaculars. The plan of the book is, in short, founded on Bridwood's "Economic Products of Bombay," with many improvements. From the catalogue of specimens of timber, bamboos, &c., we find that as many as 650 different specimens of woods have been sent from India to the Exhibition, the total number of specimens of woods and other products of trees amounting to 1,055, which, at the close of the Exhibition, are to be presented, by order of the Indian Government, to the French National School of Forestry at Nancy, "where," as we read, "for ten years past a large proportion of the forest officers of India have received their professional education."

M. DE LESSEPS has inaugurated at the Paris Exhibition a series of lectures, which will be given on Saturdays at two o'clock in the Egyptian House erected by the Suez Company and the Egyptian Government. This house has been built from designs by Mariette-Bey, and professes to represent the mansion of a noble Egyptian at the end of the thirteenth dynasty, before Abraham was born. It consists of a court and a number of rooms. In one of the largest has been placed a model of the Suez Canal and a bird's-eye view of the delta and the Isthmus. M. de Lesseps explained the great work of boring the canal, the actual state of the lands of the Company and the influence of the salaries paid to natives during the execution of the works. A second lecture by M. de Lesseps was delivered in the second hall, where has been hung an immense map of Africa as at present known. Relics of Livingstone, his books, instruments, cap, &c., have been disposed in the room as well as objects connected with the natural history, industry, and trade of the lake region. M. de Lesseps lectured on the necessity of supporting the International Society for the Civilisation of Africa, and on the results accomplished by the Egyptian Government in taking possession of the banks of the Nile from 31° to 1° N. lat.

A CONGRESS of Demography will be held at the Trocadero Palace from July 5 to 9 to discuss the following topics:—Census of population, registers of population, organisation of statistics, registration of births and deaths, publication of periodical demographical results relating to large cities, emigration, &c. A Congress of Anthropological Science well be held in the same place from July 15 to 17. The programme consists of old things adorned with new names, such as ethnodicée, ethnogenie, &c.

LET not those of our mathematical readers who are rather shaky in their French be misled by a letter in Saturday's *Times* from the editor of the *Journal des Géomètres*, inviting English *géomètres* to a conference to be held in Paris on July 8 and 9. The context seems to show that the French word *géomètres* has really its original Greek signification of "land-measurer," and corresponds more nearly to English "surveyor" than anything

else, the exact French term being, we believe, *Arpenteur-géomètre*. As the Paris *Daily News* correspondent showed the other day, even good French scholars may make themselves ludicrous to a Frenchman by translating words literally into their corresponding French forms, such as *physicien* and *chimiste*, which, we need hardly say, mean not physician and pharmaceutical chemist, but physicist and scientific chemist.

A GENERAL meeting of the Mineralogical Society of Great Britain and Ireland will be held at the Meteorological Office, 116, Victoria Street, on July 4, at 8 P.M., when the following papers will be read:—"On a New Manganesian Garnet," by Prof. M. F. Heddle.; "On Cotterite, a New Variety of Quartz," by Prof. Harkness; "On Youngite," by Messrs. David Stewart and J. J. Hood, communicated by Mr. J. B. Hannay; "Notes on Cornish Minerals," by Mr. J. H. Collins.

THERE is every prospect at present of the early commencement of another of the gigantic engineering enterprises characteristic of our century. The last steamer from Panama brings news of the ratification of the contract between the Government of Columbia and the International Committee for the Construction of a Canal across the Isthmus of Darien. Among the conditions we notice the clause declaring the future canal to stand open to the commerce of the entire world, and to be entirely neutral. Another condition is the completion of the work before 1895, but we fear that only pronounced optimists will look forward to the fulfilment of this clause. The Canal Company receives a grant of land including stretches 200 yards wide on each side of the canal, and over 1,000,000 acres in addition, to be chosen at will. It has besides the free use of all building materials on the isthmus, so that no complaint can be made of a lack of readiness on the part of Columbia to further the undertaking.

M. BARDOUX has opened at the Palais du Champ de Mars the Exhibition connected with Public Instruction. The minister said in his address that, owing to the recent progress of France, that country was now inferior to no other European nation as regards popular education. The results of the last conscription are highly satisfactory in this respect. Out of 294,382 men admitted into the ranks of the French army in 1877, only 4,992 were unable to read or write, 2,620 had taken their preliminary degrees in letters or sciences, 234,279 knew the "three R's," 36,325 could only read and write, and 5,856 could only read. Elementary schools have been established in the various regiments of the French army for years, but the attendance, which had been very limited, is now almost universal. Not less than 305,989 soldiers were pupils of regimental schools in 1877; out of these, 255,380 followed the course of elementary instruction, 36,981 the secondary course, and 4,682 the course of superior instruction. The army has been turned into a machine for promoting elementary knowledge. In 1877 not less than 33,337 soldiers learned to read, 24,483 to write, and 111,303 were taught arithmetic. Under guidance of their officers, 200 soldiers from the garrisons of Paris visit the Exhibition daily.

THE Emperor of Germany has named Prof. von Brücke, of Vienna, and the mathematician, C. Hermite, of Paris, as knights of the Order of Merit for Science and Art.

THE well-known physicist, Prof. Clausius, of Bonn, has been elected a member of the Swedish Academy of Sciences.

M. G. A. SIX has lately written a history of the progress of botany in Holland, a work for which this little kingdom has certainly furnished rich material during the past two centuries.

AN interesting fact for agriculturists is communicated by Herr Rudolf Mayerhöffer, of Prague, the editor of the agricultural serial, *Der Bienenvater aus Böhmen*. It appears that a German colonist upon the Island of Java has successfully tried the cultivation of the native bee, *Apis dorsata*, which hitherto has been valued by the natives only for the sake of the larvæ. Herr Mayerhöffer even expresses the hope that it will be possible to acclimatise the Javanese bee in Europe.

ON July 1 Prof. Victor Carus will bring out the first number of a new serial entitled *Zoologischer Anzeiger*, which will form a sort of zoological record in monthly instalments, and, to a certain extent, will be the continuation of Carus and Engelmann's invaluable "Bibliotheca Zoologica." Engelmann of Leipzig is the publisher. The new serial will contain communications regarding museums, institutions, and private collections, notes on zoological and biological subjects, besides a quantity of generally interesting scientific matter.

THE *Japan Times* understands that for the Hong-Kong "afforestation" scheme considerable quantities of seed have lately been forwarded thither at the request of the authorities. As much as will furnish a quarter of a million trees has been sent, the varieties being the *sugi*, *kinoki*, and *tsubaki* (the wild, single-flowered camellia).

PROF. FRÜS, of Christiania, who has been engaged for years in the preparation of a complete dictionary of the Lapp language, has nearly brought his work to a conclusion. This language is richer than most of the northern tongues, the first eleven letters of the alphabet embracing not less than 12,000 words.

THE Harvey Tercentenary Memorial Fund is so far advanced that it has been resolved to take steps to select a sculptor to whom the execution of the memorial statue should be intrusted. Of 1,680*l*. subscribed, 1,228*l*. are in hand.

A MANATEE, caught at the mouth of the Essequibo River, British Guiana, is now on view at the Westminster Aquarium. The poor "whale" has gone the way of its predecessor.

M. A. COSSA has recently communicated to the Academia dei Lincei the results of extensive litho-chemical investigations on the Island Volcano, north of Sicily. He has succeeded in finding here considerable quantities of the sulphates of the rare metals lithium, thallium, cæsium, and rubidium, apparently in the form of alums. The metals appear to have been present in the rocks surrounding the crater, as silicates, and the latter have been decomposed by the acid vapours mounting from the interior of the volcano. Hitherto the mineral pollux scattered over the Island of Elba has been the most abundant source of cæsium and rubidium.

THE additions to the Zoological Society's Gardens during the past week include a Pig-Tailed Monkey (*Macacus nemestrinus*) from Java, a Scarlet Ibis (*Ibis rubra*), a Red-Billed Tree Duck (*Dendrocygna autumnalis*) from South America, presented by Mr. R. M. Hyde; a Green Monkey (*Cercopithecus callitrichus*) from West Africa, presented by Mr. Samuel Curtis; an Indian Gazelle (*Gazella bennetti*) from India, presented by Miss Statter; two Prairie Marmosets (*Cynamys ludovicianus*) from North America, presented by M. J. N. Cornély; three Common Cormorants (*Phalacrocorax carbo*), British Isles, presented respectively by Mr. Edward Banks and Mr. W. Thompson; two Cereopsis Geese (*Cereopsis novæ-hollandiæ*), two Australian Sheldrakes (*Tadorna tæniatus*) from Australia, a Yellow-Billed Sheathbill (*Chiornis alba*) from Antarctic America, purchased; two Mantchurian Crossoptilons (*Crossoptilon mantchuricum*) from China, received in exchange; two Argus Pheasants (*Argus giganteus*), four Summer Ducks (*Aix sponsa*), four Chiloe Widgeons (*Mareca chiloensis*), three Australian Wild Ducks (*Anas superciliosa*), bred in the Gardens.

ON THE PHYSICAL ACTION OF THE MICROPHONE [1]

IN the paper read on May 9 before the Royal Society I gave a general outline of the discoveries I had made, the materials used, and the forms of microphone employed in demonstrating important points. I have made a great number of microphones each for some special purpose, varying in form, mechanical arrangement, and materials. It would require too much time to describe even a few of them, and as I am anxious in this paper to confine myself to general considerations, I will take it for granted that some of the forms of instrument and the results produced are already known.

The problem which the microphone resolves is this—to introduce into an electrical circuit an electrical resistance, which resistance shall vary in exact accord with sonorous vibrations so as to produce an undulatory current of electricity from a constant source, whose wave-length, height, and form shall be an exact representation of the sonorous waves. In the microphone we have an electric conducting material susceptible of being influenced by sonorous vibrations, and thus we have the first step of the problem.

The second step is one of the highest importance: it is essential that the electrical current flowing be thrown into waves of determinate form by the sole action of the sonorous vibrations. I resolved this by the discovery that when an electric conducting matter is in a divided state, either in the form of powder, filings, or surfaces, and is put under a certain slight pressure, far less than that which would produce cohesion and more than would allow it to be separated by sonorous vibrations, the following state of things occurred:—The molecules at these surfaces being in a comparatively free state, although electrically joined, do of themselves so arrange their form, their number in contact, or their pressure (by increased size or orbit of revolution), that the increase and decrease of electrical resistance of the circuit is altered in a very remarkable manner, so much so as to be almost fabulous.

The problem being resolved it is only necessary to observe certain general considerations to produce an endless variety of microphones each having a special range of resistance.

The tramp of a fly or the cry of an insect requires little range, but great sensitiveness, and two surfaces therefore of chosen materials under a very slight pressure, such as the mere weight of a small superposed conductor, suffice; but it would be unsuitable for a man's voice, as the vibrations would be too powerful, and would, in fact, go so far beyond the legitimate range, that interruptions of contact amounting to the well-known "make and break" would be produced.

A man's voice requires four surfaces of pine charcoal, as is described in my paper to the Royal Society, six of willow charcoal, eight of boxwood, and ten of gas carbon. The effects are, however, far superior with the four of pine than with either the ten of gas carbon or any other material as yet used. It should be noted that pine wood is the best resonant material we possess; and it preserves its structure and quality when converted into the peculiar charcoal I have discovered and described.

It is not only necessary to vary the number of surfaces and materials in accordance with the range and power of the vibrations, but these surfaces and materials must be put under more or less pressure in accordance with the force of the sonorous vibrations. Thus, for a man's voice the surfaces must be under a far greater pressure than for the movements of insects; still the range of useful effect is very great, as the boxes which I have specially arranged for a man's voice are still sensitive to the tick of a watch.

In all cases it should be so arranged that a perfect undulatory current is obtained from the sonorous vibrations of a certain range. Thus, when speaking to a microphone transmitter of human speech, a galvanometer should be placed in the circuit, and, while speaking, the needle should not be deflected, as the waves of + and − electricity are equal, and are too rapid to disturb the needle, which can only indicate a general weakening or strengthening of the current. If the pressure on the materials is not sufficient, we shall have a constant succession of interruptions of contact, and the galvonometer-needle will indicate the fact. If the presssure on the materials is gradually increased, the tones will be loud but wanting in distinctness, the galvanometer indicating interruptions; as the pressure is still increased, the tone becomes clearer, and the galvanometer will be stationary when a maximum of loudness and clearness is attained. If the pressure be further increased, the sounds become weaker, though very clear, and, as the pressure is still further augmented, the sounds die out (as if the speaker was talking and walking away at the same time) until a point is arrived at where there is complete silence.

When the microphone is fixed to a resonant board the lower contact should be fixed to this board, so that the sonorous vibrations act directly on it. The upper contact, where the pressure is applied, should be as free as possible from the influence of the vibrations, except those directly transmitted to it by the surfaces underneath; it (the upper surface) should have its inertia supplemented by that of a balanced weight. This inertia I find necessary to keep the contact unbroken by powerful vibrations. No spring can supply the required inertia, but an adjustable spring may be used to ensure that the comparatively heavy lever shall duly press on the contacts.

The superposed surfaces in contact may be screwed down by an insulated screw passing through them all, thus doing away with the lever and spring; but this arrangement is far more difficult to adjust, and the expansion by heat of the screw causes a varying pressure. It is exceedingly simple, however, easily made, and illustrates the theoretical conditions better than the balanced lever I have adopted in practice. In order to study the theoretical considerations, and that with the most simple form of microphone, freed from all surrounding mechanisms, let us take a flat piece of charcoal two millims, thick and one centim. square, and, after making electrical contact by means of a copper wire on the lower surface, glue that to a small resonant board, or, better for the purpose of observation, to a block or cube of wood ten centims. square. Upon this superpose one or more similar blocks, the upper surface in communication with a wire, the lower resting *flat*, or as nearly so as possible, on the lower block.

The required pressure is put on the upper block, and while in this state the two may be fastened together with glue at the sides, or, better, by an insulated screw. The pressure can then be removed, as the screw or glue equally preserves the force. Let the lower piece be called A and the upper B: when we put this block or board under sonorous vibrations, we cannot suppose an undulatory movement of the actual wave-length in such a mass, that is a length comparable with the real wave-length of the sonorous wave which may be several feet. Now we cannot suppose a wave of any length without admitting that the force must be transmitted from molecule to molecule throughout the entire length: thus any portion of a wave, of which this block represents a fraction, must be in molecular activity. The lower portion of the charcoal A, being part of the block itself, has this molecular action throughout, transmitting it also to the upper block. How is it that the molecular action at the surfaces A and B should so vary the conductivity or electrical resistance as to throw it into waves in the exact form of the sonorous vibrations? It cannot be because it throws up the upper portion, making an intermittent current, because the upper portion is fastened to the lower, and the galvanometer does not indicate any interruption of current whatever. It cannot be because the molecules arrange themselves in stratified lines, becoming more or less conductive, as then surfaces would not be required—that is, we should not require discontinuity between the blocks A and B; nor would the upper surface be thrown up if the pressure be removed, as sand is on a vibrating glass. The throwing up of this upper piece B when pressure is removed proves that a blow, pressure, or upheaval of the lower portion takes place—that this takes place there cannot be any doubt, as the surface, considered alone (having no depth), could not bodily quit its mass. In fact, there must have been a movement to a certain depth; and I am inclined to believe, from numerous experiments, that the whole block increases and diminishes in size at all points, in the centre as well as the surface, exactly in accordance with the form of the sonorous wave. Confining our attention, however, to the points A and B, how can this increased molecular size or form produce a change in the electrical waves? This may happen in two ways: *first*, by increased pressure on the upper surface, due to its enlargement; or, *second*, the molecules themselves, finding a certain resistance opposed to their upward movement, spread themselves, making innumerable fresh points of contact. Thus, an undulatory current would appear to be produced by infinite change in the number of fresh contacts. I am inclined to believe

[1] By Prof. Hughes. Communicated to the Physical Society, June 8, 1878.

that both actions occur, but the latter seems to me the true explanation; for if the first was alone true, we should have a far greater effect from metal powder, carbon, or some elastic conductor as metallised silk, than from gold or other hard unoxidisable matter; but as the best results as regards the human voice were obtained from two surfaces of solid gold, I am inclined to view with more favour the idea that an infinite change of fresh contacts brought into play by the molecular pressure affords the true explanation. It has the advantage of being supported by the numerous forms of microphone I have constructed, in all of which I can fully trace this effect.

I have been very much struck by the great mechanical force exerted by this uprising of the molecules under sonorous vibrations. With vibrations from a musical box two feet in length I found that one ounce of lead was not sufficient on a surface of contact one centim. square to maintain constant contact; and it was only by removing the musical box to a distance of several feet that I was enabled to preserve continuity of current with a moderate pressure. I have spoken to forty microphones at once, and they all seemed to respond with equal force. Of course there must be a loss of energy in the conversion of molecular vibrations into electrical waves, but it is so small that I have never been able to measure it with the simple appliances at my disposal. I have examined every portion of my room—wood, stone, metal, in fact all parts, and even a piece of India-rubber—all were in molecular movement whenever I spoke. As yet I have found no such insulator for sound as gutta-percha is for electricity. Caoutchouc seems the best; but I have never been able, by the use of any amount at my disposal, to prevent the microphone reporting all it heard.

The question of insulation has now become one of necessity, as the microphone has opened to us a world of sounds, of the existence of which we were unaware. If we can insulate the instrument so as to direct its powers on any single object, as at present I am able to do on a moving fly, it will be possible to investigate that object undisturbed by the pandemonium of sounds which at present the microphone reveals where we thought complete silence prevailed.

I have recently made the following curious observation:—A microphone on a resonant board is placed in a battery-circuit together with two telephones. When one of these is placed on the resonant board a continuous sound will emanate from the other. The sound is started by the vibration which is imparted to the board when the telephone is placed on it; this impulse, passing through the microphone, sets both telephone-discs in motion; and the instrument on the board, reacting through the microphone, causes a continuous sound to be produced, which is permanent so long as the independent current of electricity is maintained through the microphone. It follows that the question of providing a *relay* for the human voice in telephony is thus solved.

The transmission of sound through the microphone is perfectly duplex, for if two correspondents use microphones as transmitters and telephones as receivers, each can hear the other, but his own speech is inaudible; and if each sing a different note no chord is heard. The experiments on the deaf have proved that they can be made to hear the tick of a watch, but not, as yet, human speech distinctly; and my results in this direction point to the conclusion that we only hear ourselves speak through the bones and not through the ears.

However simple the microphone may appear at first glance, it has taken me many months of unremitting labour and study to bring to its present state through the numerous forms each suitable for a special object. The field of usefulness for it widens every day. Sir Henry Thompson has succeeded in applying it to surgical operations of great delicacy, and by its means splinters, bullets, in fact all foreign matter, can be at once detected. Dr. Richardson and myself have been experimenting in lung- and heart-diseases, and although the application by Sir H. Thompson is more successful, I do not doubt but that we shall ultimately succeed. There is also hope that deafness may be relieved, for telephony articulation has become perfect and the loudness increased. Duplex and multiplex telegraphy will profit by its use, and there is hardly a science where vibrations have any direct or indirect relation which will not be benefited. And I feel happy in being able to present this paper on the results obtained by a purely physical action to such an appropriate and appreciative body as the Physical Society.

In conclusion, allow me to state that throughout the whole of my investigations I have used Prof. Bell's wonderfully sensitive telephone instrument as a receiver, and that it is thanks to the discovery of so admirable an appliance, that I have been enabled to commence and follow up my researches in microphony.

LABORATORY NOTES

DURING the daily routine of life in a laboratory many observations are made of an isolated character, perhaps having no direct bearing on the subject in hand, but which, nevertheless, may be eminently suggestive to other minds. The record of such observations are often lost; they are not communicated unless they find a place in a larger research, and they go to form the capital which every worker is accumulating till his death, much of which, unfortunately, perishes with him. I therefore cordially approve of the suggestion of the Editor that workers in the various departments of experimental science should occasionally write a few notes containing a brief account of any observations recently made, and I shall be glad to contribute my quota.

1. *Carl Zeiss' New Oil Immersion Lens.*—This is a $\frac{1}{8}$th-objective, on the immersion system, in which the fluid used is oil of cedar-wood. For amount of light, clearness of definition, resolving power, and flatness of field, it is superior to any lens I have worked with. For use in histological observation, it does not require any special arrangement of light. In examining such objects as blood-corpuscles or salivary cells with very high powers it is of great advantage to be able to use cover-glasses of ordinary thickness, and to have a serviceably-working distance. This is secured by Zeiss' lens. I have found that, with ordinary Nos. 6 and 7 Hartnack-objectives, more light is obtained by using them as immersion-lenses with a drop of equal parts of oil of cedar-wood and olive oil. The method of using fluids of high refractive index, on the immersion principle, seems to me likely to lead to valuable results. With oblique light, cutting off light from the mirror, the performance of Zeiss' lens is remarkably good.

2. *The Phonograph as a Transmitter.*—By placing Hughes' microphone on the disc of the phonograph the latter will transmit the sounds recorded on the tinfoil to a telephone at a distance. Thus we have a combination of microphone, phonograph, and telephone, which promises to be of use. It is very suggestive to hear the phonograph speaking in one room and to know that some one else in another room, or at a long distance off, is also hearing a repetition of the sound. I have no doubt that arrangements might be made by which the sound might be reproduced in a dozen different places at once.

3. *The Working of the Phonograph.*—After a good deal of experience I have come to the conclusion that a thin and slightly elastic membrane is the most suitable for loudness, whilst a rigid non-elastic membrane is most adapted for distinctness. From a consideration of the histological structure of the drum of the ear this is what one would expect. After the impressions have been made on the tinfoil, distinct speech, in a feeble voice, of most peculiar quality, like what one would imagine to be the tones of the fairies of old, can be heard from one of Marey's tambours, by bringing the point of the lever on the surface of the phonographic cylinder. With this method there is almost no friction, and consequently the marks on the tin-foil are not quickly rubbed out. By connecting a tube with the tambour and carrying it from the tambour to the ear, sounds may be heard, even as speech, after the marks have been so erased from the tinfoil as to be scarcely perceptible to the eye. Thus the tambour, when so used, may be said to be a microphone.

4. *The Microphone.*—I have tried many experiments with the ingenious arrangement of Mr. Hughes, and have been much impressed with its extreme sensitiveness. It may be used to make and break at pleasure the primary coil of the induction machine. When fixed to the box of a monochord the slightest touch of the wire with a camel-hair pencil sounds loudly in the distant telephone. When placed on the sounding-board of a piano, I have heard distinctly a complicated piece of music eighty yards away; when attached to the throat by an india-rubber band, the faintest trill or whisper is audible; and it transmits the muscular sound from a powerful biceps.

5. *A Lecture Experiment.*—Place the heart of a frog on the electrodes of Du Bois-Reymond in connection with a sensitive reflecting galvanometer. The rhythm of the pulsations may then be observed by the swinging to and fro of the spot of light on a transparent screen. This has often been observed by

physiologists, but, considered as a lecture experiment, it is very instructive.

6. *The Sensibility of the Telephone to Feeble Currents.*—As an example of this, I may instance the following experiment:—The gastrocnemius muscle of a frog was placed on the non-polarisable electrodes of Dubois Reymond, so that the transverse section touched the one electrode, and the longitudinal surface the other; the current thus obtained, when sent through a reflecting galvanometer, was sufficient to drive the spot of light from end to end of the scale, placed about three feet in front of the galvanometer; the galvanometer was then disconnected, and a telephone placed in circuit; it was then found that on making and breaking the current, a faint but sharp click of the telephone plate was heard. No click could be heard when the muscle was removed and the two electrodes were connected with a bit of moist blotting paper. The muscle current was therefore sufficient to act on the telephone. The click was stronger when the muscle was placed in contact with two platinum terminals, and when a small carbon microphone was also placed in circuit. I then tried to ascertain whether any effect on the click could be produced by throwing the muscle into a state of tetanus, and I found that in these circumstances no click could be heard at all. In other words, during the state of muscular contraction the muscle current was so diminished (the *negative variation* so called) as to be unable so to affect the telephone as to produce audible sounds. The telephone thus was used instead of the galvanometer in a physiological experiment.

JOHN G. MCKENDRICK

Physiological Laboratory,
University of Glasgow, June 17

VOLCANIC PHENOMENA AND EARTHQUAKES DURING 1877

ALTHOUGH the most important results from the statistics of volcanic phenomena and earthquakes are obtained only if the observations and records spread over a period of many years, yet a number of interesting facts are revealed even in the compilation of the phenomena which occur during a single year. Prof. C. W. C. Fuchs is most indefatigable in these compilations, and he has recently published his statistical account of eruptions and earthquakes for the year 1877. From this we note the following details:—

During 1877 five important eruptions of different volcanoes took place. The eruption of the South American volcano Cotopaxi which lasted from June 25 to 28, was of a most characteristic nature for this mountain. According to the phenomena by which it was accompanied, it must be designated as an eruption of ashes and mud. Although Alexander von Humboldt's view, that the South American volcanoes do not produce lava, has been refuted long ago (Cotopaxi sent forth a copious stream of lava in 1853) yet the most frequent eruptions from this mountain are those of ashes only, without a flow of lava. Streams of mud are often combined with eruptions of this kind, and have different causes; in 1877 they particularly devastated the valleys of Chila and of Tumbaco, and in the former many hundreds of lives were lost through them. The ashes which the volcanos ejected so filled the air that complete darkness reigned everywhere, and the dust was so fine that it entered even into the interior of houses, although the doors and windows were shut.

The most violent eruption of 1877 occurred upon the island of Hawaii. Twice interrupted, the lava forced its way to the surface in three different places, and thus furnished the most undeniable proof that one and the same bed or hearth of lava, may produce eruptions in any of the numerous craters of Hawaii, according to time and circumstances. The first part of the eruption occurred on February 14 from a little side crater close to the summit of Mauna Loa; its duration was six hours, and the height of the column of smoke, which assumed the shape of an Italian pine-tree, was estimated at 5,000 metres. The second part occurred on February 24, in the Bay of Kalukeakua, well known as the place in which Cook, the great discoverer of the Sandwich Islands, was assassinated. This eruption was submarine, and lasted two days; its seat was in the middle of the bay, which is surrounded by numerous prehistoric records of its volcanic nature. On May 4 the lava found its usual way to the surface through the lava lake of Kilauea, which has solidified for some time. Here the wonderful phenomenon of high jets of lava occurred, a phenomenon which is peculiar to this spot only. During a period of six hours, now here, now there, vast jets liquid lava rose from the ground, and their number was so great that at one time more than fifty simultaneous ones were counted, some reaching an altitude of thirty metres.

The third eruption was that of the small Japanese island-volcano, Ooshima, and lasted from January 4 to February 6 or 7. Violent subterranean noise and disastrous earthquakes accompanied the volcanic phenomena, particularly on January 20 and on February 4 and 5.

On June 11 an eruption occurred in a volcanic district almost unknown hitherto, viz., near the Colorado River in Southern California, at some sixty miles' distance from Fort Yuma. The last eruption was a submarine one, and happened on June 15, near the Peruvian coast.

The number of earthquakes during 1877, of which Prof. Fuchs was able to obtain reliable accounts, amounts to 109, and he remarks that this is very nearly the average number per year, if compared to his annual compilations, which now extend over a period of thirteen years. They were distributed over the seasons of the year as follows:—

December, January, February ...	33	earthquakes.
March, April, May	31	,,
June, July, August	11	,,
September, October, November ...	34	,,

On fifteen days several earthquakes occurred simultaneously in different places. Certain districts, such as Peru, Bolivia, Tokiô (Japan), the Island of Ooshima, Hawaii, &c., were visited by real earthquake periods, consisting of a large number of more or less violent shocks and detonations, while in others several earthquakes, separated by long periods of tranquillity, were observed. Among the latter we note—

Judenburg (Styria): January 4, December 27 and 28.
Western Odenwald: January 2 and 10.
Wald (Styria): January 12, September 5.
Rattenberg (Tyrol): April 8, October 11.
Bad Tüffer (Styria): April 4, 7, 24, 25, September 12.
Callao: April 22, May 14, October 9.
Western Switzerland: May 2, October 8, November 30.
Lisbon: November 1 and 4, December 22.

The earthquakes in Switzerland spread over a very considerable area. The first shocks on May 2 began near the Lake of Zürich and proceeded in three directions, viz., as far as Glarus and St. Gallen in the east, Mühlhausen in Alsace in the west, and the Black Forest in the north. They were followed by others more violent, and even more widely spread, on October 8. These were felt most severely at Geneva, where many chimneys were thrown to the ground; but they were distinctly noticed in the whole canton of Geneva, as well as in the Vaud, the Valais, Neufchatel, Berne, Freiburg, and Basel, and also in the French departments of Drôme, Isère, Rhone, Savois, Aix, Jura, Doubs, and even at Mühlhausen in Alsace. The extent of this earthquake towards the west was therefore a far more considerable one than towards the east, where the Alps seem to have hindered its progress; only in the broad Rhone Valley it was felt as far as Sitten. This is all the more remarkable since the Jura Mountains seem to have been without influence regarding its progress in the west. The greatest breadth of the area where the phenomenon was noticed, *i.e.* from Lyons to Sitten, measures some 200 kilometres, while its greatest length, *i.e.* from Valence to Mühlhausen, is 337 kilometres.

Another earthquake of large extension was the one felt on April 4 in the Eastern Alps; it was observed from Lower Styria as far as the junction of the Save with the Danube.

The most violent earthquake of all was the one which occurred on the South American coast on May 9, and in its whole course, as well as with regard to the minor phenomena which accompanied it, it can be compared only to the earthquake which occurred in the same region on August 13, 1868. We gave at the time details concerning this disastrous occurrence.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

At the distribution of prizes at the Yorkshire College last Friday the reports were, on the whole, satisfactory, though the institution has yet much to struggle against. Its great want is want of funds, for, though it has had many generous givers, it takes a great deal of money to start an institution of such magnitude. The college, however, seems extending its influence,

and we think there can be no doubt of its ultimate complete success. The Marquis of Ripon spoke cheerfully both of the present and future of the college, and gave the students some excellent advice as to the aims they should set before them in pursuing their studies.

On the same day a similar ceremony was held at Owens College, Manchester, when much surprise was expressed that the Yorkshire College should oppose the Manchester University scheme. There need be no surprise at this, though we think that, if the two institutions thought of nothing but the educational welfare of the north of England, they would not find it so difficult to see eye to eye.

The Kirgis tribes of Siberia have contributed about 3,000*l*. to the university of Kasan, to serve as a fund for stipends for Kirgis students.

The continued existence of duels in the German universities is a sad blot on modern Teutonic civilisation. Within the past few weeks two deaths from pistol duels have occurred at the universities of Erlangen and Pest.

SCIENTIFIC SERIALS

Annalen der Physik und Chemie, Ergänzung Band viii. Stück 4.—In an inaugural dissertation, with which this number opens, Dr. Less investigates the heat conductivity of some seventeen varieties of stone, and several kinds of wood, his method being a refinement on that of Hopkins, with whose results (for stone) his own generally agree, only the numbers obtained for different varieties of one rock vary much more. In general, density and compactness favour the passage of heat, though the effect evidently does not depend on this alone. Stones of crystalline texture conduct better than those mechanically mixed, and fine-grained better than coarse-grained stones. In his table, marble from the Pyrenees is put at the top, its conductivity being reckoned 1,000; then follow Saxon granite (804), Carraran marble (769), &c., down to common clay (275). Tyndall's observation of a difference in conduction in two directions (with and at right angles to the fibres) in wood is confirmed, but the differences are found considerably less. The ratios of the galvanometer deflections are much greater in the better-conducting than in the worse-conducting woods, making it very probable that these deflections are proportional, not to the conductivities themselves, but to a somewhat higher power of them.—In two papers dealing with magnetic induction and Clausius and Weber's fundamental laws of electro-dynamics, M. Lorberg, by a development of the theory of two experiments, arrives at results throwing doubt on Clausius' law, and endeavours to show that Weber's is the only possible one.—M. Sadebeck contributes a lengthy paper on the crystallisation of markasite, and its regular growths with iron pyrites; and M. Schönn describes the absorption of light by water, petroleum, ammonia, alcohol, and glycerine.

Bulletin de l'Académie Royale de Belgique, No. 3, 1878.—In this number MM. Navez further describe their new system of telephony, by which they claim to speak at distances which are beyond the power of Bell's instruments, with an intensity equal to that of persons speaking face to face. The sender is a modified form of Edison's. A steel bar, supported in a tube, rests vertically on some rundles of retort carbon on the plate, which is copper covered with silver, and to which the sound of the voice passes through a tube of vulcanised caoutchouc attached below; bar and plate are of course in circuit, as also an induction coil. The plate is pinched between hardened caoutchouc and mahogany, which latter supports, on rundles of caoutchouc, a zinc disc with central tube for the steel bar.—Reviewing the geographical distribution of Balænopteræ, M. van Beneden shows that we cannot consider any of the four species of Balænopteræ and the one Megaptera, frequenting the North Atlantic, as proper to Europe. They all, or nearly all, visit the east coasts of North America, as well as the west coasts of Europe, and proceed, both eastwards and westwards, into the Pacific. The North Atlantic species have all representatives in the North Pacific, and *Rachianectes* alone has no representative beyond the Pacific.—Among other zoological papers M. Fraipont furnishes the second and third portions of his researches on the Acetinians of the Ostend coast (three of the forms described are new to science), and M. Longchamps makes additions to the synopsis of the Cordulinæ.—In an interesting memoir reported on by MM. van der Mensbrugghe and Folie, M. Lagrange concludes that a deformable mass, subjected to the attraction of another deformable mass, in rotation takes a motion of rotation in the same direction, which result he proposes to apply to explain the origin and establishment of astronomical movements.

Bulletin of the United States Geological and Geographical Survey of the Territories, vol. iv. No. 2, Washington, May, 1878, contains the following articles:—The geographical distribution of the mammalia considered in relation to the principal ontological regions of the earth and the laws that govern the distribution of animal life, by J. A. Allen.—Descriptions of new extinct vertebrata from the upper tertiary and Dakota formations, by E. D. Cope (describes a large number of new reptile, bird, and mammalian forms).—Notes on a collection of fishes from the Rio Grande at Brownesville, Texas, and a catalogue of the freshwater fishes of North America, by Dr. D. Jordan.—Description of a fossil passerine bird from the insect-bearing shales of Colorado, by J. A. Allen, with a plate.—The coleoptera of the Alpine regions of the Rocky Mountains, by Dr. J. L. Le Conte.—On the orthoptera of Dakota and Montana, by Prof. Cy. Thomas.—On the hemiptera of the same, by P. R. Uhler.—On the lepidoptera of Montana, by W. H. Edwards.—On some insects of unusual interest from the tertiary rocks of Colorado and Wyoming, by S. H. Scudder.

Schriften der physikalisch ökonomischen Gesellschaft zu Königsberg (1876, Nos. 1 and 2, and 1877, No. 1).—These parts, besides a large number of smaller papers and notes, contain the following more important treatises:—On the flora of the great Werder, near Marienberg, by I. Preuschoff.—Report on the recent excavations at Tengen, near Brandenburg (Natangen), by R. Klebs.—On the mechanical principle of equal temperatures in the bodies of the higher animals, by Dr. A. Adamkiewicz.—On some remains of extinct buffalo species from the province of Prussia, by Dr. Jentzsch.—On the decrease in the quantity of water in the rivers of cultivated countries, by Dr. Krosta.—On archæological museums, by O. Tischler.—On some physical relations between the human and animal organism and anorganic nature, by Prof. Grünhagen.—On the latest improvements in the photographic pigment printing process, by Dr. Benecke.—On some antiquities from Claussen, by Dr. Jentzsch.—On the latest discoveries in the diluvial fauna of East Prussia, by the same.—On the strata containing amber in the so-called Samland, by Herr Marcinowski.—On the formation of amber, by Dr. Jentzsch.—On the geognostical investigation of the province of Prussia during the year 1876, by the same.—On the *Macro-lepidoptera* of the province of Prussia, by Rob. Grentzenberg.—On the distribution of rain over the year 1876, by Dr. Schiefferdecker.—On truffles, by Dr. Caspary.—On the great Indian census of 1872, by Dr. Wagner.—Speech in memory of the late Dr. K. E. von Baer, by Prof. Zaddach.—On a naval chart from the fourteenth century, by Dr. Jentzsch.—Full reports of the meetings of the Prussian Botanical Society of Königsberg.—Craniological researches, by Dr. Kupffer.—On a map of the world dating from the year 1452, by Dr. Jentzsch.—On the retina purple, by Dr. von Wittich.—New researches on the habits of ants, Dr. Gwalina.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, May 16.—"On the Variations of the Diurnal Range of the Magnetic Declination as Recorded at the Prague Observatory," by Balfour Stewart, LL.D., F.R.S., Professor of Natural Philosophy at Owens College, Manchester.

The Prague observations began in July, 1839, and have been continued until the present date. They have been dealt with in the same way as those of Kew and Trevandrum. In the first place a set of nine-monthly values of declination range has been obtained corresponding to similar nine-monthly values of spotted solar area. When these are graphically plotted it is found that a number of points in the sun-spot curves may be fairly identified as corresponding to certain points in the declination-range curve, but that the latter invariably lag behind the former in time.

This will be seen from the following table, in which the epochs of maximum and minimum sun-spots are compared with those of declination-range:—

Solar min., June 15, 1843	Prague, dec.-range, min., Feb. 28, 1844.
Solar max., Dec. 15, 1847	Prague, dec.-range, max., March 31, 1848.
Solar min., Sept. 15, 1855	Prague, dec.-range, min., March 31, 1856. Trevandrum, dec.-range, min., Feb. 15, 1856.
Solar max., Nov. 15, 1859	Kew, dec.-range, max., April 15, 1860. Trevandrum, dec.-range, max., May 15, 1860.
Solar min., March 15, 1867	Kew, dec.-range, min., August 15, 1867.

In the next place variations are found in the declination range at Prague which appear to depend on planetary configurations, and which are similar in character to those found at Kew and Trevandrum.

An inspection of the sun-spot records reveals the fact that at times of maximum spot frequency, not only are there most spots on the sun, but that the sun-spot inequalities or oscillations (however produced) are at such times much more prominent than during times of minimum sun-spot frequency. Now, if it be true that these spot periods are due in a great measure, if not entirely, to planetary configurations, we might expect that (possibly from an increase in the susceptibility of the sun) the planetary periods should at times of maximum sun-spots be found to be greater than their average value.

This is found as a matter of fact to be the case, as will be seen from the following table, comparing together observed sun-spot planetary inequalities for periods of maximum sun-spots with the corresponding mean inequalities :—

				Period of Mercury. Observed.	Mean.
Between	0°	and	30°	− 2·31	− 3·95
,,	30	,,	60	− 20·85	− 10·63
,,	60	,,	90	− 33·07	− 12·10
,,	90	,,	120	− 40·37	− 12·33
,,	120	,,	150	− 37·50	− 11·96
,,	150	,,	180	− 21·30	− 10·13
,,	180	,,	210	− 2·04	− 5·83
,,	210	,,	240	+ 18·29	+ 1·54
,,	240	,,	270	+ 37·09	+ 6·99
,,	270	,,	300	+ 47·73	+ 10·00
,,	300	,,	330	+ 43·55	+ 8·91
,,	330	,,	360	+ 22·22	+ 3·63

				Mercury and Jupiter together. Observed.	Mean.
Between	0°	and	30°	− 5·76	− 3·22
,,	30	,,	60	− 18·95	− 7·56
,,	60	,,	90	− 33·26	− 11·72
,,	90	,,	120	− 43·66	− 13·11
,,	120	,,	150	− 37·91	− 10·91
,,	150	,,	180	− 27·22	− 8·71
,,	180	,,	210	− 11·24	− 4·84
,,	210	,,	240	+ 10·78	+ 0·73
,,	240	,,	270	+ 27·76	+ 5·45
,,	270	,,	300	+ 37·35	+ 8·33
,,	300	,,	330	+ 35·33	+ 7·29
,,	330	,,	360	+ 16·62	+ 2·41

If we now turn to declination-ranges we shall find that there are greater oscillations or sub-periods in the value of these ranges during times of maximum than during times of minimum sun-spots. But on the other hand the increased value of such oscillations is by no means so striking as in the case of sun-spots. Mr. Broun has already made the remark that while there is an increase in the whole declination-range during times of maximum sun-spots, yet this increase is not so marked as in the case of the spots themselves, inasmuch as we have a considerable declination-range when there are no spots on the sun. From what has now been said it would seem that a similar remark applies to the oscillations or sub-periods of declination-range, which, while increasing from times of minimum to times of maximum sun-spots, do not yet increase so strikingly as the oscillations or sub-periods of the spots themselves.

If we now treat the inequalities of magnetic declination that appear to depend on the two most available planetary configurations in the manner in which we have just treated sun-spot inequalities, we might expect the observed magnetic inequalities corresponding to times of maximum sun-spots to be greater than the mean inequalities, but not to the same extent as in the case of sun-spots.

That this is the case will be seen from the following table, in which observed declination-range, planetary inequalities for periods of maximum sun-spots are compared with the corresponding mean inequalities :—

				Period of Mercury. Observed.	Mean.
Between	0°	and	30°	+ 11·48	+ 10·42
,,	30	,,	60	+ 3·62	+ 7·25
,,	60	,,	90	− 3·50	− 2·25
,,	90	,,	120	− 6·91	− 3·25
,,	120	,,	150	− 9·13	− 8·16
,,	150	,,	180	− 12·37	− 11·67
,,	180	,,	210	− 13·72	− 12·12
,,	210	,,	240	− 10·44	− 8·68
,,	240	,,	270	− 2·45	− 2·62
,,	270	,,	300	+ 7·73	+ 4·10
,,	300	,,	330	+ 15·14	+ 9·26
,,	330	,,	360	+ 16·20	+ 11·27

				Mercury and Jupiter together. Observed.	Mean.
Between	0	and	30°	+ 11·87	+ 11·61
,,	30	,,	60	+ 2·56	+ 8·07
,,	60	,,	90	− 4·26	+ 2·75
,,	90	,,	120	− 8·72	− 2·45
,,	120	,,	150	− 13·85	− 7·93
,,	150	,,	180	− 16·24	− 11·97
,,	180	,,	210	− 13·44	− 11·80
,,	210	,,	240	− 8·32	− 8·71
,,	240	,,	270	+ 0·51	− 3·11
,,	270	,,	300	+ 11·39	+ 3·44
,,	300	,,	330	+ 16·91	+ 8·74
,,	330	,,	360	+ 17·06	+ 11·89

It thus appears that in the case of the magnetic declination periods there is (as in those of sun-spots) an exaltation of the observed over the mean values during times of maximum sun-spot frequency, but this exaltation is not so marked as in the case of sun-spots. Now, without pretending to know in what way the sun influences the magnetism of the earth, we may imagine that the increased values not only of the average declination-range but also of the sub-periods of these during times of maximum sun-spots may be due to one of two causes, or to both of these together. Thus we may imagine that the sun has an increased magnetic influence during such periods, or we may imagine that there is an increase in the magnetic susceptibility of the earth; or, finally, we may imagine that both of these causes operate together. The author cannot help thinking that we have some evidence of an increase of the magnetic susceptibility of the earth on such occasions derived from two facts discovered by Mr. Broun. The one is that the magnetic influence of the moon on the earth shows traces of following the solar period, this influence being greater during times of maximum than during times of minimum sun-spots. The other is that at Trevandrum the lunar magnetic influence, without changing its type, exhibits an increase of value when the sun is above the horizon at that place, as if on such occasions there were an increase of susceptibility to the lunar influence. These, however, are points which can only be determined by a further discussion of observations.

Geological Society, May 22.—Henry Clifton Sorby, F.R.S., president, in the chair.—John Collins was elected a Fellow of the Society.—The following communications were read :—On the serpentine and associated igneous rocks of the Ayrshire coast, by Prof. T. G. Bonney, M.A.—In a paper published in the *Quarterly Journal of the Geological Society*, vol. xxii. p. 513, Mr. J. Geikie states that the rocks of this district are of sedimentary origin, a felspar-porphyry being the "*maximum* stage of metamorphosis exhibited by the felspathic rocks," and the diorite, hypersthenite, and serpentine being all the result of metamorphism of bedded rocks. This view is also asserted in the catalogue of the rocks collected by the Geological Survey of Scotland. The author had seen specimens of rocks from this district which so closely resembled some from the Lizard, that he visited the Ayrshire coast in the summer of 1877. The author is of opinion that the principal conclusions of the paper referred to above are not warranted by either stratigraphical or lithological evidence. He considers it probable that the "felspar-porphyry," like so much of that in Scotland, is of old red sandstone age, and that the serpentine is of later date,

but palæozoic.—On the metamorphic and overlying rocks in the neighbourhood of Loch Maree, Ross-shire, by Henry Hicks, M.D., F.G.S. The rocks in the neighbourhood of Loch Maree have been described by various authors, but chiefly and most recently in papers communicated to the Geological Society by Prof. Nicol, of Aberdeen, and by Sir R. Murchison and Prof. Geikie, of Edinburgh. In the present communication the author endeavours to show, from results obtained by him recently by a careful examination of a section extending from Loch Maree to Ben Fyn, near Auchnasheen, that the interpretations previously given are in some important points incorrect, and that this has been to a great extent the cause of such very diverse opinions.—On the triassic rocks of Normandy and their environments, by W. A. E. Ussher, Esq., F.G.S.—On foyaite, an elæolitic syenite occurring in Portugal, by C. P. Sheibner, Ph.D., F.G.S. Communicated by Prof. T. M'Kenny Hughes, M.A., F.G.S.

Zoological Society, June 4.—Prof. Flower, F.R.S., vice-president, in the chair.—Mr. Sclater exhibited a young specimen of Temminck's Manis (*Manis temmincki*), and read a note describing habits of this animal in captivity by Mr. F. Holmwood, Assistant Political Agent at Zanzibar.—Mr. Sclater also called attention to the extraordinary mimicry of the true rectrices by the elongated upper tail coverts in *Ciconia maguari* and *C. episcopus*, as observable in the living examples of these birds in the Society's Gardens.—Mr. Edward R. Alston exhibited, on behalf of Dr. Elliott Coues, two specimens of *Synaptomys cooperi*. To this species—the type of *Synaptomys*, proposed in 1867 by Prof. Baird as a sub-genus of *Myodes*—full generic rank was accorded by Dr. Coues in 1874. The present specimens were, so far as was known, the first typical specimens sent to Europe.—Prof. Huxley, F.R.S., read a memoir on the cray-fishes, in which he gave a review of the various generic divisions of this group of podophthalmous crustacea, and pointed out how remarkably these divisions corresponded with their geographical distribution.—Prof. W. H. Flower, F.R.S., exhibited the skull of a two-horned rhinoceros from Tipperah, and read a note on the peculiarities of its structure.—A communication was read from Messrs. Godman, Salvin, and Druce, containing a catalogue of the lepidoptera collected by Mr. S. N. Walter in the Island of Billiton.—Messrs. Godman and Salvin also read a list of the butterflies collected in Eastern New Guinea and some neighbouring islands by Dr. Comrie, during the voyage of H.M.S. *Basilisk*.—Mr. A. G. Butler, F.Z.S., read a paper containing the description of a new species of the orthopterous genus *Phylloptera*, from Madagascar, which he proposed to name *Phylloptera segonoides*.—Messrs. Sclater and Salvin read a report on the collection of birds made during the voyage of H.M.S. *Challenger*. The present communication, forming the eleventh of the series, contained a description of the Steganopodes and of the Impennes. Of the first group the collection contained thirty-three specimens belonging to eight species; of the second, thirty-seven specimens belonging to six species.—Prof. E. Ray Lankester read a paper in which he gave an account of the structure of the hearts of *Ceratodus*, *Protopterus*, and *Chimæra*, with an account of certain undescribed pocket-valves in the conus arteriosus of *Ceratodus* and of *Protopterus*.

Institution of Civil Engineers, May 28.—Mr. John Frederic Bateman, F.R.S., president, in the chair.—The discussion on Mr. T. C. Clarke's paper on the design of iron railway bridges of very large spans, was continued throughout the evening.

ROME

R. Accademia dei Lincei, April 7, 1878.—The following among other papers were read:—Human skeleton in a cavern of the Arena Candida, near Finalmarina, by M. de Sanctis.—Palæontological notes on a large fossil humerus of a bear and other bones of a stag, from a cave near Poggio Mojano, by M. Ponzi.—New researches on Fourier's series, by M. Ascoli.—On carbotialdina and some other sulphurised compounds, by M. Guareschi.—On some derivatives of tetrachlorated ethers, by M. Paterno.—On new derivatives of santonines, by M. Valenti.—On secular variations of the magnetic declination at Rome, by M. Kella.—Map of the planet Mars, by M. Schiaparelli.—On Hofmannite, by M. Bechi.

PARIS

Academy of Sciences, June 10.—M. Fizeau in the chair.—The following among other papers were read:—On the results furnished by chronometers having springs with theoretical terminal curves, at the prize competition of 1877, at Neuchâtel Observatory, by M. Phillips. Of the 220 chronometers sent in 186 had springs with theoretical curves. M. Granjean's occupied the first place for their remarkable accuracy.—On the gemmiparous and fissiparous reproduction of Noctilucæ (*Noctiluca miliaris*, Suriray), by M. Robin. The processes are detailed, and several new facts communicated.—On the conservation of old types of ships, by Admiral Paris. The author's project is to reproduce figures of ancient ships from all parts of the world; he has written to the naval authorities in many countries to send drawings, with explanatory data. Some of his plates are exhibited in the Champ de Mars.—Functions of leaves in the phenomenon of gaseous exchanges between plants and the atmosphere; *rôle* of stomates in the functions of leaves, by M. Merget. His conclusion, from experiments, is thus stated:—In aërial and aquatic-aërial plants, oxygen, nitrogen, and carbonic acid are exchanged normally between the interior and the exterior atmosphere by way of the stomatic orifices. These exchanges may be produced by simple diffusion; they are promoted by all causes capable of producing a rupture of equilibrium between the two atmospheres, and in the double gaseous circulation which follows, the two movements of entrance and exit are performed with equal facility.—Observation of the transit of Mercury at Paita, by Admiral Serres. The conditions were highly favourable, and 600 daguerrotypes were obtained. Each officer made an independent report.—Researches on the sub-nitrate of bismuth, by M. Riche. The mechanism of the action of this substance in the system is controverted. It is important that the druggist should supply for it always the same product, and that the sub-nitrate be exclusively prepared with water slightly calcareous according to the formula of Codex. Every product should be rejected which contains less than 12 to 13 per cent. of nitric acid.—On the physiological *rôle* of hypophosphites, by MM. Paquetin and Soly. They are shown to be not reconstituents but diuretics.—On the colouring matter of wines, by M. Gautier. Each stock produces one or several special colouring matters, and the principles of these substances together form a family of similar but not identical substances of the aromatic series, having the *rôle* of acids, partly combined in the wines under the form of ferrous salt, and apparently resulting from oxidation of the corresponding tannins. He describes the colouring matter of two stocks.

CONTENTS

THURSDAY, JULY 4, 1878

THE EPOCH OF THE MAMMOTH

The Epoch of the Mammoth. By G. James C. Southwell, A.M., LL.D. 8vo. (London: Trübner, 1878.)

BOOKS may be divided into three classes from the point of view offered by criticism, and apart from all considerations of style. There are carefully-written books, the natural fruit of much thought and labour by men who have special knowledge of their subject and who spare no pains to avoid using faulty materials which afterwards may have to be removed, as is generally the case, with much trouble and annoyance. The second class consists of books written without care and very generally the outcome of ignorance or vanity, full of errors, and worse than useless; and lastly, there are some books containing much useful information, but so grouped around views which are utterly wrong that they are worthless for any purpose in which exact knowledge is required. In this class very generally the true is so mingled with the false that it requires the eye of an expert to tell the one from the other. With the first and second of these classes it is easy for a reviewer to deal. It is his duty to welcome the first, not without pointing out (if he can, and we know from experience that very frequently he cannot) the mistakes inseparable from all books, just as he is bound to rebuke sternly the second, and to warn the reader that he is on dangerous ground. It is, however, hard to do justice to the third; for while the information may be useful *per se*, in its position in the book it may be mischievous because it is worked into a wrong hypothesis, thus fulfilling Lord Palmerston's definition of dirt as "matter in the wrong place."

The work before us falls into the third class. Its author seems to have skimmed most of the current literature of the day, more especially reviews, and out of the vast array of facts at his command has picked out those suitable to his views on the recent origin of man. Many of his facts are true, but they are so grouped as to lead the reader to a wrong conclusion. Many of his asserted facts are untrue. The work is a sequel to "The Recent Origin of Man," reviewed in this journal, and is to a large extent an answer to the criticism which it then provoked. We regret that the author has not profited by his experience and that he should have expended so much trouble in attempting to prove a negative which in the nature of things cannot be proved.

The author's aim is to show that man has not appeared on the earth more than six or ten thousand years. He starts from the historical basis offered by the Bible, and in support of chronology ingrafted on the Holy Writ by the unfortunate ingenuity of Archbishop Usher, and in defence of the high civilisation of primeval man, he seizes some of the scraps of history flung out in the struggle between various Babylonian and Egyptian scholars. He adds to these his own views of the discoveries at Hissarlik and Mycæne, and the recent results of exploration in Etruscan tombs and dwellings in Italy, ultimately arriving at the conclusion that man is not older in the Mediterranean area than ten thousand years. To all this the obvious answer suggests itself, that history can tell us nothing as to the antiquity of the human race, because written characters, essential to history, are the result of a high civilisation. How long it took mankind to work out through picture writing a record of the past is an idle question, since we have no data bearing on the point; but we cannot believe that the art of writing was elaborated in a short time. "Fortes vixere ante Agamemnona" whose names we know not.

To attempt to circumscribe the antiquity of man within the limits of history appears to us as idle and barren an attempt as could possibly be undertaken. It would be as reasonable to seek figs growing on thistles as to look for any proof of the recent origin of man in the written record. These facts are so obvious in the present condition of knowledge, that we should not bring them before our readers were they not utterly ignored by the author of this work, as well as by some of his critics.

Our author having established to his own satisfaction the recent origin of man in the Mediterranean countries, enters into the question of the unity of the human race. The pre-Christian cross, either in the form of the handle cross of the Mediterranean districts or the Swastika of the Buddhists, was widely spread among ancient peoples. The tradition of a deluge is almost universal. That of a terrestrial paradise is widely spread: we read of the gardens of Alcinous and Laertes, of the Asgard of the Scandinavians, and of sundry other gardens mentioned in various writers Indian, Chinese, and Arabian. Then we have Megalithic monuments scattered over widely-separated countries, and the habit of distorting the human skull, and of scalping. The range, also, of the boomerang, pointed out by Gen. Lane Fox, the custom of depositing flint implements in graves, and of worshipping phalli and serpents, are taken to "prove the unity of the race, almost without any other argument on the subject."

Then the author proceeds to his application, "If the human race is one, the Egyptian, the Hindoo, the Babylonian, and the palæolithic tribes of the Somme Valley were one; and if Kephren and Cheops were near of kin to the fossil man of Mentone or the savage who owned the Neanderthal skull, and if, moreover, the antiquity of man in Babylonia does not go farther back than some ten thousand years, *then* the men of the French and English river gravels cannot be more than ten thousand years old. The reverse would only be possible on the hypothesis that the Egyptians were the descendants of the men of the Somme Valley. But this is excluded by the fact that the Egyptians appear at once as a civilised race; and, as we have stated, there are no earlier remains of any kind in Egypt" (p. 21). We give this as an example of the style of reasoning. So far as we know, nobody, not even the author, has ventured to assert that the two Egyptian kings above mentioned "were near of kin" to the so-called fossil man of Mentone, or stood in any near relationship to any of the ancient inhabitants of Europe. The argument is to us wholly unintelligible. Why should the Egyptians be descended from the men of the Somme Valley any

more than the latter from the Egyptians? The civilisation of Egypt throws about as much light upon the barbarism of the palæolithic age as that does upon the civilisation of Egypt.

The author has taken great pains to break down the archæological classification by the trite argument that bronze, iron, and stone have been very frequently found together in various parts of Europe. We suppose that no modern archæologist has disputed the fact. Dr. John Evans holds that they shade off into one another like the prismatic colours of a rainbow, Dr. Keller and Mr. Lee, his able translator, give numerous examples from the pile dwellings of Switzerland, and other places, of the association of implements composed of these materials. This association, however, has nothing to do with the question as to whether the archæological classification is correct. The conclusion of the Scandinavian and Swiss archæologists, that the use of stone, bronze, and iron characterises three distinct phases in the civilisation of mankind in Europe, has been amply confirmed by the numerous discoveries made during the last five-and-twenty years. They are merely the outward marks of new stages of culture.

Nor has the subdivision of the stone age into palæolithic and neolithic, by Sir John Lubbock, been shaken; they are separated from one another by the greatest changes in climate and geography, and in animal life, which have taken place since the arrival of man in Europe. Our author, however, denies this, and brings forward a series of examples derived, for the most part, from accounts either unverified by subsequent observers or in themselves equivocal, to show that the palæolithic men possessed domestic horses, oxen, pigs, dogs, and "hens," and were acquainted with the art of making pottery. We have no space to examine each of these statements in detail. We would merely say that the scientific exploration of caverns and tombs is by no means easy, and that until comparatively recently everything of unknown date found in them was supposed to belong to about the same age. Hence it is that the literature of archæology offers to the author the examples which he gives us.

With regard to pottery it must be remarked that the vessels assigned to a palæolithic age, such as that of the Trou de Frontal, belong to well-known neolithic types, and that the domestic animals assigned to the same age are identical with those of the neolithic farmers and herdsmen. Caves were used by the neolithic peoples for purposes of habitation and burial. The duty, therefore, of proving that these things are of palæolithic age rests with the author;—it is not the business of a reviewer to undertake proof of a negative that they are not. The assertion, however, that no neolithic implements have been met with in the same cave as the so-called "fossil man of Mentone," whom we have always believed to belong to that age, is negatived by the polished celt from that cave which we have seen in the museum at St. Germains—an important fact, which, strangely enough, has escaped the notice of all who have hitherto written on the subject.

We shall not repeat the arguments in favour of the palæolithic age of the interments at Solutré, which have already been combated in this review. We have always held that they are not earlier than Gallo-Roman times. The results of the further researches of MM. Ducrost and Arcelin, in 1875-6, show that, above the strata containing the remains of mammoth, reindeer, horses, and palæolithic implements, there is a stratum containing polished stone axes, iron and bronze implements, and interments of the neolithic, Gallo-Roman Burgundian times. The so-called palæolithic are in all probability referable to one of these three ages, and from the fact of the skeletons resting at full length to one or other of the two last periods.

The author is not content with bridging over the interval between the neolithic and palæolithic times by the asserted occurrence in the latter of characteristics hitherto to be considered peculiar to the former. He tells us that extinct pleistocene animals lived "some of them down to historic and even post-Roman times." In support of this view he brings forward the occurrence of the mammoth from the peat bogs of Holyhead, Torquay, and Colchester, just as if there were no peat bogs in the pleistocene times—as, for example, the pre-glacial forest-bed, with mammoth and other creatures, on the shores of Norfolk and Suffolk. He relies also upon the fresh condition of the carcases of the Siberian mammoth as evidence against high antiquity, just as if ice would not preserve anything imbedded in it for an indefinite length of time.

Palæontologists will be astonished to hear that the cave-bear has been met with in the peat bogs of Denmark, and in Italy in association with relics of the neolithic age. The first of these reputed occurrences has been given up by M. Nilsson, and the second has not been verified by any competent authority. The latter observation will also hold good regarding the reputed occurrence of the cave lion in the peat of Holderness. The Irish elk is asserted by our author to have been living in the marshes of Europe as late as the fourteenth century, a statement based on a speculation of Brandt's that the Machlis of Pliny and the Schelch of the *Niebelungen Lied* are identical with that animal. The palæolithic implements themselves (p. 220) are traced to the stone axe from Babylon, preserved in the British Museum, of a "palæolithic type which reappeared in Europe when some of the ruder Turanian tribes migrated in that direction."

It is not profitable to pursue this review further, for in this work one printed statement is treated as if it were of equal value with another, without any attempt being made to sift the improbable from the probable, or the true from the false. The facts are brought together in it very much like flies—if one may indulge in a comparison—on a fly-paper, and bear the same relation to each other as the heterogeneous collection of dead and dying winged creatures there brought together in a strange fellowship. We regret that the writer should have spent so much time as he evidently has spent in collecting matter for a book written without scientific method, and which certainly does not prove that the age of the mammoth is removed from the present time by an interval of from six to ten thousand years.

W. B. D.

RECENT MATHEMATICAL WORKS

An Elementary Treatise on the Dynamics of a System of Rigid Bodies, with Numerous Examples. By E. J. Routh, M.A., F.R.S., F.R.A.S., F.G.S. Third Edition, Revised and Enlarged. 8vo, pp. 564, xii. (London: Macmillan and Co., 1877.)

A Treatise on Statics, containing some of the Fundamental Propositions in Electro-Statics. By G. M. Minchin, M.A. 8vo, pp. 450, xii. (Longmans, 1877.)

Lectures on the Elements of Applied Mechanics, comprising (1) *Stability of Structures;* (2) *Strength of Materials.* By M. W. Crofton, F.R.S. Printed for the Use of the Royal Military Academy. Pp. 107. (C. F. Hodgson and Son, 1877.)

Handbook of Natural Philosophy—Mechanics. By D. Lardner, D.C.L. New Edition, Edited and considerably Enlarged, by B. Loewy, F.R.A.S. Pp. 489, xxii. (Crosby Lockwood and Co., 1877.)

The Book of Mechanics. Part I.—Statics. By R. Oscar Thorpe, M.A. (Stewart's Local Examination Series, 1877.)

THE main features of Mr. Routh's admirable treatise are well known to students. The first edition, of 336 pages, appeared in 1860; the second, of 492 pages, in 1868; the present consists of 564 pages, each page containing from a third to a half as much matter more than the page of the earlier editions. Some idea may thus be formed of the great amount of new matter. Of this increase take another proof: in the second edition the chapter on *Small Oscillations* took up pp. 273 to 322; in this edition the subject occupies pp. 325 to 403! The author assigns as a reason for this increase, "I have been led" to make these additions "because there are so many important applications which it did not seem proper to pass over without some notice."

An interesting feature is the increased number of historical notices, though these are confessedly very slight, drawn from Montucla (by a misprint Montuela), Prof. Cayley's Report on Theoretical Dynamics (British Association Report, vol. xxvi.), and other sources. Some of these are relegated to an appendix. A great number of original memoirs have been consulted and some of these of very recent date. We do not notice in Articles 282, 475, in which a discussion of the problem of Laplace's three particles is given, any reference to the author's paper on the subject in the *Proceedings* of the London Mathematical Society (vol. vi. No. 81, pp. 86–97), though of course the substance of this paper is given in the text. We note this, because in both places Mr. Routh cites a reference, by M. Jullien, to a Thèse de Mécanique, by M. Gascheau, which he has not succeeded in verifying. Perhaps a notice of this point in NATURE may lead to the matter being cleared up for Mr. Routh's satisfaction. We have not ourselves met with this pamphlet by M. Gascheau. We could dwell much longer on this fine work, pointing out the numerous places where new proofs are given and entirely new matter is introduced, but we need only say that it must claim a very high place in our mathematical literature, and go far to remove the reproach brought against Cambridge text-books by students who have become familiar with the works of continental mathematicians. There is an ample and diversified collection of problems which are given in the several chapters and appended to them. Following a common practice, the author gives a list of articles to which beginners should first turn their attention.

Prof. Minchin purposely omits the prefix "Elementary," his main object being to give "a tolerably comprehensive view of statics." Very early in his book he introduces the conception of "virtual work," a term he adopts from the best French writers (Collignon, Delaunay, and others) in preference to "virtual velocities." His reason for bringing the subject so soon before the student is "the conception of work is the most prominent in modern physics, and, therefore, at the risk of being charged with prolixity, I have shown in the earliest chapters how all the conditions of equilibrium of a system may be obtained from the principle of virtual work independently of the usual mode of the reduction of forces." Graphic methods are used in the earlier portions; a good feature, now that the treatises of Culmann, Bauschinger, and Cremona are in the hands of many English students. The subjects treated of are much the same as in other treatises in our hands, and the last chapter (pp. 403 to 450) is devoted to the theory of the potential; the modes of treatment, however, are different.

Prof. Minchin attaches great importance to the solution of problems, and so takes care to solve a great many leading cases, and has done good service to students by these solutions and by the figures which he gives. The following remarks speak for themselves:—"It is characteristic of the system of 'cramming,' which has been called into existence by modern competitive examinations, that the *applications* of mathematics, as exhibited in the solution of examples, are greatly neglected. A cause contributing to this objectionable system appears to me to exist in our mathematical treatises, many of which are almost wholly filled with unsolved problems and dry 'book-work,' which the student never learns to apply. I have therefore very largely illustrated the principles of the subject by solved examples, and I have attached at least as much importance to examples, all through, as to the abstract principles which they illustrate."

We cordially commend the book, and hold that it is no unworthy companion of such text-books as those of Dr. Salmon and of Mr. Williamson.

Prof. Crofton's book is a "Synopsis of a Course of Lectures on the Elements of the Theory of Structures and the Strength of Materials, forming the First Part of the Course of Applied Mechanics at present studied by the Gentlemen Cadets of the Royal Military Academy." The book requires to be read with some care, as the author's idea is that it should be supplemented by *viva voce* instruction and by experimental illustrations. It is thoroughly elementary, however, and avoids all aid from the differential and integral calculus. Great importance has been deservedly attached to the elegant method of diagrams of forces and to Culmann's graphical method. In the first part are considered such matters as frames, roofs, trussed beams, chains, and cords, and the stability of walls. In the second part come under notice resistance to stretching and to compression, theory of beams, moment of resistance in rectangular beams, girders, open girders, partially loaded beams, and other thoroughly practical matters. Prof. Crofton has wisely given a great

number of figures, and in addition to numerous unsolved exercises, has given very many worked-out problems. In his introduction he points out that "the practical man, unlike the theoretical, cannot choose his problems; he must take those which the requirements of his art present, whether elegant and curious, or cumbrous and repulsive. Moreover, in his case, *some* solution of every problem must be obtained; if he is unable to find a rigorous scientific solution he must make some further assumptions or have recourse to experiment; he cannot lay the question aside." He goes on further to point out the differences between the two studies of theoretical and applied mechanics. The author has brought the subject before the notice of mathematicians in communications to the Mathematical Society and the *Educational Times*.

Mr. Loewy has retained much of the elementary part of the late Dr. Lardner's treatise, having carefully revised it and brought it up to modern requirements. He has re-written, for the most part, the descriptive chapters on machines, clockwork, &c. Many new illustrations and a great number of solved exercises have been added, so that now the work is embellished with nearly 400 illustrations. An account is given of the modern units of force and work (the dyne, poundol, &c.). The result is a neat and readable book on properties of matter, theory of machinery, and illustrations of the application of mechanical principles in the industrial arts. We do not pretend to have read the work for it is full of matter, but what we have examined we have found interesting and carefully done. We have detected a few slips (typographical, chiefly) in the solutions. A good feature is an index.

The last book on our list is neatly got out and is doubtless adapted for the end in view, the author having written it for candidates for the Oxford and Cambridge Local Examinations. It is such a book as might have been compiled at any time within the last twenty-five years from the Cambridge text-books, for it keeps quite to the old Cambridge "lines;" it "aims at being simple, but not childishly so." The modern treatment of the subject has been altogether avoided. This is, perhaps, no fault of the author, but rather the exigencies of the above-named examinations have compelled him to move in this rut. There is a sufficient number of exercises taken from the examination papers, and a chapter is devoted to hints for, and examples of, the selection of problems. The figures generally are clearly drawn, but a cylinder on p. 43 is a sorry representation of such a solid.

OUR BOOK SHELF

Mikrographie der Glasbasalte von Hawaii: Petrographische Untersuchung. Von C. Fr. W. Krukenberg. (Tübingen, 1877.)

THE interesting facts made known of late years by Prof. Möhl, of Cassel, and Dr. Bořický, of Prague, as the result of their study of the microscopic characters of the vitreous and semi-vitreous rocks of basic composition, have rendered it eminently desirable that a thorough investigation of the remarkable lavas of the Sandwich Islands should be undertaken by some competent observer. We therefore hail the appearance of the monograph now before us as supplying a want which has been felt for some time past by all who are interested in micro-petrographic studies.

From the older analyses of the Sandwich Island lavas as tabulated by Herr Krukenberg, we learn that the composition of these rocks varies within very wide limits—the proportion of silica ranging from 39·74 to 59·80; the author's own analyses, however, would seem to indicate much less widely separated rocks as having been subjected to examination by him, for the proportions of silica are given as from 50·865 to 53·61. The most remarkable circumstance about the composition of these Hawaiian lavas is probably the large proportion of iron-oxide which they contain, the percentage of this substance ranging from 13 to 33 per cent., while alumina is only present in small quantity, or is sometimes altogether absent.

Herr Krukenberg first describes the curious structure revealed by the microscope in the compact basaltic glass in which are detected numerous beautiful examples of those skeleton crystals built up of crystallites to which Vogelsang first directed the attention of geologists, and to which the name of "chiasmoliths" has been applied. Among the perfectly-formed crystals porphyritically embedded in this compact or glassy mass, the author noticed felspar (both orthoclastic and plagioclastic) and olivine, but he failed to detect augite.

The curious forms assumed by the threads of Pele's hair are admirably described in the work before us, and are illustrated by numerous figures. Gas bubbles appear to be very common in these glass threads, and they are often drawn out into elongated cavities or fine capillary tubes. Minute crystals are sometimes seen in the midst of the glass threads, which sometimes exhibit a concentric structure and at others a series of transverse striations. In the ordinary porous glass lava the author finds structures intermediate between the chiasmoliths and the crystalline plates seen in Pele's hair; his drawings, indeed, very admirably illustrate the mode of development of crystals in glassy magmas. The last variety of the Sandwich Island lavas described in this monograph is the sphærulitic; but the sphærulites of the basaltic rocks do not appear to differ in any essential point of structure from those so well known as occurring in acid vitreous rocks.

In an appendix to the paper the author notices the existence in the Sandwich Islands of a true obsidian which yielded 76·10 per cent. of silica. The monograph is illustrated with four lithographic plates, and is a very valuable contribution to petrographic science. J. W. J.

Preventive Medicine in Relation to the Public Health. By A. Carpenter, M.D., C.S.S., Camb. (London: Simpkin, Marshall, and Co.)

UNDER the title of "Preventive Medicine" Dr. Carpenter has reprinted lectures which he gave, during the summer session of 1876, at St. Thomas's Hospital. They were addressed to students, and the form in which they were first given has been preserved. At a time when, in the words of the Prince of Wales's letter to the Society of Arts, "the supply of pure water to the population is exciting deep interest throughout the country," the volume will be found a convenient and ready *résumé* for those who wish to inform themselves on the more important questions that enter into the consideration of what is a good water supply, and what is to be done with fouled water. As is well known, Dr. Carpenter advocates sewage-farms as the proper way to dispose of sewage, and the chapters devoted to this subject enter into financial as well as scientific consideration. In speaking of the spread of epidemic diseases by water and by air Dr. Carpenter explains the germ theory, but we cannot find that he even alludes to any other possible explanation. It appears as if he regards the germ theory of disease as really *proved*. Is it?

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

The Phonograph

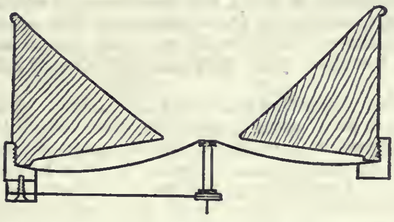
I HAVE received the following interesting letter from Dr. Blake, Boston, U.S.A. :— W. H. PREECE

"You may possibly be interested in some recent experiments which I have made with the phonograph, and unless you have been pursuing the same course, may find them worthy of repetition.

"I found that the groove in the cylinder, covered with tin-foil, became a resonator for the high scratching noise of the embossing point, materially interfering with the reproduction of the quality of the voice.

"By stretching a thin layer of rubber tissue over the cylinder this resonating effect was done away with and the scratching noise materially lessened. This experiment was new to Mr. Edison, and has since been repeated with like success.

"Since experiments made with the ferrotype telephone and phonograph-discs show that they transmit with almost astonishing accuracy the lighter over-tones of the human voice, but at the same time give especial prominence to certain over-tones to which the metal disc especially responds, I constructed a diaphragm upon the principle of the human drum membrane, to be used as a reproducing disc; the object being to employ a membrane which, from its structure and shape, would reproduce the lighter over-tones representing the quality of the voice, and at the same time 'cut off' the sharper exaggerated over-tones embossed as such by the metal disc upon the tin-foil. The results of the experiments with such a membrane were very



A small rod of light pine wood having a rubber pad at either end is placed between the boss which carries the embossing point and the centre of the membrane. This, the first form of disc constructed, worked very well.

gratifying. After embossing with the metal disc, the curved membrane was substituted, and the voice reproduced from the phonograph without the sharper over-tones, with much more natural and agreeable quality and with more than double intensity. On using the curved membrane for embossing as well as reproducing, I found, as would be expected, that the quality of the voice was more accurately represented, and that the embossing could be done at a distance of over fifteen feet from the phonograph, and be reproduced with clearness.

"Mr. Edison is now experimenting with this form of diaphragm, and, I understand, with very good results.

"The material used for these discs may be either stout felted paper (to be varnished on the outer surface when used for speaking) or drum-head, moistened and pressed into concave form before using. The principle governing the vibrations of such a disc is that of imparting the vibrations to the centre of a membrane the curve of which enables it to reproduce a large range of over-tones, its tension serving as a counterbalance to the central pressure. "CLARENCE J. BLAKE

"W H. Preece, Esq., London."

Physical Science for Artists

WITH reference to Mr. Norman Lockyer's and Prof. Brücke's observations on the appearance presented by the shadow of the earth at sunset or sunrise (NATURE, vol. xviii. p. 223), I beg to be allowed to confirm them by my experience in Switzerland. Early starts for expeditions give one, among other good things, opportunities for seeing sunrise from the very beginning, and I have repeatedly seen the shadow of the earth, as it were, gradually driven down by the illuminated portion of the sky, the boundary between them being very well marked and roughly circular like the horizon, but I think with a greater apparent curvature. At this distance of time (some years) I cannot remember anything of an effect of foreshortening such as Prof. Brücke notices.

Once, in 1868, I saw an even more curious effect. As we stood at sunrise on a moderately sharp ridge running pretty closely north and south, at a height of 9,000 to 10,000 feet, there was an interval of appreciable duration in which it was a visible and striking fact that it was night on one side of the ridge and day on the other. F. POLLOCK

Savile Club, Savile Row, W., June 27

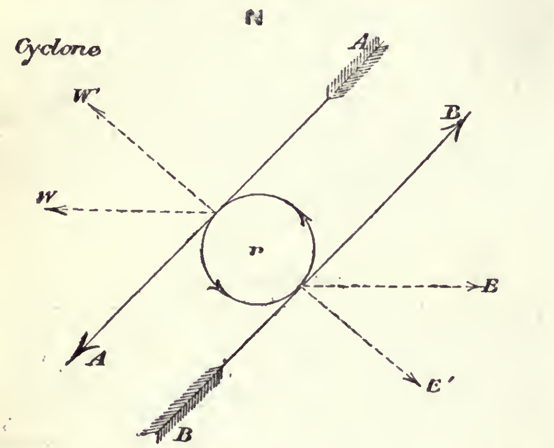
Cyclones and Anticyclones

I WILL endeavour to put into a written form the ideas which have occurred to me respecting the law which, as I suppose, connects and governs the atmospheric phenomena which I see referred to in the newspapers as cyclones and anticyclones. I have seen it stated, as the result of observation, that in whatever direction the wind may be blowing at any given time, if you place your back to it the barometer will be found to stand lower upon your left than upon your right. I have also seen it stated that what are termed cyclones are rotatory movements of the air occasioned by the meeting and passing one another of two currents of air moving in opposite, or nearly opposite, directions, and that these cyclones or rotatory storms, though differing much in area, have certain features common to most, if not all, of them; namely, that the direction of their rotation is from right to left, or, in other words, the opposite of the motion of the hands of a watch, and that in their centre is found a considerable diminution of atmospheric pressure. On the other hand, in what is termed an anticyclone, the direction of the rotatory movement is in the opposite direction, that is, from left to right, or in the same direction as the movement of the hands of a watch, while in the central region the barometer is found to be standing high.

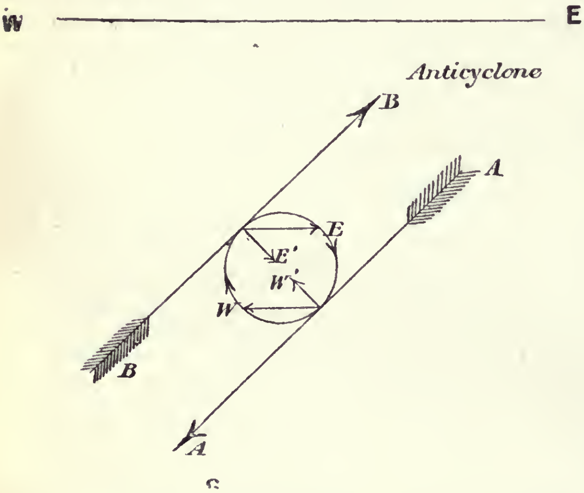
These various phenomena appear to me to be closely connected one with the other, and to be, in fact, due to the rotation of the earth upon its axis, which, having regard to its spherical form, makes it inevitable that the superincumbent air at the equator must rotate with the earth under it at a much greater velocity than that which is near the pole. For it seems evident that a current of air coming from the north travels into a region which is moving to the east more quickly than itself, and will therefore present itself as a north-east wind to the inhabitants of the more northern latitude, and not only so, but will tend to arrest the air on its right or westerly hand, while it is left or abandoned by the more quickly eastward-moving air on its left or easterly hand. This consideration will explain, so far as northerly winds are concerned, the first-mentioned of the phenomena above referred to, namely, the lower glass on the left hand, the higher glass on the right. Taking next the case of a northerly wind, it will be obvious that in travelling northward it comes to a country moving westward more slowly than itself, and consequently appears as a north-west wind to the people over whose land it passes; and not only so, but by pressing on the air to the right, or eastward side, it increases pressure in that direction, while it tends to leave behind the more slowly moving air on its left, or westward side, thus again producing the first-mentioned phenomenon of a high glass on the right and a low glass on the left, so far as southerly winds are concerned. If this principle is considered with reference to a cyclone and the direction of the rotatory movement is also taken into account, it seems to be made clear that a cyclone is occasioned *by the meeting and passing each other of a northerly and southerly current so that they pass each other on the left hand respectively.*

When this occurs the low pressure on the left or east side of the north wind coincides with the low pressure on the left or west side of the south wind, and thus a depression is formed round which the wind rotates. It follows that the west and south wind is found in the south and south-east side of the storm,

while the north-westerly current is on the north-west side, or as it is sometimes termed, the back of the storm. In the case of an anticyclone the whole thing is reversed. *The two currents pass each other on their respective right hands.* This enables the high glass on the right side of each to coincide with one another. The two winds instead of *dragging away from each other, are pushing against each other, and form a heap of air round which they*



CYCLONE.—A A, a north-east wind; B B, a south-west wind; W, westerly drift of A A due to difference of absolute velocities of earth's motion at different latitudes; W', effective part of W, in producing rarefaction at *r*; E', effective part of E as above. *Result*, rarefaction in centre of cyclone.



ANTICYCLONE.—*Mutatis mutandis* as cyclone. *Result*, condensation at centre of anticyclone.

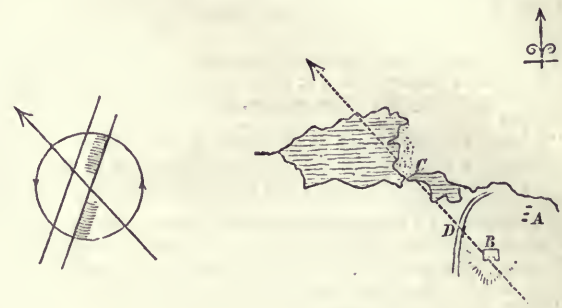
rotate, not necessarily in the opposite direction to that of the cyclone. It would be interesting to know whether an anticyclone travels from north-east to south-west. Whether it does so or not I do not know; but this is what would seem to follow if the above imperfectly-stated theory is a correct one.

EUSTACE BARHAM

Whirlwind

As meteorologists appear to be taking much interest in whirlwinds and waterspouts, you will perhaps allow me to offer you a few notes respecting a whirlwind that passed over a mountainous part of Northumberland on April 14, 1869, and left indisputable evidence of the direction in which it revolved, a fact of some importance, and one in general so difficult to ascertain, that after much research I have never yet met with a description of either whirlwind or waterspout that can be considered satisfactory in this respect.

I have long held the opinion that the smaller whirlwinds and waterspouts are of the same nature, and follow the same laws, as the greater cyclones, although Sir Wm. Reid, at p. 461, vol. i. of his "Law of Storms," is of a contrary opinion, founded on observations of waterspouts at sea, where it is extremely difficult to judge by the eye in which direction a spout is rotating. The cases where, as in America, attempts have been made to settle this point by the direction of trees thrown down by the whirlwind are very unsatisfactory; and there is nothing definite



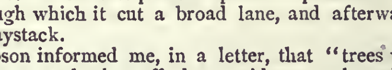
on this head to be met with in the description of upwards of three hundred whirlwinds described by Peltier in his work "Sur les Trombes."

The 14th of April, 1869, was exceedingly wild and stormy, and so dark at mid-day that we could scarcely see to write at a meeting of churchwardens in the vestry of Hexham Abbey Church. Having heard of the whirlwind at Sweethope, about ten miles north of Hexham, I went thither in July, 1869, accompanied by my friend Dr. James Smith, of Newcastle-upon-Tyne. We passed the night at Sweethope Farm (A), and examined the course of the meteor carefully. Masons were still engaged rebuilding a stable and boathouse (C) which stand at the northern extremity of the embankment that separates the larger from the smaller lake, from which issues the River Wansbeck, that flows past Morpeth, about twenty miles to the eastward.

The whirlwind was first noticed by the inmates of Mr. Robson's house, A, as it passed a small plantation on a hill at B, and was seen to travel in a north-westerly direction across the road at D, along the embankment between the two lakes, over the boathouse, C. From this point it passed a plantation of young trees, through which it cut a broad lane, and afterwards overturned a haystack.

Mr. Robson informed me, in a letter, that "trees were torn up by fifties, some broken off about midway, and carried a considerable distance in the air. Stones were turned up that would have taxed the powers of three or four strong men. Several sheep and lambs were lifted up into the air and killed by the fall; others were carried up, and, falling into the lake, were drowned. There was a tremendous thunderstorm, with forked lightning and very large hailstones. It did not travel very fast, and was like a large volume of smoke."

The boathouse was entirely unroofed and the nails drawn out of the planks of a floor of a room in the upper part of the building. The small plantation at A is 812 feet above the sea-level. Nothing was seen or heard of the whirlwind beyond the



limits of the diagram, which is copied from the Ordnance Survey Maps on the scale of an inch to a mile.

So far, the Sweethope whirlwind presented only the usual features of its class, and we were about to depart, after some good sport among the fish in the lakes, when Mr. Robson's son mentioned to me that the whirlwind, in crossing the road (at D), had thrown part of the wall into the road and another part into the field, a significant fact of which we at once proceeded to examine the details. At the point D in the diagram the wall runs in a direction nearly north by east, and has been about four feet high. At the southern end we found about two feet of the

upper part for a distance of eighteen yards thrown into the field. Then came about 9 yards of wall quite undisturbed, and afterwards thirty-six yards half down, but lying in the roadway on the opposite side of the wall. About seventeen yards of the coping-stone at the extreme northern end of the broken wall was also thrown into the road.

Fortunately the whole lay at the time of our visit just as when the whirlwind had passed, and proved conclusively that, in this case at least, the order of rotation was the same as that of the cyclones of the northern hemisphere. THOMAS DOBSON

Marine School, South Shields, June 22

Zoological Geography—Didus and Didunculus

I AM at a loss to understand how *Didunculus* can be called "a near congener" of the Dodo, as Mr. Searles V. Wood, apparently following Dr. Litton Forbes (whose paper I have not seen), terms it (*suprà*, p. 220). The two birds, so far from being congeneric, belong to perfectly distinct groups of the Order *Columbæ*, and nearly thirty years ago Bonaparte treated them as the types of distinct families—*Didideæ* and *Didunculidæ*—an example which has been generally followed by the best authorities. If Mr. Wood will refer to a paper in the *Philosophical Transactions* for 1869 (pp. 327–362) I think he will see that there is good ground for not attaching much importance to the slight and superficial characters in which *Didunculus* resembles the *Dididæ*. ALFRED NEWTON

June 30

A Subject-Index to Scientific Periodical Literature

I BEG permission to ventilate in your columns a subject which must make itself felt more or less to all your readers, viz., the want of some subject-index to the vast amount of material scattered about in the numerous scientific periodical publications of the present day. It is true we have the admirable catalogue of the Royal Society, but unless you are acquainted with the name of every author who has written on your subject, it is nearly hopeless attempting a complete bibliography of it. Now I would suggest whether an index to the Royal Society's catalogue cannot be made on the same plan that has been adopted by the committee of the new edition of "Poole's Index," viz., by getting different societies, libraries, or individuals to take certain parts of the work. The following is a short abstract of how this committee have set about their work; any of your readers who wish for further information will find it at pp. 109–206 of the "Transactions and Proceedings of the Conference of Librarians, London, 1878," and on p. 201 a short specimen may be seen. The index is made on sheets of foolscap, and the indexer has nothing to do with alphabetical arrangement; he makes his entries in the order the articles occur in the volume at which he is working; these sheets are then sent to the editors, who cut them into slips and work them into alphabetical order with the material coming in from other sources. By this method complete uniformity is maintained; for should the indexer have a peculiar idea of his own how any particular part should be done, his peculiarity is put right at the central bureau or editorial office.

I have said this should be an index to the Royal Society's catalogue, but if this scheme is ever carried into execution I would strongly urge that the index should be made from the periodicals themselves, and not from the entries in the Royal Society's catalogue, as it is absolutely impossible to index a paper properly from the title only; and another advantage is that under this plan the work could be better carried out, as each indexer could confine himself to his own branch of study; whereas if the index were made from the catalogue itself, it must be cut up into alphabetical portions, and each man would have to do a variety of subjects. This may seem to many too large a matter even for consideration, but for many years so was a good alphabetical catalogue of the different scientific papers; this has been conquered by the Royal Society, and if that learned body would constitute itself the central bureau, I think willing workers would soon be found, and the success of the index be assured. Of course all this would cost money, but surely an appeal might fairly be made to scientific societies and individuals to help in this work, which would be so great a help to the "advancement of science." JAS. B. BAILEY

Oxford

A NEW TRIUMPH OF CHEMICAL SYNTHESIS

THE year 1868 was a marked epoch in the progress of chemical synthesis as well as of tinctorial processes. The German chemists, Profs. Graebe and Liebermann, succeeded at that date in preparing from the hydrocarbon anthracene manufactured from coal tar the brilliant dye-stuffs hitherto won from madder, and in establishing also the chemical constitution of these various compounds and their relationship to other well-known bodies. This was the first instance in which the chemist had succeeded in artificially preparing colours occurring in the vegetable kingdom; and although the manufacture of artificial madder colouring matters has assumed at the present day colossal proportions and bids fair to entirely supersede the preparation of the natural products, it has hitherto remained the only instance of the kind in the history of chemistry, all other vegetable and animal dyes obstinately refusing to disclose the secret of their composition and be classified among the compounds of well-defined molecular structure. Within the past few weeks the madder colours have ceased to occupy this unique position. Modern chemistry has succeeded in preparing synthetically none other than common indigo, the well-known product of the *Isatis tinctoria*, and *Nerium tinctorium* of India and South America.

This discovery is likewise due to a German chemist, Prof. A. Baeyer, the genial successor to the chair of Liebig at Munich, one of the most indefatigable and successful investigators of our day. For a score of years he has been seeking to solve the problem of the constitution of indigo and its synthetical preparation. Slowly and patiently he has gathered together and elaborated one fact after another, until finally, at the last session of the German Chemical Society, he was able to announce the completion of the long research and the discovery of the last link in the chain of synthetic reactions leading to the formation of indigo.

We will sketch briefly the various steps in this synthesis, which is not only one of the most brilliant chemical achievements of the present year, but affords an unusually interesting glimpse into the methods and aims of the modern chemist.

Indigo blue, or indigotine, possesses the formula C_8H_5NO, and, from the products of its decomposition, aniline, orthoamidobenzoic acid, &c., has long been regarded as closely allied to the benzene series. Attempts without number have been made to show the nature of this connection by starting from benzene compounds, but hitherto with fruitless results. As in the case of the alizarine compounds, where Graebe and Liebermann first found that anthracene was obtained from alizarine by reducing agents, so has the first step in the solution of the indigo problem been to study carefully the various compounds resulting from successive decompositions, each in turn yielding a body of a simpler constitution. Passing from one compound to another, Prof. Baeyer finally reached alpha-toluic acid or phenylacetic acid, $C_6H_5.CH_2.COOH$, a not uncommon body, easily prepared from cyanide of benzyl. Here he stopped, and began to retrace his footsteps.

The first reaction was to replace one of the hydrogen atoms in the phenylic group of *phenylacetic acid*,

$$\text{(A.)}\quad C_6H_5.CH_2.COOH,$$

by the group NO_2—a familiar operation effected by treatment with nitric acid—and giving, among other compounds, *ortho-nitrophenylacetic acid*,

$$\text{(B.)}\quad C_6H_4\begin{cases}CH_2.COOH\\NO_2\end{cases}.$$

This, when reduced by nascent hydrogen—*i.e.*, submitted to treatment with a mixture of tin and hydrochloric

acid—gives the corresponding *ortho-amidophenylacetic acid*,

$$\text{(C.)}\quad C_6H_4\left\langle\begin{matrix}CH_2.COOH\\NH_2\end{matrix}\right. .$$

In a neutral solution this acid is changed into its anhydride by the elimination of a molecule of water forming

$$\text{(D.)}\quad C_6H_4\left\langle\begin{matrix}CH_2.CO\\NH\end{matrix}\right\rangle .$$

And here we leave the long names indicative of the structural composition of the compounds: for Prof. Baeyer has found that this anhydride is identical with oxindol, one of the derivatives of indigo. The next steps are to introduce the nitroso group, NO, forming *nitroso-oxindol*,

$$\text{(E.)}\quad C_6H_4\left\langle\begin{matrix}CH(NO).CO\\NH\end{matrix}\right\rangle ,$$

and to reduce this as before to *amido-oxindol*,

$$\text{(F.)}\quad C_6H_4\left\langle\begin{matrix}CH(NH_2).CO\\NH\end{matrix}\right\rangle .$$

This compound, when oxidised with chloride of iron or copper, or with nitrous acid, is changed entirely into *isatin*,

$$\text{(G.)}\quad C_6H_4\left\langle\begin{matrix}COCO\\NH\end{matrix}\right\rangle ,$$

a substance resulting from the oxidation of indigo, which already in 1870 Prof. Baeyer, by the action of phosphorus trichloride, had changed back into *indigo-blue*,

$$\text{(H.)}\quad \begin{matrix}N.C_6H_4.CO.CH\\ \| \qquad\qquad \| \\ N.C_6H_4.CO.CH\end{matrix} ,$$

by the union of two molecules and the elimination of two atoms of oxygen. With this last transformation the synthesis was completed. Although the operations are too numerous and too costly to allow at present any hope of the practical utilisation of this ingenious succession of reactions, the series presents still a remarkable example of the possibilities in the hands of the organic chemist, of the powers of combination requisite for the successful pursuit of modern synthetical research, and of the attractions which draw to this province the majority of our leading chemists. T. H. N.

BIOLOGICAL NOTES

THE COMET-FORMS OF STAR-FISHES.—Ernst Haeckel, in a recent number of the *Zeitsch. wiss. Zool.* (1878, Supplement 3), draws attention to these forms, and the support which the facts recently established as to the power possessed by certain star-fishes of multiplying by throwing off their arms, lends to his theory of the origin of the Echinoderma by the continually increasing integration or centralisation of a radially-connected colony of worm-like persons. The phenomenon of self-division across the disc has been observed in species of Asteracanthion (Uraster) by Lütken and Kowalewsky; the production of comet-forms depends, however, on the separation of single arms, which then reproduce the whole disc and remaining arms by budding. Martens, in 1866, observed this in the case of a Luidia (Ophidiaster) in the Red Sea. Kowalewsky found that it was a common process with similar species and same locality. Sars observed it in Brisinga. Studer has described the regular occurrence in Labidiaster of a spontaneous casting off of the arms, but not the regeneration of disc and arms on the separated arms. Sir John Dalyell observed the whole process of reproduction of the disc on a single detached arm of *Asteracanthion* (*Uraster*) *glacialis*. The support which these facts lend to the "Astrocormus" theory is not of that value which Haeckel would assign to them, for such physiological tests of morphological doctrine are necessarily delusive. We have only to remember the facts as to cuttings and graftings in organisms generally in order to see that no special argument can be based upon them as to details of morphological composition. Haeckel proposes to divide the Echinoderms or Estrellæ as follows:—

Group I.—Protestrellæ: Class I. Asteriæ.
Group II.—Anthestrellæ: Class 2. Ophiuræ; Class 3. Crinoida.
Group III.—Thecestrellæ: Class 4. Blastoida; Class 5. Echinida; Class 6. Holothuriæ.

The second and third groups have developed from the first as diverging branches, whilst the Holothuriæ are modified descendants of Echinida. The resemblances between Gephyrea and Holothurians are declared by Haeckel to be entirely due to parallel adaptation (homoplasy), the pair of branched excretory organs of Bonellia, &c., being totally unrelated to the dendriform water-lungs of Holothurians, which are *five* in number in primitive forms and agree with branched inter-radial cœca (not the so-called "hepatic" cœca) of the intestine found in certain star-fishes (Archaster, Astropecten). E. R. L.

THE TRANSFORMATIONS OF BLISTER-BEETLES.—According to Dr. C. V. Riley, who has studied these creatures for some years, the young of all vesicants belonging to the Meloidæ, develop in the cells of honey-making bees, first devouring the egg of the bee and then the honey and bee-bread. They are all remarkable for their hypermetamorphosis, passing through several larval stages. The young Meloës are at first simple larvæ called triungulins, running actively about, climbing to flowers visited by bees, to which they attach themselves. They have stout thighs and claws, but feeble jaws. Only a few can get attached to the proper bees, the others must perish. Once in the cell the creature eats the bees' egg, and then moults and assumes the second larval condition. In this state it is clumsy and little locomotive, and feeds on the honey store. It then becomes a pseudo-pupa, and later a third larva within the partially-rent skin; the true pupa stage being still later. Another genus of the family is *Hornia*, of which a remarkable species is found around St. Louis, with the elytra and wings extremely reduced. The Hornia resides mostly in the galleries of *Anthrophora sponsa*, out of which it can scarcely crawl. The hypermetamorphosis is of the same character as in Meloë (*American Naturalist*, April). The genus Epicauta exhibits a very parallel history.

CURIOUS SOCIAL RELATIONS.—Stories about prairie dogs, owls, and rattlesnakes are well known, but trustworthy scientific observations about them are not very numerous. Mr. S. W. Williston (*American Naturalist*, April) gives the results of several years personal observations. He says that prairie dogs can thrive even in the dry scorched deserts of Southern Colorado, and the cold bleak Laramie plains. They are very provident in summer for winter, but yet emerge in spring much reduced in plumpness. At the approach of danger signals of distress are given, and when actually attacked they get into their mounds with wonderful speed, escaping beyond reach even when a rifle has scattered the brains of the animal. The burrowing owl not unfrequently occupies the same hole; the prairie dog pays little heed to it but tolerates it. The owls present a most ridiculous appearance, standing during the day at the entrance of their dwellings, in an attitude of the deepest contemplation; at the appearance of an intruder they begin the most comical bowings and curtsies, and at last with a cry like a watchman's rattle fly off to a neighbouring mound. The rattlesnakes cannot be said to be friendly with either of these creatures. Out of many hundreds of rattlesnakes destroyed by Mr. Williston, a number had devoured the young of the prairie dog, but none the young owl. The occupancy of a burrow by a

rattlesnake does not, however, prevent the entrance of the dog; the rattlesnake is never wanton, and only defends itself and takes necessary food. The dog will pass by it to enter its burrow without being molested.

CLEISTOGAMOUS FLOWERS IN GRASSES.—Mr. C. G. Pringle has discovered in Western Vermont cleistogamous flowers in several grasses, especially *Danthonia spicata*. The latter has many flowers totally concealed in the sheaths, the glumes and pales being much simplified, but the sexual parts being perfect and producing seeds. This plant is spreading rapidly in Vermont. The seeds borne on the top of the culm fall mostly at midsummer and lodge close to the parent plant, but the concealed seeds stored around the culm remain till these are disjointed and driven about by the autumn and winter winds; consequently, a wide means of dissemination is provided.

ON THE VIEW OF THE PROPAGATION OF SOUND DEMANDED BY THE ACCEPTANCE OF THE KINETIC THEORY OF GASES

1. IT is an accepted fact that the molecules of a gas are *in motion* among themselves in their normal state, and incapable of acting on each other at a distance; so that a theory of the propagation of sound, based upon the contrary suppositions that the molecules of a gas are *at rest* in their normal state and capable of acting on each other at a distance, cannot possibly be tenable. It thereby becomes necessary to inquire what view of the propagation of sound is demanded by the acceptance of the kinetic theory of gases; and this inquiry would appear to be all the more important in view of the fact that the mechanism of the propagation of sound in gases forms the physical basis of a great part of acoustics, or the groundwork upon which a number of its problems depend—the *physical basis* that underlies a system being admittedly the most important of the whole.

2. The molecules of a gas being in motion among themselves, it becomes evident after a very brief consideration of the question, that the only way in which a small impulse (or variation of velocity) termed a "wave" can be propagated through a gas, is by the exchange of motion normally going on among the molecules of the gas. *For the molecules have no other mode of acting upon each other, excepting by exchange of motion.* The rate at which this "wave" (or small variation of velocity) is propagated through the gas, will therefore depend on *the rate at which the molecules exchange motion*, i.e. on the normal velocity of the molecules of the gas. The sole condition determining the velocity of propagation of sound in a gas is therefore the velocity of the molecules of the gas. Here, therefore, we have a very simple condition for the velocity of sound (on the basis of the kinetic theory), or the velocity of sound becomes thus dependent only on *one* condition. This simplicity is characteristic of the rest of the kinetic theory, and is (it may be added) the recognised quality of scientific truth. In gases of the most diverse densities, specific gravities, pressures, and temperatures, the velocity of sound is only dependent on *one* condition, viz., *the velocity of the molecules*, of the gas.

3. That the velocity of sound is independent of *density*, will be evident from the consideration that the molecules of gas are almost indefinitely small compared with their length of free path, and also the time of a collision is indefinitely small compared with the time taken to traverse the free path, so that it does not matter how many collisions (or exchanges of motion) occur along the line of passage of the impulse (or "wave"), but simply on the *rate of motion* of the molecules conveying the impulse. So (to take a simple analogy by way of illustration), it does not matter how many couriers are along the line of route conveying a message, but on the rate of motion of the couriers. Adding to the number of molecules in unit of volume of a gas (or adding to the density) does not, therefore, alter the velocity of sound in a gas, because it does not alter the *velocity* of the molecules which (by their exchange of motion) propagate the wave. The old theory supposes that the velocity of sound is here unaltered, because increased density *diminishes* the velocity of propagation of the wave, and increased pressure (attendant on the increased density) *augments* the velocity of the wave, and thus the two conditions counteract each other. On the kinetic theory, neither of these conditions can have any effect, and therefore the explanation of the unaltered velocity of the wave is perfectly simple, being the cosequence of the *unaltered* velocity of the molecules which propagate it. It is unnecessary to comment on the contrasted simplicity of the view on the kinetic theory; which is, moreover, the *true* view, if the kinetic theory be accepted.

4. That the velocity of sound on the kinetic theory is independent of *pressure*, is sufficiently clear at first sight; for pressure evidently could not influence the rate at which the molecules exchange motion among each other, through which means alone the impulse is conveyed.

5. That change of *specific gravity* (or molecular weight) can by itself have no effect on the velocity of the sound-wave, is evident from the fact that it cannot matter whether the molecules exchanging motion among each other (and propagating the impulse) be heavy or light, provided their velocity be the same. It has been (as is known) demonstrated, generally from dynamical principles, that a system of bodies in free collision all tend to acquire the *same* absolute energy. Hence the velocity of each body depends on its mass (or varies inversely as the square root of its mass). So the mass of the molecules of hydrogen being (as is known) one sixteenth that of the molecules of oxygen, the velocity of the molecules of hydrogen is four times greater than that of the molecules of oxygen; and accordingly *for this reason* the velocity of sound in hydrogen is exactly four times greater than its velocity in oxygen—not, however, because the molecules propagating the wave are heavy or light. The molecules of hydrogen in their normal exchange of motion, move at four times the speed (compared with those of oxygen), and therefore propagate by this exchange of motion the sound-wave at four times the speed. The specific gravity (or molecular weight) of the gas has evidently nothing whatever to do with the rate of propagation of sound. The reason why the velocity of propagation of sound *appears* to depend on the molecular weight of the gas is because the *velocity* of the molecules of the gas depends on the molecular weight.

6. So also the velocity of sound is independent of the *temperature*, provided the molecular velocity remains the same. Of course this could only be true of *different* gases (*i.e.*, of gases of different molecular weights), which—as is known—may be at different temperatures and yet possess the *same* molecular velocities. In one and the same gas of course the temperature could not be altered without altering the molecular velocity, for the "heat" itself consists in the motion of the molecules of the gas. This is therefore evidently the *cause* why the application of heat to a gas increases the velocity of sound. The addition of "heat" simply represents (as is known) the addition of velocity to the molecules of the gas, which consequently, by their exchange of motion, propagate the wave at a greater rate. The explanation of the increased velocity of sound in a heated gas is thus simple and direct. On the old theory the increased velocity of the sound-wave in a heated gas is referred to the diminished *density* of the heated gas (attendant on its expansion); and when the gas is confined, to its increased pressure. Surely this is at best a somewhat laboured and

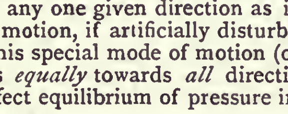
indirect way of accounting for a fact, and (as we have seen) according to the *kinetic* theory it cannot hold, since according to this theory, *density* and *pressure* can have no influence on the velocity of the wave, and on the other hand it is *a known fact* that the velocity of the molecules in their exchange of motion (by which means alone they can propagate the wave) is *increased* by the heat—indeed this augmentation of velocity itself represents the added "heat." This explanation of the increased velocity of a sound-wave in a heated gas commends itself therefore not only by its simplicity, but as a matter of scientific truth.

7. This serves also to explain in a direct and simple manner the relation the velocity of sound in a gas bears to the temperature. The absolute "temperature" of the gas represents (as is known) simply the energy of the molecules. The velocity of the molecules (as of any moving system of bodies) is proportional to the square root of their energy, and therefore proportional to the square root of the absolute temperature (since the "temperature" represents the energy). The velocity of sound, therefore (which is proportional to the velocity of the molecules), is thereby proportional to the square root of the absolute temperature of the gas.

8. To afford a more distinct idea of the mode of propagation of the wave and the physical effect (condensation and rarefaction) produced on the gas by its passage, the following considerations may serve. It is an important fact to keep in view that a system of bodies in free collision, such as the molecules of a gas, do not move in a mere chance or perfectly irregular manner, but a certain *regularity* exists. It has been mathematically proved that a forcible self-acting adjustment goes on among the colliding molecules of a gas so as to cause them to move in a *special* manner, viz., so that *an equal number of molecules are moving in all directions*, or as many molecules are moving in any one given direction as in the opposite. This mode of motion, if artificially disturbed, will correct itself. It is this special mode of motion (or movement of the molecules *equally* towards *all* directions) that produces the perfect equilibrium of pressure in all directions, observed in a gas.

9. From the fact that as many molecules are moving in any one direction as in the opposite; it follows that if an imaginary plane be placed in any position outside a vessel containing gas, the number of molecules (in the vessel) which at any instant are approaching the plane, is equal to the number which at the same instant are receding from it. Or otherwise, if we suppose any imaginary straight line in a gas, and visualise the molecules upon this line, then, *as many molecules are moving in one direction as in the opposite.* In the case of those molecules which are moving *obliquely* to the line, the resolved component of the motion in the direction of the line can be taken. This consideration enables the mode of motion of the molecules of a gas in its normal state, and the manner of propagation of waves through that mode of motion, to be illustrated in a very simple manner.

10. In the annexed diagram, let 1, 2, 3, &c., represent a line of spheres moving in such a way that *as many spheres are moving in one direction as in the opposite.* All the spheres marked with the odd numbers may be supposed



to move in one direction, while those marked with the even numbers move simultaneously in the reverse direction, the *vis viva* in the one direction balancing that in the opposite direction (as is the case with a gas). Each alternate sphere thus simply oscillates backwards and forwards in opposite directions within the limits represented by the dotted lines in the diagram, the spheres continually rebounding from each other, and the line of spheres tending to open out or expand and separate the final controlling surfaces A and B (like the expansive action of a gas). It will be observed that this is in *principle* the only mode of motion possible by which the spheres can be in equilibrium; or half move in one direction and half in the opposite, so that the centre of gravity of the whole is at rest, in analogy with the centre of gravity of a portion of gas (the *vis viva* being at the same time balanced). There are only minute differences of detail as regards the comparison with a gas, none of *principle.* One detail is that every *alternate* molecule (in a line of molecules taken in a gas) does not necessarily move in an opposite direction, but it is rigidly true (on account of the vast multiplicity of molecules) that in any appreciable portion of a line taken in a gas, as many molecules are moving in one direction as in the opposite; for if not, the gas could not be in equilibrium in the direction of this line, whereas it is known to be in equilibrium in *every* direction. Another detail is that some of the molecules of a gas are moving *obliquely* to such an imaginary line, so that the mean path of the molecules is generally greater than that represented by the spheres. These details cannot however in the least affect the *principle,* and therefore the above method of illustration will serve (keeping in view the small differences mentioned) to convey a perfectly just idea of the character of the motion of the molecules of a gas in its normal state, and the way in which through that mode of motion "waves" are propagated through the gas. It is evident that an illustration is desirable in order to visualise clearly the facts.[1]

11. Suppose, now, a slow oscillatory motion in the form of a movement of vibration to be communicated to the plane A. The plane B may be supposed removed and the line of spheres extended indefinitely from the plane A. Then at the first forward swing of the plane A, the sphere 1 will receive an increment of velocity which it will transfer by collision to sphere 2, the sphere 1 returning with its normal velocity to the plane, and receiving from it a second increment, &c. By the forward swing of the plane, a succession of small increments of velocity will thus be propagated in the form of a pulse or semi-wave along the line of spheres, the velocity of propagation of the pulse being that of the spheres themselves. By the backward swing of the plane (to finish one complete vibration) a series of small decrements of velocity forming the second half of the wave will be propagated in precisely the same manner along the line of spheres. Owing to the succession of increments of velocity received by the spheres in the first half of the wave, these spheres will be shifted bodily forwards (to a slight extent), and owing to the succession of decrements of velocity sustained by the spheres in the second half of the wave, these spheres will be shifted (to a slight extent) bodily backwards, an alternate closing and opening out of the line of spheres corresponding to "condensations" and "rarefactions" being the result. There is only one slight (quantitative) difference in the case of an actual gas. Owing to the fact that some of the molecules in the case of a gas are moving (at the instant of passage of the wave) *obliquely* to the line of propagation of the wave, the rate of advance of the wave along the line of propagation will necessarily be somewhat slower than the velocity of the molecules which propagate it. It is (to take a homely illustration) as if some couriers were transmitting a message, and some of them were moving obliquely to the line of transmission of the message, when evidently the rate of transmission would be less than the velocity of the couriers. In order to obtain the true rate of propagation of the wave in the gas, the oblique motions of the molecules must be taken into account.

12. In connection with a former paper[2] bearing on this

[1] The mere fact of molecules, in the case of a gas, shifting their positions (through diffusion) can of course make no difference, since the *same character* of motion is rigidly kept up.

[2] *Phil. Mag.*, June, 1877.

subject, the true mathematical expression for the velocity of the wave in terms of that of the molecules of the gas has been determined by Prof. Maxwell. The expression is—Velocity of wave equals $\frac{\sqrt{5}}{3}$ into the velocity of the molecules. This expression requires a slight additional correction in the case of most gases, owing to the movements of *rotation* developed at the collisions of the molecules, depending on their more or less irregular shape, which rotation calculably must delay the wave to a certain extent. According to the experimental results of Kundt and Warburg, the above expression for the velocity of sound in terms of that of the molecules holds exactly true (without correction) for vapour of mercury (whose molecules, it might perhaps be remarked, are simple or monatomic). The slight deviations from the above constant for the velocity of the wave that one observes in fact, are quite consistent with what one would expect from theory.

13. It may be observed that all the usual apparatus for illustrating sound-waves of course applies to the kinetic theory, as such apparatus is only intended to show the effect produced on the *mass* of air, or the condensations and rarefactions, without exhibiting the molecular mechanism that underlies it. A true view of the mode of propagation of the wave and the manner in which the condensations and rarefactions are produced at its passage, can only be obtained by visualising the fact that the molecules of gas are *in motion* in the normal state of the gas, in accordance with the accepted kinetic theory of gases.[1]

14. The kinetic theory thus reduces the conditions on which the velocity of sound in a gas depends to *one*, viz., the *velocity of the molecules of the gas*. It is not, however, this simplification alone that should recommend it, for it is not a mere question of choice or preference of one view over another, but a question of *fact*. For a theory of the conditions physically affecting the velocity of sound and its mode of propagation that may apply to one view as to the constitution of a gas (viz., the old view where the molecules are supposed *at rest*), cannot possibly apply to the diametrically opposite view of gaseous constitution represented by the accepted kinetic theory. It would appear desirable and fitting that the kinetic theory, having been applied so generally in other respects, should find a general application to so important and fundamental a fact affecting a gas as the propagation of sound in it.

15. Since the *physical basis* of a system is admittedly the most important of the whole, it would appear reasonable to expect that the investigation of problems in acoustics might gain by regarding the propagation of sound on the true physical basis represented by the accepted kinetic theory of gases; or by taking a true physical basis to ground the investigations upon, instead of one (based upon the old view of gaseous constitution) that admittedly does not harmonise with the facts.

NOTE.—It has recently come to my knowledge that two papers have been lately published on this subject, one by Prof. Roiti, of Florence (*Nuovo Cimento*, 1877), the other by Prof. J. H. Hoorweg (*Archiv Néer.*, xi., 1876), a brief abstract of which also appears in *Beiblätter zu den Annalen der Physik und Chemie* (vol. i. part 4, p. 209, 1877). Though the latter of these papers appears to precede mine (*Phil. Mag.*, June, 1877), I may add that a sketch of the same theory appears in a little book ("Physics of the Ether," E. and F. N. Spon), published by me in 1875. There is also an interesting paper by Mr. J. J. Waterston (*Phil. Mag.*, 1859, supp. to vol. 16), in which he proposes to illustrate the propagation of waves by a system of spheres, but he does not go into the explanation as to how the motion he assigns to the spheres can properly represent the case of a gas in its normal state. There are, nevertheless, points of interest in the paper.

S. TOLVER PRESTON

[1] It would appear not unreasonable to conclude that a realisation of the *molecular basis* underlying the propagation of sound, according to the accepted kinetic theory, might be able to throw some light on the investigations in connection with the telephone and other allied instruments, where the *molecular basis* of the phenomena would seem to be the essential point to be considered.

WHAT IS MORPHOLOGY?[1]

IF those of us who have laboured up the hill of life revert to the studies of our youth, I think we shall not remember to have heard our teachers speak of the "Morphology of Animals." I cannot remember when or where I first met with the word; although the idea itself with regard to plants, has been familiar to me for nearly forty years, that is, since the time when I became possessed of "Lindley's Introduction to Botany;" but he used the term "Organography." The term "Morphology" was used by Schleiden in his "Principles of Scientific Botany" at least thirty years ago; and I may say in passing that the study of that work was one of the best preparations I received for the work I have undertaken since.

A comparison of the mode in which both plants and animals are developed was suggested to me, if not for the first time, yet then with new force, by reading Johann Müller's "Physiology of Man;" especially in the part on Generation, and more especially in his statement of, and criticisms upon, Caspar J. Wolff's "Theory of Generation," which was published at Halle in 1759. The very mention of this date is interesting, for this is evidently the time, and this work of Wolff's was surely the work, which suggested to the great, rich mind of Goethe the idea of an underlying unity amid all the diversity of vegetable and animal forms. How fruitful this conception of the simplicity and unity of vegetable and animal patterns has been, I need not tell you; for more than a century it has been yielding precious and ever increasing results. It was natural, therefore, that a division of biology so new and so fascinating, should gain for itself a name: and as naturalists had been from time immemorial familiar with the *metamorphosis* of certain types, the term "morphology" which especially treats of such changes in the individual life-history of a plant or of an animal, was natural, easy, and appropriate.

The *à priori* dreams which made the study of vertebrate morphology appear transcendental, and indeed gave it that title as a cognomen, caused great loss of time and of talent: and if Prof. Huxley had done nothing else whatever than dispel the glamour of these dreams, he would have deserved well of his age. His "Croonian Lecture," delivered at the Royal Society about twenty years ago, was as "a trumpet that gives a certain sound;" the dreamers awoke from their dreams, and became the workers, who since that time have wrought with labour and travail night and day. But the science of morphology, which had become an elegant pastime here, had long before Prof. Huxley's time found a noble band of workers in Germany; from that land came the dream; in that land arose the workers; the labours of Rathke, von Baer, and Reichert were ready to the hand of our biological reformer. After these, who were the chiefs of the band, came others, all men of name and renown; "but they attained not to the first three."

My own indebtedness is primarily to Johann Müller, who in his "Physiology of Man," already referred to, gave such an excellent abstract of the labours of the embryologists, his countrymen. I ought not to forget his lamented translator, Dr. Baly; for in the original Müller's work was a sealed book to me, and indeed would be now.

The fact that all organic beings pass through various stages, and run a certain round of life, is now becoming

[1] The first of a course of lectures "On the Morphology of the Batrachia," delivered at the Royal College of Surgeons, by Prof. W. K. Parker, F.R.S.

generally known. In the midst of the very beginnings of life the unspeakably minute monads, as the beautiful researches of Dallinger and Drysdale show, pass through several stages in their individual life-history. All the intervening living forms, between the monad and the man pass through several stages. The "Seven Ages" attributed by the poet to man are preceded by twice seven stages.

In all times the insects showed the wonderful working of the morphological force; the poets noticed these facts and sang of them; the philosophers, also, and reasoned upon them; but it was left for us to learn that these facts are not unique, but universal. Nevertheless, "the bee who is small amongst those that fly, and yet her fruit is the chief of sweet things," and that still smaller creature, the wise-hearted ant, architect, soldier, and lawgiver; these, and the other members of the insect-class, are metamorphosed *openly*. So, also, are the amphibia among the vertebrates, for instance, the frog and the newt, whose changes of form are so familiar to us. Still, for the most part, in the vertebrata "these things *are* done in a corner;" their most important changes of form are hidden from unassisted vision; to search out those secrets is the work of the morphologist.

Here, however, I will let "that old man eloquent"—Lord Bacon—speak for me; he says that Solomon, who was a great example with him, did "compile a Natural History of all verdure, from the cedar upon the mountain to the moss upon the wall (which is but a rudiment between putrefaction and an herb), and also of all things that breathe or move. Nay, the same Solomon, the king, although he excelled in the glory of treasure and magnificent buildings, of shipping and navigation, of service and attendance, of fame and renown, and the like, yet he maketh no claim to any of those glories, but only to the glory of inquisition of truth; for so he saith expressly, 'The glory of God is to conceal a thing, but the glory of the king is to find it out;' as if, according to the innocent play of children, the Divine Majesty took delight to hide his works, to the end to have them found out; and as if kings could not obtain a greater honour than to be God's playfellows in that game; considering the great commandment of wits and means, whereby nothing needeth to be hidden from them."

It seems to us now a little thing for a great mind meditating upon the form of a vertebrated animal to think that the axial structures should pass into the skull, when the main nervous axis so manifestly expands to become the brain. Yet men were held in bondage from generation to generation by the force of mere teleological ideas, that do but as Bacon expresses it, "Slug and stay the ship from sailing." In one place he compares people who will have all these meanings and ends of things at any cost, and who cannot bear to look at things in the "dry light" of their efficient causes, to those low and sensual people of whom one reads in holy writ, who accounted the manna as poor, thin diet, and clamoured for the onions, the leak, and the garlic, that flavoured the flesh-pots of Egypt. Now, however, the study of structures, according to their mere uses, and the imagining of ideal exemplars, these modes, the one imperfect and the other illusory, are giving place to the observation of the rise and progress in life of living creatures.

This rise and progress may be traced *gradationally*; which is a tracing of form after form in the adult animals existing at the present time; a most profitable study surely. To this has been added (within the last century almost) the investigation of forms that have become extinct; here, in "palæontology," we come athwart forms that are lower in type than their nearest relatives now living. Lower, and more generalised are they: and thus the mind is led to look towards the causes that have operated in the extinction of the old, rough, archaic forms, and the production or creation of the "lovely living things" that now adorn the earth. These are very often smaller, and, as a rule, more specialised in all respects, beautified, refined, and elevated in type beyond anything that could have been seen in their predecessors or progenitors. But that which both the gradationalist and palæontologist want, is a knowledge of the *development* of the types, their life-history, indeed.

Here is the work, this is the labour! Our immediate fathers began it; we have entered into their labours; but our children's children will have their hands full, not for one, but for many generations. Were this done, could we describe in detail the rise and progress of every part, and of every organ in the structure of any form in the genera, families, orders, classes, and sub-kingdoms of the animal kingdom; we might then come to some conclusion as to the relations of these various forms, and make some safe guesses as to how they have arisen. Nevertheless, if we cannot do all, that is no reason why we should do nothing, and stand as men who cannot find their hands; the light is breaking in upon us already; albeit, the work has but just been begun. The relations of living forms to each other—even in the adults—and the relations of extinct to living types; these flowers of science are opening and displaying their beauties to patient observers. We are now not merely considering the relations of the various vertebrate classes to each other, or of the various articulate, or molluscous, or radiated classes, within their own special circle; but embryology is leading us to the origin, as it were, of each great primary group, and of the branching off, so to speak, of each great group from some common stock.

However admirable in form and action *man* now is, he will soon, as a *vertebrate*, be ready to call the worm his sister and his mother; for his group is being set side by side with the worm-group—with the living forms from which sprang the "poor beetle," and the labouring ant. Indeed, as seven cities claimed Homer, so several *invertebrate* stocks now claim to have given birth to the noble *vertebrata*. The noisiest claimants are the *worm* and the *ascidian*—that poor relation of the oyster; by some this is thought to be madness, but there is method in it. I will now quote part of an article which appeared in the *Nineteenth Century* for December last, on "Recent Science." The writer is giving an account of Prof. Reichenbach's beautiful researches into the embryology of the common freshwater cray-fish, and then he goes on to compare the development of the nervous axis both in the invertebrata and vertebrata.

"Until quite recently the manner in which the central nervous system arises has always been considered as one of the most important distinctions between vertebrate and invertebrate animals. In the former, at the period when the embryo is a small three-layered patch on the surface of the egg, a longitudinal groove appears, the side walls of which, meeting above, inclose a tube lined by the epiblast. From the epiblastic cells thus shut off, the whole brain and spinal cord are produced, together with the roots of the cranial and spinal nerves, as the recent observations of Mr. Balfour [1] and Dr. Marshall [2] have shown. In the invertebrata, on the other hand, it was always supposed that the nerve-cord was produced from the middle layer of the embryo, or mesoblast; but this has been shown not to be the case, for it has now been proved that, in many of these, the nervous system arises from a thickening of epiblast, which only differs from the corresponding structure in vertebrata by the fact that it is not sunk in a groove. But the relation, in this respect, of the two great groups of the animal kingdom has never been more clearly brought out than in Reichenbach's [3] paper. He shows not only that the nerve-cord is a product of the epiblast, but that it arises from the cells lining an actual

[1] "On the Development of the Fresh-water Crayfish." (Die Embryonanlage und erste Entwickelung des Flusskrebses.") *Zeitscrift für wiss. Zool.* xxix., Bd. 2, Heft, July, 1877.
[2] *Phil. Trans.*, vol. clxvi., and *Journ. of Anat.*, April, 1877.
[3] *Journ. of Anat. and Phys.*, April, 1877.

groove—a groove having precisely the same relations, and in one part of its course being nearly as deep, as the 'medullary groove' of a chick or a tadpole. He also shows that the eyes are formed not, as is usually stated, as elevations, but as depressions in the epiblast; the cells lining these depressions becoming connected with those of the first ganglion of the nerve-cord. Here again is a remarkable resemblance to vertebrates, in which the organs of the higher senses always originate as involutions of the surface-layer" (page 896).

I have thus passed insensibly from the meaning to the aims of morphology. I trust you will agree with me that it is "a topmost fruitful bough" of the great tree of modern science; it is certainly fuller of buds than of flowers, for now is its early spring only. Kindly attend whilst I open a bud or two to show you what the flowers promise to be.

The ends and aims of *morphology* are different from those of *physiology; histology* may be said to be equally related to each and ancillary to both. The study of one branch seems to ask in its workers for an innate fitness for the one rather than for the other. One man sharply questions the *why* of nature; the other patiently searches after the *how*. *Morphology* asks for one who can work and wait in silence year after year; and his qualities have need to be those of quick insight, combined with the most phlegmatic laboriousness. Here, in this case, *natural* qualifications are of more importance than those which can be acquired. But the physiologist sharply asking *why* needs to be trained for his work; he must be a mathematician and a chemist as well as an anatomist; ready action and cunning inventiveness are most needed in him; a seeing eye, a copying hand, and a somewhat imaginative nature; these are the qualifications asked for in the morphologist. Delight in living forms and their transformations shows itself very early in us all; morphology is *æsthetic* before it is *scientific;* it becomes *scientific* as soon as it is *comparative*. The morphologist is nothing if not comparative; the development of accurate observation, combined with ready and constant comparison and unconscious classification—these are the necessary elements in the morphological worker.

The group of animals to which we belong—the vertebrata—considered as to their skeletal morphology, form alone a wide field; "there is yet much land to be possessed." In that division of a subdivided science I have chosen for time and for work's sake mainly the head; for in it are to be found the most intricate interweavings, the hardest knots of nature. For a time, for work's sake, one kind of head is enough; if all the parts are to be considered in their origin and relations, in their changes and development. For the solid and supporting parts of the *building*, so to speak, are to no purpose, have no meaning, if we could possibly forget their contents and their outgoing and overlying parts.

Considering the great complexity of structure in the highest types, the mind casts about to see if there be no similar forms of living creatures in which the structural problems are simpler. As man does not stand alone, but is merely—in respect of his lower nature—one of a large series of living forms, something, surely, may be learned of him, collaterally, and from below, by seeking what may be seen in the types that come nearest to him. Feeling our way down among the branches of the great vertebrate life-tree, we come to forms somewhat simpler, indeed, but formed on the whole on the same pattern, and having on the whole the same mode of embryogeny, and no real break occurs, even among living types, until we have passed the lamprey and his companions. Searching downwards, however, from any culminating type of mammal, we shall come to no form directly underlying them until we are among aquatic creatures; the birds, lying over the reptiles, belong to another "leader" in the life-tree.

Do but consider what a manufactory, what a laboratory, what a temple (if I may so speak) the head is! Yet it and all the body, of which it is the chief part, is developed vegetatively—its *growth* is as the growth of a plant, but its *architecture* stains the pride of all the glory of human skill. Man, not *structurally* only, but *socially*, also, is both husbandry and a building. And as the forces that bind the units of society together are the same as those that perfect the individual as such; so, also, is it in that which enclothes man and brings him into conscious relation to his fellows. The forces that work in the elementary parts are the same as those that work in the whole to make it one whole. The body is compacted together by that which every cell, every tissue, and every organ supplies; "according to the effectual working in the measure of every part" does it live, grow, and build up itself, and perform its wondrous and inimitable functions.

For a century past the thinking mind has been gradually trained to consider the earth, which is our temporary home, as a *development*, as being in a state now very different from that which it had at first, as having undergone, not one, but a thousand changes. Every one, now, knows that the earth did not "rise like an exhalation," and immediately assume its present form, wear its present robes, carry its present living forms; but that, during *Eonian days*—immeasurable secular periods—the face of the earth has changed as much as the face of a man changes during the "seven ages of his eventful history." It will take some time to bring the mind face to face with *our* facts; the thinkers as well as the unthinking will be slow in parting with the old cherished idea of the sudden apparition of a perfect man upon the earth, and the more because this *seems* to be the teaching of the most venerable records of history; which, indeed, ought to be *sacred* to us, if for no other reason than their undoubted antiquity. Those most venerable records have not suffered now that we get a Pisgah-view of the earth's development; they will not suffer from any doctrine of the slow development of man.

I had to speak of the *aims* of morphology; its highest aims are to be able to read off the archaic writings in which the members of man were in olden times written; to decipher the first promise and prophecy of his organic life in its initial letters up to the characters that express the form of the jointed worm, and to see the form of the jointed worm exalted into the fulness of the form of man. Yet we know of nothing but the sequences and results of the morphological force; we know absolutely as much of the nature of the human soul as of the nature of protoplasm, and nothing of either. The morphologist, as such, for the time, is like Gallio, he "careth for none of these things;" he refuses to be hindered with side-questions, however grave and important; his motto is, "this one thing I do." His work is to trace the *germ* into the *adult* or *germ-grower;* to scale every stage and step of a living creature's life; to map out each form, passing into succeeding forms, until the perfect form appears.

The ladder of man's life reaches up to the highest heaven of organic beauty; that of the horse, the ox, and the lion stops far short of this height; yet are they all perfect after their kind. You will see at once that man is an animal *plus* something that has made it possible for him to become "in form and moving so express and admirable; the beauty of the world, the paragon of animals!" Prof. Flower will show you what a poor thing man is when that which makes him *man* is arrested or suppressed; you will then "look on this picture and on this," on man in his highest development; his outward form corresponding to the power and excellence within; and on man undeveloped, brutal, foul in face, and fouler still in life.

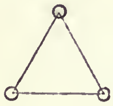
W. K. PARKER

L 2

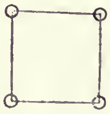
FLOATING MAGNETS

THE publication of my experiments on "Floating Magnets," in the *American Journal of Science* and in NATURE, was made merely as a claim to this new method of experimenting. I now send you the law of the morphology of their configurations, and show how these experiments illustrate the phenomena of allotropy,

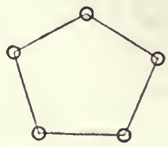
1*a*



3



4

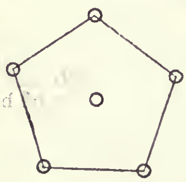


5*a*

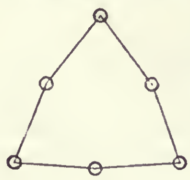
isomerism, expansion or solidification of water, bismuth antimony, &c., the atomic hypothesis, and the kinetic theory of gases.

The configurations of the floating magnets given in this paper are reduced to half-size. They were obtained as follows:—A cylindrical magnet, 387 millimetres long

5*b*



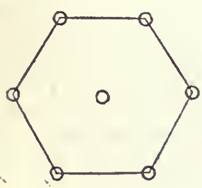
6*a*



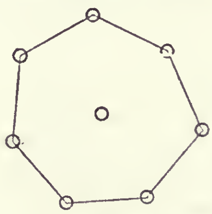
6*b*

and thirteen millimetres in diameter, was clamped with its lower end sixty millimetres above the plane in which were the ends of the floating magnets.

After each configuration was formed the tips of the needles were dotted with printer's ink, and a flat piece of cardboard was carefully lowered on to the configura-

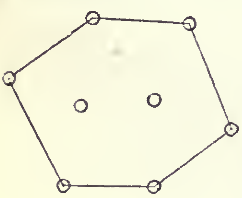


7

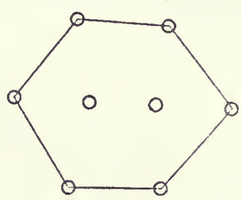


8*a*

tion, which was thus printed on the card. The points formed in this way were placed on drawing-paper, and the imprinted points were pierced with a needle. Thus the centres of the magnets were located, and around these points were drawn the circles of the element of the configurations. The configurations here given are



8*b*



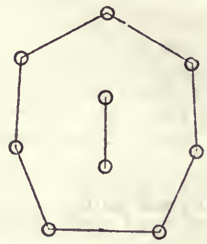
8*c*

one-half the size of the prints taken from nature, "with all their imperfections on their heads," produced by the unavoidable unequal magnetization of the component needles.

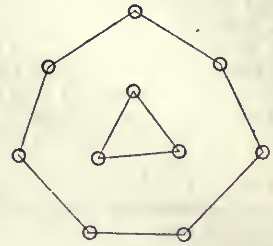
These configurations are numbered from 2 up to 18 *b*; the numbers indicating the numbers of floating magnets in the configurations. Where *a*, *b*, and *c* occur under a configuration they show the order of their stability. Thus 5 *a* is more stable than 5 *b* and 6 *a* than 6 *b*.

The law of the morphology of these forms is as follows:—They are divided into primaries, secondaries, tertiaries, &c. The primary configurations are from 2 up to 9 *a*.

The secondaries begin with 9 (one might even say with *b* and *c* of 8). These secondary configurations have the



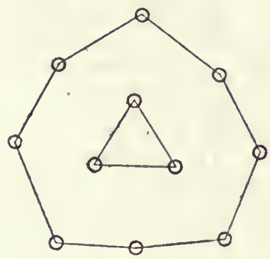
9



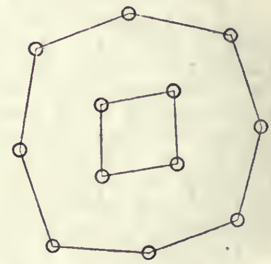
10

stable primaries for *nuclei*. Thus configurations 9, 10, 11, 12, 13, 14, 15, 16, 17, 18*a*, and 18*b*, have respectively 2, 3, 3, 4, 4, 5 (flattened), 6*a* (which is "5 flattened" expanded to a regular pentagon), 7, *pointed* (*compressed?*) towards a vertex of the hexagon, 7, 7, 8.

Nineteen needles form the first configuration of the tertiaries. This is formed of 9 as nucleus, surrounded by 10 floating magnets.

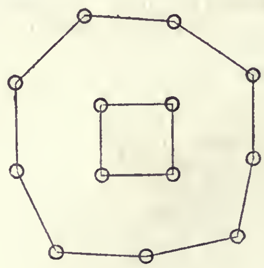


11



12

Twenty has 9 for nucleus, with 11 circumposed; but this form is unstable, and soon changes into Fig. 20, which has 10 magnets for nucleus with 10 circumposed. This is the only instance (except the flattened pentagon, Fig. 14) I have found where a nucleus is changed in form by the action of the circumposed magnets. This nucleus of 20 cannot be formed without the circumposed magnets, as in Fig. 20.



13

Twenty-two has 11 for nucleus, surrounded with 11 magnets.

Twenty-three has 11 for nucleus, with 12 circumposed needles, arranged parallel to nucleus.

Twenty-four is formed of 11 for nucleus, surrounded with 12, and *one* opposite the base of ∴.

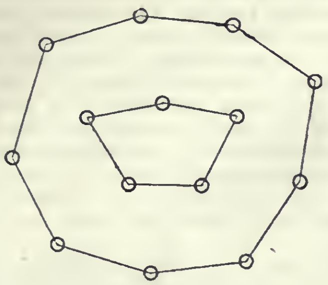
Twenty-five is formed of 13 for nucleus, with 12 circumposed, and parallel to nucleus.

Twenty-six is formed of 14 for nucleus, with 12 magnets circumposed.

Twenty-seven is formed of 15 for nucleus, with 13 magnets circumposed.

Twenty-eight is formed of 14 for nucleus with 13 circumposed.

Twenty-nine is formed of 16 for nucleus, with 13 circumscribed.

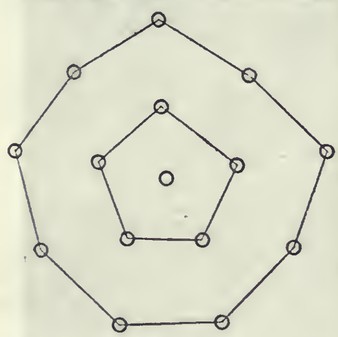


14

Thirty is formed of 17 for nucleus, with 13 circumscribed.

Thirty-one is formed of 18 for nucleus, with 13 circumscribed.

Thirty-two begins the *Quaternary Configurations*, for it is formed of 19, with 13 circumposed magnets.

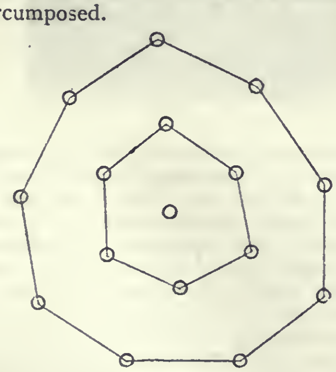


15

Thirty-three is formed of 20 for nucleus, with 13 magnets circumposed.

Thirty-four is formed of 21 for nucleus, with 13 magnets circumposed.

Thirty-five is formed of 22 feet for nucleus, with 13 magnets circumposed.



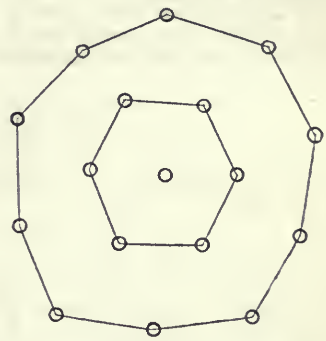
16

Thirty-six is formed of 23 for nucleus, with 13 magnets circumposed.

Thirty-seven is formed of 24 for nucleus, with 13 circumscribed magnets.

Thirty-eight is formed of 26 with 12 magnets circumscribed.

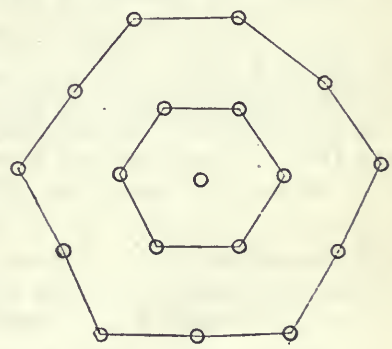
The expansion of liquids, like ice and antimony, on solidification, allotropy and isomerism, are illustrated by the fact that different configurations formed of the same number of atoms have densities inversely as their areas.



17

Thus 5*b* is about $\frac{1}{10}$th greater in area than 5*a*. So if 5*a* represent water at 0° C., 5*b* may stand for ice at 0° C.

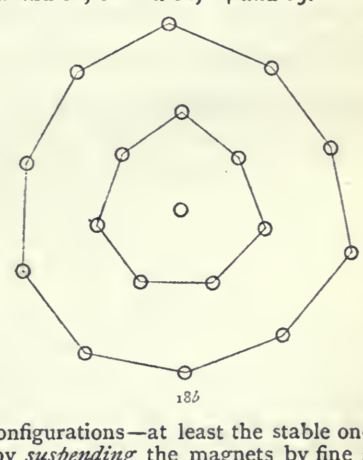
Similarly in allotropy if 6*a* stand for graphite, then 6*b* may stand for diamond, and the three forms of titanic oxide, rutile, brookite, and anatase, and their different densities may be illustrated by configurations 1*a*, 8*b*, 8*c*.



18*a*

If 6*b* stand for calcite, then 6*a* will stand for its isomer anagonite.

The law ruling the density of the configuration is evidently that a central magnet always expands the contours of the configuration. For example, compare Figs. 4 and 5*b*, 5*a* and 6*a*, 6*b* and 6*a*, 14 and 15.

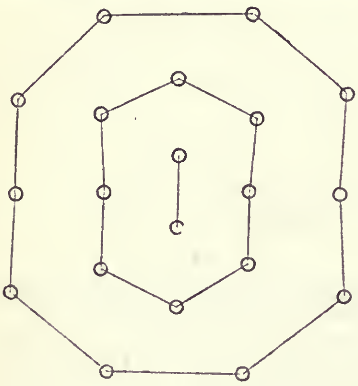


18*b*

These configurations—at least the stable ones—can be obtained by *suspending* the magnets by fine silk fibres. I have thus obtained all the stable forms; and the plan proposed to me by my friend Prof. Rood will no doubt give these configurations. He proposed to me to suspend

gilt pith balls by silk fibres and then electrify them with the same electricity.

If *suspended* configurations be brought near each other we will cause the vibrations of their component magnets (atoms), and thus we may illustrate the atomic vibrations in molecules. If a suspended configuration be brought in contact with a piece of paper, supported vertically, the interaction of the suspended magnet may force it from the



20

vertical, and cause it to fall, and thus may be illustrated the molecular pressure of gases.

I will here point out the stable and unstable configurations. $5a$ is more stable than $5b$, and $6a$ is more stable than $6b$. The latter is sent into $6a$ on vibrating it. $8c$ is very unstable (like . . . .), and goes into $8b$ on vibration, caused by elevating and lowering the superposed magnet.

A. M. MAYER

P.S.[1]—As to the configuration . . . . it is so *very* unstable that I have not reproduced it in these configurations, for it is really *too unstable* to exist except for *an instant*.

The hexagon only exists with a central magnet. Mr. C. S. Pierce and I have had several discussions about the stability of . . . . . . I always have maintained that it was *impossible* to get this form, for a central repellant body was necessary to the tension of the . . . . . . which is like a soap bubble *without* cohesion of contiguous elements. Seven magnets form *only* . . . .

. . . . is more stable than . . . . . .

[1] Addressed to Sir Wm. Thomson.

ON A REMARKABLE FLASH OF LIGHTNING[2]

On the evening of August 16 last year (1877) a heavy thunderstorm took place in this vicinity (Southport). It was preceded by a fall of the barometer not exceeding one-tenth of an inch, the wind at 1 o'clock P.M. being west, backing gradually until at 9 o'clock P.M. it was south. At the time of the storm to which my present observations refer it was south-west, and consequently its direction was nearly parallel with the coast line.

I was standing at the shore-end of Leicester Street, watching the approach of the storm, and observing the progress and direction of the more important flashes, when about 8 o'clock a vivid flash of lightning fell apparently into the channel (the water being not much above low water mark of a neap tide) about one-sixth of a mile north of the end of the pier. In about a minute afterwards another fell about one-sixth of a mile north-east of the previous one, and after a similar interval a third stream of electricity descended about another one-sixth of a mile in the same direction. The first and third flashes were of the usual character of forked lightning, but the second presented an appearance which I do not recollect to have witnessed before. From its exit from the clouds to its fall into the sea it seemed composed of small detached fragments which caused it to assume the aspect depicted below.

On the following day, in the course of a conversation respecting the storm of the previous evening, I mentioned the phenomenon to Mr. Thistlethwaite, who informed me that he had been particularly struck by the extraordinary

appearance of this singular flash, which he had observed whilst sitting in the "parsonage" (the house adjoining the south-west side of the Manchester and Liverpool District Bank), and which to him appeared exactly as I have depicted it. This gentleman could, however, have seen the upper portion of the flash only, as the houses in Lord Street and on the Promenade intervened between his point of observation and the shore.

Heavy rain seemed to follow in the wake of the third flash, and came on with a noise like that of a great rush of wind, but as the direction of the storm was nearly coincident with the water-line, inclining but slightly towards the beach, about ten minutes elapsed before the downfall reached the place where I stood.

From the information I afterwards obtained the thunderstorm was subsequently, a few miles to the north-east of Southport, more severe than it was in Southport itself.

Southport, March 11 B. ST. J. B. JOULE

[2] Paper by B. St. J. B. Joule, at the Lit. and Phil. Soc., Manchester.

OUR ASTRONOMICAL COLUMN

CACCIATORE'S SUPPOSED PLANET OF 1835.—It might have been expected that long ere this, if the object twice observed at Palermo in May, 1835, were really a planet, it would have been recovered by one or other of the astronomers who have occupied themselves with the examination of the ecliptical region of the sky.

The particulars of the Palermo observations were communicated by Cacciatore to Valz in a letter dated September 19, 1836, and at an earlier period to the late Admiral Smyth, as will be known to readers of the "Cycle of Celestial Objects." Valz sent a copy of Cacciatore's letter to Schumacher, who published it in No. 600 of the *Astronomische Nachrichten*. When observing the star 503 of Mayer's catalogue with the Ramsden circle, on May 11, 1835, it was noted down that a smaller star of the eighth magnitude followed Mayer's star two seconds of time, and was about 2½′ to the south. Such entries were frequently made by Piazzi, when observing with the same instrument, as may be seen from his catalogue, but although No. 503 occurs there, no mention is made of a star near it. On the next fine night, May 14, observing Mayer's star again, the assistant, according to custom, read out the note made on May 11: "Seguita da una altra di 8 per 2″ circa di A.R. circa 2½′ al sud." No star was then visible in this position even in a dark field, but one of the eighth magnitude *preceded* Mayer's star nine seconds of time, only 1⅓′ to the south. Cacciatore says he intended to repeat the observation on the following evening, the weather promising to continue fine. Returning to the library he found that no one of the four small planets known at that time was in the observed position, and he appears to have considered the object either a planet beyond Uranus or a comet, remarking: "Onde con impazienza attendeva il dimani." But the night of May 15 proved unfavourable, rain setting in, followed by clouded skies for upwards of a fortnight, and not until June 2 could an observation be attempted, "Ma la stella era involta nel crepusculo feci varj tentativi fuori del méridiano, non transcurai ogni mezzo per riconoscere la mia osservazione." Cacciatore says his assistants were unsuccessful on other evenings to the end of June. The search was repeated in the first five months of 1836, but to no purpose.

Valz first showed that a body with the observed positions on May 11 and 14, could not be a distant planet, as Cacciatore had conjectured, but rather a pretty near member of the minor-planet group, which, on the hypothesis of a circular orbit, might have a period of revolution of about three years, with the ascending node of the orbit in longitude 339° 36′ and an inclination of 3° 22′ to the plane of the ecliptic. In 1849 Dr. Luther repeated the calculation with the following results:—Radius of orbit, 2·1055; ascending node, 343° 20′; inclination, 3° 37′; period, 1,116 days; and from these elements Oeltzen computed a *zodiac* for the planet, or a table indicating with right ascension as argument, the northern and southern limits of declination (*Astron. Nach.*, No. 662). It is certain that any determination of the position of the orbit from Cacciatore's data must be open to considerable uncertainty, and hence a search for his supposed planet amongst the one hundred and eighty-eight planets now discovered would not be decisive one way or the other if confined to similarity in the position of the nodes and the inclination; places must be calculated for the epoch of Cacciatore's observation for such planets as could by possibility pass near Mayer's star. An attempt in this direction has failed to identify the object. That a minor planet which so far from opposition attains the brightness of stars of the eighth magnitude can still remain unknown to us is, to say the least, very improbable. Must we leave Cacciatore's star in the same category as those reported to have been observed by Huth in 1801 and Reissig in 1803, to which reference has been made in this column?

THE TOTAL SOLAR ECLIPSE OF 1883, MAY 6.—In continuation of our notices of the total eclipses of the sun during the remainder of the present century, we present the elements of the eclipse of May 6, 1883:—

G.M.T. of conjunction in R.A., May 6, at 9h. 44m. 42s.

	° ′ ″
R.A.	43 30 52·2
Moon's hourly motion in R.A.	38 22·6
Sun's ,, ,, ...	2 25·0
Moon's declination	16 11 32·2 N.
Sun's ,, ...	16 37 52·5 N.
Moon's hourly motion in declination ...	7 26·2 N.
Sun's ,, ,, ,, ...	0 41·9 N.
Moon's horizontal parallax	60 52·0
Sun's ,, ,, ...	8·8
Moon's true semi-diameter	16 35·2
Sun's ,, ,, ...	15 51·0

The central and total eclipse begins in longitude 156° 1′ E., latitude 34° 43′ S., and ends in 86° 44′ W. and 13° 41′ S., and the central eclipse occurs with the sun on the meridian in 147° 4′ W., and 9° 11′ S. The following are also points upon the central line:—

Long. ° ′	Lat. ° ′	Long. ° ′	Lat. ° ′
179 51 E.	25 43 S.	137 44 W.	6 24 S.
168 19 W.	19 52	119 52 W.	5 51
160 49 W.	15 49	108 12 W.	7 51 S.
140 51 W.	7 7 S.		

The path of the eclipse is almost wholly a sea-track, and the only probable region for obtaining observations of any value will be in the Marquesan longitudes. A direct calculation for the island Fetou-houhou or Chanel Island, with the position of the Admiralty chart, gives for commencement of totality 0h. 42m. 3s. local mean time, and duration of the total phase 2m. 53s. The following are the limits of the zone of totality about the Marquesas group:—

Longitude W. °	South limit, Latitude. ° ′	North limit, Latitude. ° ′
141	8 11	6 2
140	7 55	5 46
139	7 41	5 31
138	7 25	5 16

NOTES

DR. SCHUSTER, the leader of the English Government Eclipse Expedition to Siam, in 1875, sails in the White Star Line ship *Germanic* to-day to observe the eclipse of the 29th instant. Prof. Thorpe, F.R.S., accompanies him on the same errand, and will make magnetic observations over a great portion of his route. Mr. Norman Lockyer intends to sail in the *Baltic* on the 9th instant. We learn that the appropriation made by the American Government is so small that, in strange contrast to what has happened in the case of all English Expeditions since 1870, no facilities can be offered officially to observers from other countries. Still we doubt not that they will receive both welcome and aid from their *confrères*.

CAPT. MOUCHEZ has been appointed Director of the Paris Observatory. A sub-director has also been appointed, but contradictory reports have reached us as to who has been selected.

A LARGE number of foreign men of science have promised to be present at the Dublin meeting of the British Association; among the names mentioned at the last meeting of the local committee are Professors Sachs, Würzburg; C. Pierce, New York; S. H. Scudder, Cambridge, Mass.; A. S. Packard, Salem, Mass.; and Karl Koch, Berlin. The programme of excursions will be finally settled at the next meeting of the committee. Visits to almost every place of interest within easy distance of the city will be arranged for, and the

usually vexing questions of locomotion and commissariat carefully attended to. The gentlemen of the Excursion Committee are sparing neither time nor trouble to work out the exceedingly difficult task they have undertaken in a thoroughly satisfactory manner. A report from Professors M'Nab and Macalister, editors of the "Guide Book," was read, and shows that the little volume will be a most interesting one. It will consist of sixteen parts, embracing every subject of scientific interest, and will have the following maps:—The six-inch map of the City of Dublin, the ten mile to the inch map of the province of Leinster, and a geologically coloured map of the country, on a scale of a quarter of an inch to the mile. The maps are being prepared under the direction of Major Wilkinson, chief of the Ordnance Survey in Ireland.

AMONG the excursions arranged for in connection with the approaching Paris meeting of the French Association are the following: On August 24, to Fécamp and Etretat; 28th to Tancarville Château, the Roman remains at Lillebonne, and the manufactures of Bolbec; on the 31st to Rouen, returning by steamer to Paris.

THE following is a list of the scientific conferences still to be held in connection with the Paris Exhibition:—Demography, July 5–9; Ethnography, July 15–17; Means of Transport, July 22–27; Hygiene, August 1–10; Civil Engineering, August 5–14; Anthropology, August 15–21; Commerce and Industry, August 16–22; Meteorology, August 24–28; Geology, September 2–4.

THE distribution of the medals of the Paris Geographical Society at the Sorbonne on Friday was witnessed by an immense crowd which had gathered to hear Mr. Stanley, who was received with tremendous enthusiasm. His address, in which he carefully expounded the state of African exploration when he began his work, was delivered in English, and a translation read in French by M. Maunoir, the general secretary of the Society. The meeting was presided over by Admiral La Roncière le Nourry, who spoke in English when he handed over the gold medal to the great African explorer. Stanley returned thanks in English. These two addresses were not translated as it became evident a large number of the audience understood the proceedings. The other medals to M. Vivien De St. Martin and Dr. Harmand were then delivered.

AT the Paris *fête* of June 30 the part played by electricity was smaller than anticipated. The number of electric lamps was large, but the effect not particularly good. The Jablokhoff candles, although superior to gas-lamps, were not sufficient to overcome all the mass of light which surrounded them. A large number of Bunsen elements had been put into requisition, but the regulators were wanting in regularity, and much of the effect was consequently lost. Competent persons say that the result would not have been so poor if previous successes had not raised too ambitious expectations in the public and too much confidence in the operator.

THE position in the physical section of the French Academy of Sciences, rendered vacant by the death of Becquerel in January last, has been filled by the election of M. A. Cornu. The recipient of this honour is best known by his investigations into such fundamenta as the density of the earth and the velocity of light. Among his other researches of more recent date we might mention those "On the Experimental Determination of the Principal Elements in an Optical System," and "On the Optical Polarisation due to the Reflexion on the Surface of Transparent Bodies." M. Lecoq de Boisbaudran has been elected a Corresponding Member in place of the late M. Malaguti.

PROF. A. W. HOFMANN, of Berlin, has passed through a severe attack of fever during the past month, and, although now out of danger, will be for some time unable to fulfil the active duties of his position. A handsome *brochure* has lately appeared, commemorating the grand *commers* given in honour of his sixtieth birthday, by the students of Berlin, last March.

AT the general meeting of the Scottish Meteorological Society to-morrow Dr. Mitchell and Mr. Buchan will read a paper comparing the weather and health of New York with London; and Mr. Buchan another on the influence of the physical configuration on the seasonal distribution of the Scottish rainfall.

MR. EDWYN C. REED, well known to many English zoologists from the collections he has from time to time forwarded from Chile, and author of several papers on the entomology of that country, has left his post in the Museum of Santiago and accepted the appointment of Professor of Zoology in the "Liceo" of Valparaiso, and Director of its Museum. Mr. Reed sends us copies of two papers which he has recently published at Santiago, one on the Diurnal Lepidoptera of Chile, the other on the Mammals and Birds of the hacienda of Cauquenes, in the province of Colchagua. Both of these deserve the attention of European naturalists.

THE completion of Giffard's large captive balloon has been postponed owing to the bad weather which prevailed in Paris during the greater part of June, but the recent fine weather has enabled the works to be resumed and the balloon will be completed in a few days. On the occasion of the *fête* of June 30, a balloon of 17,000 cubic feet was sent up with two aeronauts. It was filled in forty minutes with hydrogen gas generated with M. Giffard's continuous apparatus, which contrivance is in perfect order, and will be used this week to fill, in about two days, the monster of 25,000 cubic feet.

THE Birmingham Natural History and Microscopical Society have decided to have this year again a marine excursion to Arran. Facilities will be afforded both for dredging excursions in Lamlash and Brodick Bays and elsewhere in the vicinity; and for land excursions to investigate the botany and highly interesting geology of the island. During the summer season a most interesting series of observations may be made on the microscopic larval forms of marine life (hydroids, echinoderms, crustaceans, annelids, &c.) which abound in the sea, and may at this time readily be taken by the tow net. The late season of the year when the previous excursions have been made precluded much attention to this most interesting branch of marine zoology, which will be specially studied on this occasion. Should a sufficient number join the excursion it is hoped that a small steamer may be chartered, which will economise time and add to personal comfort. The time for the excursion will be from about the 19th to the 27th of July, but the days will be positively fixed at a meeting of those who are desirous of joining the excursion. The expense will be very moderate, and the Birmingham Society deserve every credit for their enterprise.

THE committee who are charged with the arrangements for the celebration of the fiftieth anniversary of the opening of University College desire to make known that ladies will be admitted to the festival on the same terms as gentlemen. Many ladies have already obtained tickets, and others, who may desire to do so, will find full particulars in the advertisement now appearing in our columns. Among those who have accepted the invitation of the committee are the Earl and Countess of Granville, the Earl of Derby, the Earl of Northbrook, the Earl Fortescue, Lord and Lady Ebury, Lady Belper, Mr. and Mrs. Goschen, Sir John and Lady Lubbock, &c., &c.

"THE aforesaid Martin was one of those unfortunates who were at that time of day (and are, I fear, still) quite out of their places at a public school. If we knew how to use our boys, Martin would have been seized upon and educated as a natural

philosopher." So writes "Tom Brown." Our thoughts reverted to the above description whilst considering the contents of some recent numbers of the *University College School Magazine*. Our present purpose is not to give an account of this Magazine, which, we remark, appears at uncertain intervals, and contains articles of like character with those found in most similar school publications. Bur we desire to draw attention to the formation of a "scientific society" amongst the boys themselves. This was started in January, 1876. At first the members read quasi-original papers once a week, and then a discussion took place upon the same; in May of the present year it was resolved to have papers twice a week. At the outset a library was started, books being presented by the members, and in the Michaelmas term of 1877 a reading-room was opened for use between morning and afternoon school, four days in the week. Amongst the scientific papers taken in, we notice that NATURE heads the list. In the present year a museum has been started, and we give particulars, as doubtless some old U.C.S. boy may be able to add to it for "auld lang syne." It contains a collection of minerals, fossils, metals, &c.; specimens illustrative of economic botany; ethnological implements, weapons, coins, &c.; osteological specimens; marine shells; a Tennant's geological box (200 specimens); and a case of British birds' eggs. The working staff is composed partly of the boys and the terminal subscription is a shilling. The privilege of membership (subject to a ballot) is restricted to the fourth and higher science classes. It will be seen that it is yet the "day of small things" with the Society, but we predict for it considerable usefulness. Had such a society existed at Rugby in the days of "Martin," each would have been made for the other. Papers have been read on "Carbon Dioxide as a Motive Power," "Voltaic Electricity," "Frictional Electricity," "Electro-Magnetism," "Cyclones," the Barometer, the Thermometer, Comets, Coral, &c. The Society fosters study further by offering to its members every year three prizes, awarded at the annual distribution, for the best collection of natural objects and of microscopical objects, and for the best model of any scientific instrument—all these to be made or mounted by the exhibitor. We believe the usefulness of the Society might still further be increased could its council induce leading scientific men to deliver lectures which should be open to the friends of members and of the boys generally. We are sure the present head and vice-masters would lend their countenance to such a proceeding.

FRANCE was visited by an earthquake on June 25, which was felt at Lyons, Mâcon, Valence, Villefranche, and Châlons, The movement was from the east towards the west. The shock lasted half a minute, but caused no damage.

PROF. BURDON SANDERSON delivered the Harveian oration on June 26. He occupied himself with the researches by which, in the first half of the present century, the Harveian doctrine of the working of the circulatory apparatus was developed.

A FINE colossal bronze statue of Capt. Cook, by Mr. Woolner, R.A., which is at present placed in the open space between the United Service Club and the Athenæum, is intended for erection at Sydney, New South Wales.

THE Council of the Sanitary Institute of Great Britain have appointed a committee, consisting of His Grace the Duke of Northumberland, president, Mr. Edwin Chadwick, C.B., Dr. Richardson, F.R.S., and Dr. Lory Marsh, to represent the Sanitary Institute at the Congrès International d'Hygiène in Paris, from the 1st to the 10th of August next. During the congress in Paris the Société Française d'Hygiène, with which the Sanitary Institute is affiliated, will entertain at a banquet on August 9 the members of the Sanitary Institute and their other foreign associates.

WE have received from the Imperial College of Engineering, Tokei, Japan, the Calendar for Session 1877-8, and the reports of the professors for the period 1873-77. We have already referred to this admirably conducted institution so fully already (vol. xvi. p. 44), that we need only say now that these publications confirm all we have said about the college. It is based on the best continental models, and the course of instruction is so complete, thorough, scientific, and practical, that English engineering students who read the syllabus of instruction, the list of contents of the museum, the catalogue of the admirable library, and the professors' reports, will be inclined to wish that Japan were a little nearer home. The enthusiasm and discrimination which the Japanese have shown in adopting all the best characteristics of European civilisation and learning, are one of the wonders of the age; they have been specially fortunate in obtaining as principal of their Engineering College, so intelligent and accomplished a man as Mr. Henry Dyer has shown himself to be. Among the professors are Messrs. J. Perry and W. E. Ayrton, whose names must be known to many of our readers as the authors of valuable original papers in physics, sent by them occasionally to this journal and to the scientific societies.

MR. CONSUL LAYARD, of Noumea, sends to the *Colonies* particulars of extraordinary volcanic eruptions at the island of Tanna in the beginning of the year. The first eruption took place on January 10 last, about 10 A.M. The bottom of the harbour at the west side rose above water for about fifty fathoms length at the first earthquake shock. A new volcano burst out close to "Sulphur Bay," between it and the old volcano. The west side of Port Resolution was all bursting up with steam. A second great eruption and earthquake took place on February 11, and the bottom of the harbour was again upheaved for about another fifty fathoms, making the entrance of the harbour very narrow. Three rocks rose up about a cable's length from the west point to seaward out of eleven fathoms water. There is now a bar with only fifteen feet of water, where there used to be five or five and a half fathoms, right across the mouth of the harbour, a little inside. A tidal wave about fifty feet high swept the east point of the harbour, destroying all the native plantations. The wave occurred on both occasions, but the first was the biggest. The old mountain was very active, roaring and throwing up huge rocks. The tidal wave was very local; so was the shock. The missionary on the other side of the island hardly felt it, and there was no wave there. The natives say they never knew anything like it before. On the west side the earth has cracked and sunk very considerably; on the east side the land was swept by the tidal wave; the plantations on both sides are destroyed.

THE glaciers of the Western Himalayas, according to measurements recently given in the *Tour de Monde*, far surpass in extent any hitherto examined outside of the polar regions. In the Mustagh range, two glaciers immediately adjoining one another possess a united length of sixty-five miles. Another glacier in the neighbourhood is twenty-one miles in length, and from one to two miles in width. Its upper portion is at a height of 24,000 feet above the level of the sea, and its lower portion terminating in masses of ice 250 feet in height, and three miles in breadth, is 16,000 feet above the sea.

SOME interesting fossils have just been found near Holmestrand, on the Bay of Christiania, Norway. They consist of large quantities of dolphin bones and are imbedded in loam some three metres below the present surface, although more than forty-three metres above the level of the sea. Their surroundings are unquestionably of the most recent geological formation, and this discovery may serve as a proof that even during the latest geological period the coast of Southern Norway has risen at least forty-three metres. Not a single fossil of the pliocene or pleistocene has been found.

M. J. S. POLIAKOW, of the Russian Geographical Society, is about to commence his researches in the government of Vladimir and in Lithuania into the remains of the stone period. This expedition is in continuation of the labours of M. Poliakow, commenced more than ten years ago by the discovery of implements of stone in the plane of the river Irkout (1867). Later, in 1871, he found implements of the same kind in the government of Olonets, on the banks of Lakes Lago, Kenozero, &c.; in 1874 he found them also on the banks of the lakes of the upper basin of the Volga; and lastly, the journey which he undertook in 1876, in the valley of the Obi, convinced him not only of the existence of remains of the stone period in Western Siberia, but led him to seek the explanation of many stone implements among the implements of peoples possessing only a low degree of culture, as among the Ostiaks. This series of journeys has enabled M. Poliakow to form a very considerable collection of implements in stone and of curious data on the natural history of that epoch. Now, new discoveries have been made by other travellers in the districts of Mourom, Vladimir, and in Esthonia, on the banks of Lake Bourtnek, where a tumulus has been discovered containing remains of objects connected with cookery. All these discoveries have led M. Paliakow to request the Society to send him into the government of Vladimir and into Esthonia, to study upon the spot these new remains. What attracts the attention of M. Poliakow is that there are, among other things, proofs that, during the stone period, there existed in the small fresh-water lakes a species of seal recalling, by its dimensions and characteristics, the seal of Greenland and the Caspian. Another remarkable fact is the discovery made, along with the instruments of stone in the district of Mourom, remains of the mammoth. After having investigated the materials referred to, M. Poliakow proposes to visit Stockholm, Copenhagen, and other cities, to inspect the pre-historic museums, with a view to complete the materials he possesses for studying the stone period.

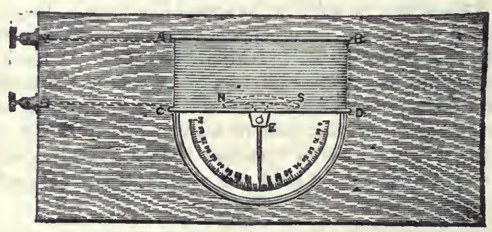
DR. SCHOMBURGK, in his Report on the Botanic Garden and Government Plantations of South Australia for 1877, gives an account of an interesting experiment he made with some Arctic wheat. He received a sample of wheat taken from a quantity left by the American Arctic Expedition ship *Polaris* in 1871, which had been abandoned in north latitude 81° 16′. This wheat had been left on the beach exposed to the snow and a temperature of 72° to 104° of frost for five years, and was found in a heap by Dr. Ninnis, of H.M. ship *Discovery*, on the return of the last Arctic Expedition to England. Dr. Schomburgk received 1,000 grains, of which he sowed about 300. From the 300 grains about sixty germinated. The plants grew healthy and reached to the height of from three to four feet. It is a bearded wheat, and ripened in the commencement of January. The ears contained about thirty grains each, which were but small, though round and plump. The birds unfortunately destroyed the greater part before it came to maturity, but the interesting fact proves the assertion that the grain of the cereals possesses a vitality not surpassed by any other seed.

THE additions to the Zoological Society's Gardens during the past week include a Japanese Wolf (*Canis hodophylax*) from Japan, presented by Mr. H. Heywood Jones; a Rhesus Monkey (*Macacus Erythræus*) from India, presented by Mrs. Walcot; a Brown Capuchin (*Cebus fatuellus*), a Crested Curassow (*Crax alector*) from Venezuela, presented by Mr. A. Warmington; a Mona Monkey (*Cercopithecus mona*) from West Africa, presented by Capt. C. F. Filliter; a Green Monkey (*Cercopithecus callitrichus*) from West Africa, presented by Mrs. George Yeomens; a Common Marmoset (*Hapale jacchus*) from South East Brazil, a Pinche Monkey (*Midas œdipus*) from New Granada, presented by Mr. Edward Clayton; a Bonnet Monkey (*Macacus radiatus*) from India, presented by Mr. Allen Forbes; a Short-Tailed Capromy (*Capromys brachyurus*) from Jamaica presented by the Hon. J. Burford Hancock; a Pine Marte, (*Martes abietum*) from Ireland, presented by Mr. Robert Walter; six Goldfinches (*Carduelis elegans*) British Isles, a Common Chameleon (*Chamæleon vulgaris*) from North Africa, presented by Mr. C. F. Johnson; four Cunningham's Skinks (*Egernia cunninghamii*) from Australia, presented by Mr. D. C. Pearson; a Garden's Night Heron (*Nycticorax gardeni*), a Common Boa (*Boa constrictor*) from South America, purchased; a Green-necked Peafowl (*Pavo spicifer*) bred in the Gardens.

A NEW GALVANOMETER FOR LECTURE PURPOSES

ALL who have had the experience of attempting to exhibit to a large audience the simple phenomena of dynamical electricity will bear testimony to the difficulty of rendering apparent over the whole of a lecture-theatre the movements of a galvanometer needle. When the galvanometer lies flat upon the table and the movements of the needle itself, or of the index attached to it are observed, the number of observers must be confined to those near at hand. Even the mirror galvanometer, indispensable as it is for delicate experiments, is open to the objection that a popular audience does not immediately appreciate the significance of the motions of the wandering spot of light. The devices for projecting the moving needle upon the screen have, up to the present time, been so large and inconvenient as to militate against their use for popular demonstration.

These facts led the writer some months ago to attempt to construct an instrument for projection upon the screen that should be within the size of an ordinary magic-lantern slide. The early attempts to do this were unpromising, and possessed little sensitivity even for considerable currents. In the latest form of the instrument, however, this defect has been overcome, and the galvanometer has in several trials before large popular audiences, as well as in the teaching of the lecture-room, shown itself to



answer with complete satisfaction the purposes for which it was designed.

In the most improved form, the galvanometer consists of a mahogany block the size and thickness of an ordinary magic lantern slider, which serves as a frame to contain the working parts. The coil of wire is wound upon a flat bobbin of brass or ivory, its ends being brought to a pair of terminals at the extremity of the slider. Within the coil the magnetised needle is suspended delicately by a horizontal axis between two adjustable screws. Attached at right angles to the needle is a light index of thin brass or of aluminium. The scale, which is transparent, is reduced by photography upon a glass plate. The arrangements are therefore on a small scale like those of a Becquerel's vertical galvanometer inverted. When no current passes, the index arm hangs downward, the centre of gravity being adjusted very little below the centre of suspension so as to secure the greater degree of sensitivity. But to correct for the dip when the plane of the instrument is nearly in the meridian a small compensating magnet may be placed upon the table below. Thus the slightest movement of the needle is at once made visible by the motion of the magnified image of the scale and index; and will be quite apparent even without lowering the lights of the room.

As the instrument may be constructed with either a short-coil or a long-coil, it can be applied to a variety of experimental uses. And its portability and simplicity exceed those of any galvanometer hitherto employed for purposes of demonstration.

SILVANUS P. THOMPSON

EARTHQUAKES ON THE PHILIPPINES IN THE YEAR 1876, *ACCORDING TO THE PUBLICATION OF THE "ATENEO MUNICIPAL" IN MANILA. COMMUNICATED BY DR. A. B. MEYER, DRESDEN.*

Date.	Hour.	Place.	Province or District.	Island.	Direction.	Remarks.
January 19	1 A.M.	Vigan	Ilocos sur	Luzon	—	—
February 7	4–5 A.M.	Iba	Zambales	,,	—	—
,, 10	7 A.M.	Laoag	Ilocos norte	,,	—	—
March 18	12.15 A.M.	Vigan	Ilocos sur	,,	—	—
,, 18	11–12 P.M.	Benguet	Benguet	,,	—	—
,, 21	5–6 A.M.	Batangas	Batangas	,,	—	—
,, 31	2.25 P.M.	Laoag	Ilocos norte	,,	N.–S.	—
April 14	2–3 A.M.	Iba	Zambales	,,	—	—
June 12	12.45 A.M.	Nueva-Cáceres	Camarines, S.	,,	—	—
,, 12	11–12 P.M.	Albay	Albay	,,	—	—
,, 18	? A.M.	Laoag	Ilocos norte	,,	N.–S.	—
July 13	10.30 P.M.	Iba	Zambales	,,	—	—
,, 13	,,	Cavite	Cavite	,,	N.–S.	—
,, 13	10–11 P.M.	Bulacan	Bulacan	,,	—	—
,, 13	10.34 P.M.	Manila	Manila	,,	N.N.E.–S.S.W.	Intensity 3°, sismometer 2 mm.
,, 26	? A.M.	Tacloban	—	Leyte	—	Strong.
,, 26	4 A.M.	Surigao	—	Mindanao	E.–W.	—
,, 27	10 A.M.	Baler	Com. P. M. d. Principe	Luzon	N.–S.	—
August 10	5.45 A.M.	Nueva-Cáceres	Camarines, S.	,,	W.–E.	—
,, 21	12–1 A.M.	Iba	Zambales	,,	—	—
,, 22	10–11 P.M.	,,	,,	,,	—	—
,, 23	12.30 A.M.	Vigan	Ilocos, S.	,,	—	—
September 13	10 A.M.	Nueva-Cáceres	Camarines, S.	,,	—	—
,, 15	11.30 P.M.	Surigao	—	Mindanao	W.–E.	—
November 15	7–8 A.M.	Iba	Zambales	Luzon	—	—
,, 17	11.30 P.M.	Vigan	Ilocos, S.	,,	—	—
,, 17	night.	Benguet	Benguet	,,	—	—
,, 17	,,	Cayan	Lepanto	,,	—	—
,, 18	4–5 A.M.	Iba	Zambales	,,	—	—
,, 19	8 P.M.	Mobo	—	Masbate	—	—
,, 22	noon.	,,	—	,,	—	—
,, 26	—	Albay	Albay	Luzon	—	Great eruption of Mount Tacbacon.
,, 27	11.5 P.M.	Manila	Manila	,,	—	One shock, intensity 3 mm.
December 12	4.5 P.M.	,,	,,	,,	N.E.–S.W.	Intensity 1°, sismometer 1 mm.
,, 12	9.45 P.M.	Zamboanga	—	Mindanao	—	—
,, 13	4 P.M.	Baler	Com. P. M. d. Principe	Luzon	N.–S.	—
,, 13	11.45 P.M.	Zamboanga	—	Mindanao	—	—
,, 18	? P.M.	Iba	Zambales	Luzon	—	—
,, 20	4–5 A.M.	Batangas	Batangas	,,	—	—
,, 25	3 A.M.	Calapan	—	Mindoro	N.–S.	—
,, 25	9.23 A.M.	Manila	Manila	Luzon	S.E.–N.W.	Intensity 1°.

If we take, with Dr. von Drasche ("Fragmente zu einer Geologie der Insel Luzon," Vienna, 1878), North Luzon to a little north from 16° N., Central Luzon from there to about 14° 30′ N., and South Luzon on the south of this line, we have 11 earthquakes in North Luzon, 15 in Central Luzon, 8 in South Luzon; and on the islands mentioned—1 on Mindoro, 2 on Masbate, 1 on Leyte, and 4 on Mindanao.

There were recorded in the year 1876, altogether 41 earthquakes on the Philippine Islands.

SOME RESULTS OF THE SUPPOSITION OF THE VISCOSITY OF THE EARTH[1]

SIR W. THOMSON'S investigation of the bodily tides of an elastic sphere has gone far to overthrow the idea of a semi-fluid interior to the earth, yet geologists are so strongly impressed by the fact that enormous masses of rock have been poured out of volcanic vents in the earth's surface, that the belief is not yet extinct that we live on a thin shell over a sea of molten lava. It appeared to the author, therefore, to be of interest to investigate the consequences which would arise from the supposition that the matter constituting the earth is of a viscous or imperfectly elastic nature. In this paper these hypotheses were followed out, and the results were fully as hostile to the idea of any great mobility of the interior of the earth as are those of Sir W. Thomson.

[1] Abstract of a paper on the bodily tides of viscous spheroids, by G. H. Darwin. Read before the Royal Society, May 23.

It is first shown that every problem about the strains of an incompressible elastic solid has its analogue touching the flow of an incompressible viscous fluid, and that the solution of Sir W. Thomson's problem of the bodily tides of an elastic sphere may thus be adapted to give the bodily tides of a viscous spheroid. The state of internal flow of a viscous spheroid is then found, under the joint influence of any external disturbing force and of the mutual gravitation of the parts of the spheroid.

When there is no disturbing force this gives the law of the subsidence of inequalities on the surface of a viscous globe under the influence of simple gravitation; and it is suggested that some light may possibly be thrown thereby on the laws of geological subsidence and upheaval. It appears that inequalities of wide extent will subside much more quickly than wrinkles, as might have been expected from general considerations.

The rate is found at which a rotating spheroid would adjust itself to a new form of equilibrium, when its axis of figure is not coincident with that of rotation; and the law is established which

was assumed in a former paper.[1] The case is next considered where the disturbing force is regularly periodic in time; this is the assumption appropriate for the tidal problem. The forces which act on the spheroid in this case do not form a rigorously equilibrating system; but there is a small couple called into existence, the consideration of which is deferred to a future paper.

It appears that the bodily tide lags, and is less in height than it would be if the spheroid were perfectly fluid; also the ocean tides on such a spheroid are accelerated, and are less in height than they would be on a rigid nucleus.[2]

This theory is then applied to the lunar semi-diurnal and fortnightly tides, and numerical tables of results are given.

A comparison of the numbers given with the viscosity of pitch at near the freezing temperature (as roughly determined by the author) shows how enormously stiff the earth must be to resist the tidally distorting influence of the moon. It may be remarked that pitch at this temperature is hard, apparently solid and brittle; and if the earth was not very far stiffer than pitch, it would comport itself sensibly like a perfect fluid, and there would be no ocean tides at all. It follows, therefore, that no very considerable portion of the interior of the earth can even distantly approach the fluid condition.

This does not, however, seem conclusive against the existence of bodily tides in the earth of the kind here considered; for, under the enormous pressures which must exist in the interior of the earth, even the solidest substances might be induced to flow to some extent like a fluid of great viscosity.

The theory of the bodily tides of an "elastico-viscous" spheroid is next developed. The kind of imperfection of elasticity considered is where the forces requisite to maintain the body in any strained configuration diminish in geometrical progression, as the time increases in arithmetical progression. There are two constants which define the mechanical nature of this sort of solid: first, the coefficient of rigidity, at the instant immediately after the body has been strained; and second, "the modulus of the time of relaxation of rigidity," which is the time in which the force requisite to maintain the body in its strained position has diminished to ·368 of its initial value. The author is not aware that there is any experimental justification for the assumption of such a law; but after considering the various physical objections which may be raised to it, he concluded that the investigation was still of some value.

The laws of flow of such an ideal solid have been given (with some assistance from Prof. Maxwell) by Mr. Butcher,[3] and they are such that the solutions already found might easily be adapted to the new hypothesis. The results of the application to the tidal problem are not quite so simple as in the case of pure viscosity. By a proper choice of the two constants, the solution becomes either that for a purely viscous spheroid or for a purely elastic one. This hypothesis is therefore intermediate between those of pure viscosity and pure elasticity.

Sir William Thomson worked out numerically the bodily tides of elastic spheres with the rigidities of glass and of iron; and tables of results are given for those rigidities, with various times of relaxation of rigidity, for the semi-diurnal and fortnightly tides.

It appears that if the time of relaxation of rigidity is about one-quarter of the tidal period, then the reduction of ocean-tide does not differ much from what it would be if the spheroid were perfectly elastic. The acceleration of high tide still, however, remains considerable; and a like observation may be made in the case of pure viscosity approaching rigidity. This leads the author to think that perhaps one of the most promising ways of detecting such tides in the earth, would be by the determination of the periods of maximum and minimum in a tide of long period in a high latitude.

The second part of the paper contains a dynamical investigation of the ocean tides in an equatorial canal running round a yielding nucleus, and the results are confirmatory of the previous ones.

The author states as the chief practical result of this paper that it is strongly confirmatory of the view that the earth has a very great effective rigidity; but that its chief value is, that it forms a necessary first chapter to the investigation of the precession of viscous and imperfectly elastic spheroids—an investigation which he hopes to complete very shortly.

[1] On the Influence of Geological Changes on the Earth's Axis of Rotation. *Phil. Trans.*, vol. clxvii. Pt. I.

[2] The law is as follows:—If $\frac{v}{2\pi}$ be the frequency of the tide, μ the coefficient of viscosity, g, gravity, a, earth's radius, w, earth's density, and if $\tan \epsilon = \frac{19\mu v}{2gwa}$, the tide of the viscous spheroid is equal in height to the equilibrium tide of a perfectly fluid spheroid multiplied by $\cos \epsilon$, and the tide is retarded by $\frac{\epsilon}{v}$. Also the equilibrium tide of a shallow ocean overlying the nucleus is equal to the like tide on a rigid nucleus multiplied by $\sin \epsilon$, and there is an acceleration of the time of high water equal to $\frac{\pi}{2v} - \frac{\epsilon}{v}$.

[3] *Proc.* Lond. Math. Soc., December 14, 1876, pp. 107–9.

PHYSICAL GEOLOGY[1]

A Geological Proof that the Changes of Climate in past times were not due to changes in the position of the Pole; with an attempt to assign a minor limit to the duration of Geological Time.

IF we examine the localities of the fossil remains of the Arctic regions, and consider carefully their relations to the position of the present North Pole, we find that we can demonstrate that the Pole has not sensibly changed its place during geological periods, and that the hypothesis of a shifting pole (even if permitted by mechanical considerations) is inadmissible to account for changes in geological climates.

We are thus driven to the conclusion that geological climates are due to the combined cooling of the earth and sun; and on comparing the rates of cooling of such a body as the earth with the maximum measured thicknesses of the several strata, we find a remarkable proportion between them, which leads towards the conclusion that the maximum thicknesses of the strata are proportional to the times of their formation; and so I deduce a minor limit of geological time.

Climate of the Parry Islands in the Jurassic Period.—Capt. M'Clintock found in the Parry Islands, on the north coast of America, at Point Wilkie, in Prince Patrick's Island, lat. 76° 20′, tropical shells, and drew the attention of geologists to the difficult task of providing a tropical climate inside the Arctic Circle, to accommodate the habits of the animals that lived there in jurassic times. The tropical fossils found in the Parry Islands were:—

Ammonites M'Clintocki (M'Clintock).
Monotis septentrionalis ,,
Pleurotomaria sp. ,,
Nucula sp. ,,
Ichthyosaurus sp. (vertebræ) (Sir Edward Belcher).[2]
Teleosaurus sp. (vertebræ) (Capt. Sherard Osborne).[3]

The *Teleosaurus* was a reptile closely resembling the gavial of India, which is found nowhere outside the Tropics, and requires warmer water than the alligator of America. The alligator flourishes in the neighbourhood of New Orleans, whose climate is represented by the following figures:—

Mean Monthly Temperature of New Orleans.

	°		°
January	54·8 F.	July	+ 81·6 F.
February	56·4 ,,	August ...	+ 81·2 ,,
March	62·9 ,,	September ...	+ 78·5 ,,
April	69·0 ,,	October	− 69·8 ,,
May	74·8 ,,	November ...	− 60·2 ,,
June	79·9 ,,	December ...	− 56·0 ,,
Yearly mean	68°·7 F.		

Reptiles requiring a climate such as is indicated by the preceding table, lived in the jurassic period within 900 miles of the North Pole, where the present climate is represented by the following figures:—

Mean Monthly Temperature of Melville Island.

	°		°
January	− 31·3 F.	July	+ 42·4 F.
February	− 32·4 ,,	August	+ 32·6 ,,
March	− 18·2 ,,	September ...	+ 22·5 ,,
April	− 8·2 ,,	October ...	− 2·8 ,,
May	+ 16·8 ,,	November ...	− 21·1 ,,
June	+ 36·2 ,,	December ...	− 21·6 ,,
Yearly mean	+ 1·2 F.		

[1] "Notes on Physical Geology." Paper read at the Royal Society, April 4, by the Rev. Samuel Haughton, M.D. Dublin, D.C.L. Oxon, F.R.S., Professor of Geology in the University of Dublin. No. IV.

[2] Exmouth Island, lat. 77° 12′ N. (only 900 miles from the Pole).

[3] Rendezvous Hill, at north-west extremity of Bathurst Island, lat. 77° N.

Climate of Spitzbergen in the Triassic Period.—The Triassic beds of Spitzbergen, lat. 79°, have afforded species of

Nautilus	*Ceratites*
Ammonites	*Halobia*

closely allied to, if not identical with, those of the St. Cassian beds of South Austria.

Climate of Alaska in the Triassic and Jurassic Periods.—In the neighbourhood of Cook's Inlet, in Alaska, lat. 60° N., shells characteristic of the triassic and jurassic periods have been found:—

Monotis	Triassic.[1]
Aucella	Jurassic.
Ammonites Wosnessarski	,,
Ammonites biplex	,,
Belemnites pascillosus	,,
Pleuromya unioides	,,

and similar fossils are found along the Pacific Coast of North America.

It is not possible to explain the occurrence of tropical animals in the three above-mentioned localities, by any change in the position of the earth's axis, even if so great an amount of change as would be required were possible. This statement can be proved as follows:—Let a great circle be drawn, joining Spitzbergen with Cook's Inlet, Alaska; this circle will pass nearly through the North Pole. In order to explain the tropical climate of these two localities, and also of the Parry Islands, the pole must be displaced at right angles to the great circle joining Spitzbergen and Alaska, along the meridian long. 117° E., nearly that of Pekin. The present difference of latitude between New Orleans and Spitzbergen is 45°; so that, in order to make the Arctic regions tropical, we must move the North Pole 45° on the meridian of Pekin, bringing it within 300 miles to the north of that city. Hence it follows that, during the triassic period, Pekin lay under the North Pole, covered by the polar ice-cap. Let us now consider what the South Pole was doing: it had moved on the opposite meridian, and had reached the mouth of the Rio Negro, on the east coast of Patagonia, about 1,000 miles to the south-south-east of Valparaiso and the Chilian Andes. Jurassic strata have been found in the Chilian Andes at 34° S., containing the tropical *Ammonites biplex*, which is found also in Alaska, 60° N., and in Europe. This locality lies within 700 miles of the necessary position of the South Pole, and cannot have enjoyed a tropical climate. The proposed alteration of the North Pole is consistent with the occurrence of tropical animals in the Parry Islands, in Spitzbergen, and in Alaska; while the proposed alteration of the South Pole would permit tropical animals to exist in New Zealand and New Caledonia; but the occurrence of jurassic ammonites within 700 miles of the South Pole is fatal to the proposed shifting of the axis of rotation, even if that were allowable to the extent required.

The Climate of the Arctic Regions during the Tertiary Miocene Period.—There is abundant evidence to show that during the miocene tertiary period the northern parts of the continents of America and Europasia possessed a nearly common forest vegetation, with a temperate climate, resembling that now enjoyed by the northern parts of Italy, such as Lombardy.

The localities in which the *lignite* beds are found, that indicate the former existence of this remarkable vegetation, are the following:—

Greenland (Disco), lat. 70°.
Grinnell Land, lat. 81° 44′.
Spitzbergen (West Coast), lat. 77°.
Alaska and Mackenzie River, lat. 70° to 60°.

The genus *Sequoia* (redwood) has representatives in all these localities, and one Greenland species, *S. gigantea*, is very near the great Californian species which lived in North America in cretaceous times (and still live in California). In Spitzbergen there are found in the miocene beds two species of *Libocedrus*; and of these, one, viz., *L. decurrens*, is now living in California among the Redwoods, while the other still lives in the Andes of Chili. The common *Taxodium* (cypress) of the Southern States occurs fossil in the miocene beds of Spitzbergen, Greenland, and Alaska.[2]

All the genera mentioned in the note are found in Greenland and Spitzbergen, as well as in Alaska; and, according to Prof. Heer, indicate a mean annual temperature of 48° F. during the miocene period in localities where the mean annual temperature is now as low as zero. During the eocene tertiary period, according to Ettingshausen, there flourished a flora in the Tyrol, which indicates a mean annual temperature between 74° F. and 81° F., the species being largely Australian in character. According to the same author the miocene flora of Vienna was sub-tropical, corresponding to a mean annual temperature between 68° F. and 79° F., and closely resembling that of sub-tropical America. It can be shown by a method similar to that employed for the triassic and jurassic periods, that the North Pole was practically in the same place during the miocene period that it now occupies.

If we join the Mackenzie River and Spitzbergen by the arc of a great circle the North Pole must be moved at right angles to this arc, away from Greenland, through 30°, in order to give all these northern localities a Lombardic climate. The direction in which the pole must be moved is on the meridian of Nagasaki (one of the Japanese Islands), and it reaches a point close to Yakutsk, within 800 miles of the Peninsula of Kamtschatka and the Island of Saghalien, off the Amoor.

Here we meet with a difficulty similar to that offered by the South Pole in the triassic period. The Island of Saghalien and the Peninsula of Kamtschatka contain miocene coal beds, requiring at least a sub-tropical climate, which would be impossible under the supposed circumstances. Also the Islands of Yesso, Nagasaki, and Kiusiu, somewhat farther off, contain similar coal beds.[1]

It is very remarkable that, while there exist so many proofs of a warm climate near the North Pole in former geological periods, there is no evidence from fossils of cooler climates having ever existed in the tropics. It was at one time thought that an exception to this statement occurred in the Island of Java, where, it was asserted, a tertiary flora was to be found, indicating rather a temperate than a tropical climate. The full investigations of Göppert, however, have satisfactorily shown that the tertiary flora of Java is of eocene age, and essentially tropical in character, containing numerous specimens of palms, Musas, peppers, laurels, magnolias, and Proteaceæ.

From all these facts we are entitled to conclude that, down to so recent a period as the miocene tertiary, climates depended chiefly on the internal heat derived from the cooling earth. As we are precluded from assigning large changes in the position of the poles as a cause for large changes of climate, a very interesting question thus arises as to the sense in which we call the miocene tertiary a recent period. This question may be thus discussed:—We may regard the plants and animals preserved in the fossil state in the Arctic regions as self-registering thermometers, recording for us the mean temperature of those regions at successive epochs, marking so many fixed points on the earth's thermometrical scale. In addition to these we have the present temperature of the Arctic regions directly observed, and two other temperatures determined by physical and physiological conditions: these are the temperature of boiling water, and the temperature at which albumen coagulates. No stratified rocks could have been formed on the earth before the first point

[1] Triassic slates, containing *Monotis* and *Halobia*, have been recently discovered in places widely separate from each other, all over the globe, viz.:—New Zealand, New Caledonia, North-West America, Upper India beyond the Himalayas, and in Spitzbergen.

[2] The following genera have been described by Prof. Heer as found at Mackenzie River and Alaska; there are many species of each:—

Planera.	*Juglans* (walnut).
Castanea (chestnut).	*Carya* (hickory).
Diospyros (ebony tree).	*Rhus* (sumach).
Daccinium (bilberry).	*Vitis* (vine).
Acer (maple).	

All these are indicative of a Lombardic climate, for their living representatives (excepting *Vaccinium* and *Acer*) do not extend into the north temperate region.

[1] Spitzbergen, and the islands of New Siberia, in miocene times, supported a vegetation pointing to like conditions of climate. Further east the coal-fields of Saghalien seem to be likewise of mid-tertiary age, and those of Yesso, I believe, belong to the same epoch. In the Island of Kiusiu miocene rocks are developed to an enormous extent; the volcanic conglomerates, shales, and sandstones of Nagasaki, seem to exceed 5,000 feet in thickness, and the upper portion of this very important series contained, prior to its denudation, one of the richest coal-fields in the world. In the Island of Takosima, where a good section is obtainable, there exist, within a thickness of little more than 300 feet, no less than fourteen beds of coal, varying from 1 to 8 feet thick, and whose united thickness amounts in the aggregate to about 57 feet. Some of them rest on shales containing remains of the old flora, which bears a close resemblance to that of the district at the present day. Unfortunately this rich fossil flora remains as yet undescribed. The fossil flora of Spitzbergen and New Siberia finds its nearest analogue in that of North China and Japan, so that we are compelled to believe in the former extension of a similar flora over the intermediate districts, as well as in the occurrence of very similar conditions of climate."—"The Border Lands of Geology and History" (pp. 28-9). By Thomas W. Kingsmill. Shanghai 1877.

of cooling down was reached, because there was no water to form them; and no life could have existed on the earth until it cooled down to the latter temperature.

Thus we find in the Arctic regions, the following successive temperatures:—

1.—212° F.	... ...	Boiling water.
2.—122° F.	... ...	Coagulation of albumen.
3.— 68° F.	... ...	Triassic and jurassic periods. (Climate of Gulf of Mexico.)
4.— 48° F.	... ...	Miocene tertiary period. (Climate of Lombardy.)
5.— 32° F.	... ...	(Climate of Labrador.)
6.— 0° F.	... ...	Present climate.

The interval between the first and second corresponds to the azoic rocks; that between the second and third to the palæozoic rocks; and that between the third and fourth to the neozoic rocks. Now, although we do not know the coefficient that fixes the rate of cooling of the hot earth suspended in cold space, we know the law of such cooling, and can compare, by calculation, the proportions of the foregoing intervals of time with each other.

When this calculation is made we obtain:—

Azoic time...	(212°–122°)	... ...	33·0 per cent.
Palæozoic time ...	(122°– 68°)	... ...	41·0 ,,
Neozoic time ...	(68°– 48°)	... ...	26·0 ,,
			100·0 ,,

In my former note, iii. p. 545,[1] I have given a table of the approximate thicknesses of the several strata in Europe. That table was prepared, with the assistance of the late Prof. Phillips, many years ago, and is not as complete as it might be. I therefore sought the assistance of Prof. Edward Hull, Director of the Geological Survey of Ireland, and with his help I have constructed an improved table.

Converting the maximum thicknesses recorded in that table into percentages, and comparing them with the percentages of time found from the theory of a cooling globe, we find—

Scale of Geological Time.

Period.	From Theory of Cooling Globe.	From Maximum Thickness of Strata.
Azoic	33·0 per cent.	34·3 per cent.
Palæozoic	41·0 ,,	42·5 ,,
Neozoic	26·0 ,,	23·2 ,,
Total ...	100·0 ,,	100·0 ,,

The agreement between these figures, derived from entirely independent sources, is remarkable, and tends to justify the principle held by many geologists, that—

The proper relative measure of geological periods is the maximum thickness of the strata formed during those periods.

This is equivalent to supposing the rate of deposition of strata to have been constant during the period contained in the table, which is probable enough on other grounds; for, although the rock-making forces were greater when the heat was greater, it must be remembered that the land surfaces to be denuded were smaller, and that the sea bottoms, on which the *débris* was to be spread, were also greater. The calculation founded on the theory of the cooling globe cannot with safety be carried down to near the point of equilibrium temperature, which is the Fahrenheit zero (for the Arctic regions under consideration); but we may, without risk, extend the calculation from 48° F. to 32° F.; that is, we may estimate the interval of time from the miocene tertiary epoch, when the Parry Islands and Northern Greenland enjoyed a Lombardic climate, to the epoch (probably long past) when those districts suffered a climate like that of Labrador, but better than that they now have.

The result of the calculation, when reduced to the same scale as that used in the table, is 32 per cent., a result, the importance of which will be better seen by the following propositions which flow from it:—

1. *A greater interval of time now separates us from the miocene tertiary epoch than that which was occupied in producing all the secondary and tertiary strata, from the triassic to the miocene epoch.*

2. *The enormous interval of time that separates us from the miocene epoch affords ample opportunity for the development of the gigantic mammals, which are commonly supposed to have somewhat suddenly made their appearance on all our continents, and to have disappeared as suddenly.*

All the foregoing facts point to the conclusion that the present condition of the earth's surface is profoundly different from its condition in the geological periods when climates depended chiefly on the internal heat of the earth, and not on that of the sun, as at present.

The following table contains estimates of the number of years required by the several rivers to scrape off one foot from their respective rain-basins, and carry the materials to the sea, where it is spread out on the sea bottoms by ocean currents. The figures are obtained by carefully measuring, at frequent intervals, the total discharge of water and the total weight of mud held in suspension. This weight of mud, reconverted into surface rock, must cover the entire rain-basin to a depth of one foot spread uniformly.

Rates of Denudation of Rain-Basins Lowered One Foot.

Ganges	2,358 years.
Mississippi	6,000 ,,
Hoang Ho	1,464 ,,
Yangtse Kiang	2,700 ,,
Rhone	1,528 ,,
Danube	6,846 ,,
Po	729 ,,
Mean	3,090 ,,

From this table it appears that atmospheric agencies are capable, at present, of lowering the land surfaces at the rate of one foot per 3,000 years; but since the sea bottoms are to the land surfaces in the proportion of 145 to 52, the rate at which (under present circumstances) the sea bottoms are silted up, that is to say, the present rate of formation of strata, *is one foot in* 8,616 *years*. If we admit (which I am by no means willing to do) that the manufacture of strata in geological times proceeded at *ten times* this rate, or at the rate of one foot for every 861·6 years, we have for the whole duration of geological time, down to the miocene tertiary epoch,

$$861{\cdot}6 \times 177{,}200 = 152{,}675{,}000 \text{ years.}^{1}$$

To this must be added at least one-third, as before shown, to bring in the period from the Miocene Tertiary to the time when the Parry Islands and North Greenland had the climate of Labrador.

This gives for the whole duration of geological time a *minimum of two hundred millions of years.*

[1] *Proc.* R. S., December 20 1877

[1] The coefficient 177,200 is the total number of feet of maximum thickness of all the known stratified rocks.

ACTION OF DRUGS ON THE LIVER[2]

PROF. RUTHERFORD'S paper described the concluding results of the long research undertaken by him on "The Biliary Secretion with Reference to the Actions of Cholagogues." He pointed out the difficulties which had rendered it impossible for physicians to arrive at precise knowledge as to the actions of substances on the liver from observations on the human subject, and the imperative necessity for having recourse to experiments on animals, whereby some of the factors that complicate the case in the uninjured system may be eliminated, and definite knowledge regarding the action of agents on one of the most important organs of the body instituted for the vague guesses of twenty centuries. Several previous investigators had striven by experiments on animals to settle this question, but all had failed owing to the faulty character of the methods employed. By a new and precise method of continuous collection of the bile, and measurement of the amount secreted every fifteen minutes—with a careful elimination of disturbing factors—the whole physiological pharmacology of the liver has been worked out by Prof. Rutherford—as far as it seems at present desirable to proceed. The actions of as many as forty-six substances on the bile-forming function of the liver have been investigated, and results of much importance for rational therapeutics obtained. Some of the

[2] Abstract of paper read at the Royal Society of Edinburgh on June 17, by Prof. Wm. Rutherford, F.R.S., Prof. Sir Wyville Thomson (in the absence of Sir Wm. Thomson) in the chair.

substances employed, viz., sodium salicylate, the benzoates, phytolaccin, physostigma, euonymin, sanguinarin, ipecacuan, &c., have not hitherto been known to stimulate the liver; and definite information has now been obtained regarding the influence of a number of other substances whose effects have been hitherto altogether doubtful. He has also proved that if a purgative agent has no direct stimulating power on the liver it diminishes the secretion of bile, and the importance of this fact is indicated. The results of the experiments which were performed on dogs are in complete harmony with every fact that has been perfectly ascertained in the human subject. The experiments with every substance supply a precision of knowledge regarding the effect of that substance on the liver which has not previously existed. In indicating the place for such experiments in medical science, Prof. Rutherford said :—"We all know how excessively complicated the analysis of the effects of drugs becomes when they are administered to a bodily system distorted in its action by the effects of disease. Of necessity the influence of a drug upon a diseased state is the ultimatum of pharmacology; and every experiment upon a healthy bodily system, whether of man or animal, is merely ancillary to experiments with the drug in disease. If we discover that a drug stimulates the healthy liver of a dog, we do not conclude that it must also stimulate the human liver in health, and still less do we conclude that it must have this action in disease. The experiments on the healthy liver of the dog, on the normal and on the abnormal human liver, are three sets of experiments, closely related, but still distinct. The results of any one of the three series cannot be substituted for those of the other two. Each set of facts has its own proper place, and must be carefully kept there. When, therefore, we show by our physiological method of experiment that such a substance as sodium salicylate or sodium benzoate powerfully stimulates the liver of a dog, we do not for a moment say to the clinical observer—You will find that these things act thus in man; but we merely say this: Experiment with these agents on man, and tell us whether or not you find that they stimulate his liver, and tell us also in what diseased states you find the employment of this or of that substance most advantageous. The clinical experimentalist has a far more difficult task to discharge than the physiological investigator, and he urgently requires all the assistance that physiological methods can render him; and the more so because it is now admitted by all competent thinkers that the actions of medicinal agents in diseased conditions cannot be rightly understood unless we also know their effects in a healthy condition of the bodily system." He further showed that although therapeutics can never be brought within the sphere of exact science, it is nevertheless very urgently our present business not to fold our hands in a despairing nihilism, but to search for every fact that can throw light on the function of every bodily organ, the nature of its diseased conditions, and the manner in which it is influenced by medicinal agents in its normal and abnormal states; and all we desire is that those who don't comprehend our methods of procedure, although they are ever ready and eager to profit by its results, will, instead of throwing obstacles in our way, leave us to do what we can to alleviate not only the sufferings of human beings, but also those of animals.

At the conclusion of the paper Sir Robert Christison characterised the professor's communication as of the greatest importance, and as one which would hand his name down to a very distant future. The professor deserved the commendation of the Society for his courage in going on, in spite of a sentimental opposition, with his researches. He thought that the time would come when the public would wake up from the delusion in this regard in which it now was. Sir Wyville Thomson, in intimating the thanks of the Society to Prof. Rutherford, said that, in his opinion, if a man in a public position felt that he had knowledge and nerve sufficient to perform these experiments for lessening the suffering and prolonging the lives of men, even though they should involve a certain amount of suffering to the lower animals, he was not only entitled but was bound to peform them.

SOCIETIES AND ACADEMIES

London

Royal Society, May 16.—"On the Spectra of Metalloids. Spectrum of Oxygen." By Arthur Schuster, Ph.D., F.R.A.S. Communicated by J. Clerk Maxwell, F.R.S., Professor of Experimental Physics in the University of Cambridge.

The many unexplained phenomena attending the passage of electricity through gases will probably for some time to come occupy the attention of experimental physicists. It is desirable that the subject should be approached from as many different sides as possible. One of our most powerful instruments of research is the spectroscope, but before it can be applied to the study in question we have to settle the chemical origin of the different spectra, which we observe in vacuum tubes, and to discuss in what way such spectra are liable to change under different circumstances. I have chosen oxygen as a first subject of investigation. Though Plücker and Wüllner have, as far as their experiments went, accurately described the phenomena seen in oxygen tubes, the following contains much that is new, and will put some of the older facts on a firmer basis.

As some of the facts brought to light by the investigation bear directly on the question of double spectra, our knowledge on that point must be briefly referred to. We divide all known spectra into three orders—continuous spectra, channelled space spectra, and line spectra. With regard to continuous spectra, it is shown that the older statement which limited them to liquid and solid bodies is no longer tenable. Most gases give continuous spectra long before they condense. Two theories of continuous spectra are noticed. The one considers that the vibrations of a molecule always tend to take place in a fixed period, but that the impacts of other molecules may, when the pressure is great or in liquid and solid bodies, prevent complete oscillations taking place, and thus produce a continuous spectrum. The other theory considers that, when a gas condenses, molecular combinations take place, which make the molecular structure more complicated, and may produce channelled space spectra or continuous spectra. According to the latter theory such molecular combinations are possible before the gas condenses, and thus the state of aggregation of the gas only indirectly affects the spectrum. The latter theory seems to be more consistent with experiment than the former one. For instance, it is shown that oxygen gives a continuous spectrum at the lowest temperature at which it is luminous. If the temperature be raised, the continuous spectrum is replaced by a line spectrum. This seems to be inexplicable by theory of molecular impacts.

The chief difficulty in the way of a complete investigation of the spectrum of oxygen consists in the great disturbing influence of the presence of even a small quantity of any carbon compound. Amongst a great many oxygen tubes which were filled by various makers I only found one, which showed the spectrum of pure oxygen; all the others gave a spectrum of carbonic oxide. It is therefore necessary in filling oxygen tubes to avoid all greased joints and all india-rubber tubings. I have used a Sprengel air-pump which communicated with the vacuum tube by means of a ball and socket joint. The joint was kept air-tight solely by means of strong sulphuric acid. The vacuum tube was fused directly to the ball of the joint. To one end of the vacuum tube a piece of hard glass tubing had been fused. This was filled with different substances which, on heating, gave off pure oxygen. The oxygen, therefore, came only into contact with glass, mercury, and sulphuric acid, and the metal of the electrode. Permanganate of potash, oxide of mercury, and chlorate of potash, were used in turn, to prepare the oxygen, but no effect was observed which could be traced to the substance used. The effect of the electrodes was eliminated by varying the metals. Aluminium, platinum, silver, brass, and iridium were used as electrodes. Any possible effect of the glass was eliminated by finally repeating all experiments in a glass receiver six inches in diameter, so that no part of the spark came nearer than 2½ inches to the glass. In this way it is believed all possibility of error due to the presence of any possible impurities was avoided.

Four different spectra of oxygen must be distinguished. At the lowest temperature at which oxygen becomes luminous it gives a continuous spectrum. As the temperature is gradually raised the continuous spectrum is successively transformed into two distinct line spectra, which I call respectively the compound line spectrum and the elementary line spectrum. It is one of the principal objects of this paper to show that these two line spectra which have been much mixed up together have a separate existence. The generation of one always involves the destruction of the other. The fourth spectrum is that which is always seen in vacuum tubes at the negative pole.

The Continuous Spectrum.—The following facts prove the statement that at the lowest temperature at which oxygen is luminous it shows a continuous spectrum.

1. The wide part of a Plücker tube generally shines with a faint yellow light. When looked at by means of a prism the spectrum is perfectly continuous.

2. If a spark of an ordinary Ruhmkorff coil is taken in oxygen at atmospheric pressure, one of the line spectra generally appears, but when the break is put out of adjustment so as to weaken the spark, the lines disappear and are replaced by a continuous spectrum which has its maximum of intensity in the greenish-yellow, and gradually fades away towards both ends of the spectrum.

3. Becquerel mentions an observation according to which the point of the oxy-hydrogen flame takes a yellow colour when an excess of oxygen is present. The description of the somewhat characteristic colour which Becquerel gives coincides exactly with the colour of the spark in oxygen, when it shows the continuous spectrum. According to Plücker an excess of hydrogen shows the hydrogen lines, and it is therefore reasonable to suppose that in Becquerel's experiment the oxygen was sufficiently heated up to become luminous.

The continuous spectrum must not be confounded with the continuous spectrum, which under high pressure forms the background to the line spectrum.

The Elementary Line Spectrum.—This is the spectrum which is seen when a strong spark passes through oxygen at the atmospheric pressure. It can be seen at all pressures when a jar and air break are introduced into the circuit.

The Compound Line Spectrum.—Plücker, in his first investigation of oxygen, says it consists of four lines, one in the red, two in the green, and one in the blue. In his later drawing of the spectrum of oxygen, he gives a great number of lines of which these four form a part. Wüllner says that the four lines in question are always the first to appear in oxygen tubes. Thalèn and Angström do not give these lines; Huggins does not give them; Salet does not give them. Plücker and Wüllner are the only observers who experimented under the circumstances under which the lines appear. They come out equally well whatever way the oxygen is prepared, whatever the nature of the electrode, and I have seen them under the large glass receiver already mentioned. The following is the appearance of the spectrum of oxygen as it undergoes gradual exhaustion.

When the pressure is sufficiently diminished to allow the spark to pass, it shows a yellow colour and the spectrum is perfectly continuous. Almost immediately, however, the four lines are seen in the capillary part of the tube above the continuous spectrum. The continuous spectrum in the wide part is stronger than in the narrow part. The four lines seem to have taken away part of the energy of the continuous spectrum. As the exhaustion proceeds, the spark spreads out in the wide part, and the continuous spectrum is therefore diminished and becomes less intense than in the capillary part; but it gradually loses in intensity also in the narrow part, until the four lines stand out on a perfectly black background. If under these circumstances the jar and air break are inserted in the circuit, everything will disappear and the elementary line-spectrum will come out. We have here as complete a transformation as from the band-spectrum of nitrogen to the line-spectrum of nitrogen taking place under precisely the same circumstances; and it is therefore not unlikely that the two phenomena are due to the same cause. There are two reasons why the existence of the compound line-spectrum of oxygen as a separate spectrum may have escaped previous observers. There is a blue line in the elementary line spectrum which is nearly coincident with the blue line of the compound line spectrum. It requires considerable dispersion to notice the difference; the complete disappearance of the compound line-spectrum has therefore escaped notice. The two green lines and the red line of the compound spectrum widen easily at higher pressure and as has been remarked by Wüllner, even fuse together to a continuous spectrum. If the experiment is therefore made at a pressure at which oxygen has a continuous back-ground, the disappearance of these lines might be taken for their widening and fusing together. No such mistake is possible when the vacuum is good. I have not been able to determine with certainty whether the red line seen at atmospheric pressure is a remnant of the compound line-spectrum, or whether it is a line of the elementary line-spectrum closely coincident. I am inclined to the former view, although it often seemed as if the line seen at atmospheric pressure was less refrangible than the red line of the compound line-spectrum. I have drawn attention in a letter to NATURE (vol. xvii. p. 148), to the fact that the compound line spectrum of oxygen seems to be reversed in the sun. I have no further information to add on that point, and the wave-length of the lines will be found in that letter.

The Spectrum of the Negative Pole.—This spectrum has first been correctly described by Wüllner. It consists of five bands, one of which is too weak to be measured. Careful measurements of the bands have been taken. With regard to the explanation of the separate spectra found at the negative pole in nearly all gases, I incline to the view that they are due to separate molecular compounds which are formed at the pole. The following experiments seem to support that view. When the pressure is very small the spectrum of the negative pole extends throughout that half of the tube which incloses the negative pole, and which I shall call the negative half. If the current be suddenly reversed the spectrum of the negative pole will still be seen at first, in that part which was the negative half and now is the positive half of the tube; but it will gradually disappear and a permanent state will be established, in which the spectrum of the negative pole is, as before, only seen in the negative half. That it is the reversal and not the interruption of the current which produces the result is easily proved by interrupting the current and at once closing it again the same way, when no difference will be seen. If, however, the current be left interrupted for some time, say one minute, so that any compounds which may have been formed in the negative half may diffuse into the other half, and if then the current is closed, either the same or the opposite way, the negative spectrum will be seen at first throughout the tube, but gradually disappear in the positive half.

If the current be rapidly reversed in succession, after a little while, when the effect of the first reversal has disappeared, the permanent state will always be established at once, and the spectrum of the negative pole will appear only in the negative half.

If after the last experiment the current be interrupted for some time and then closed, the spectrum of the negative pole will at first be seen throughout the tube, and gradually disappear in the positive half.

It is not quite easy to see the explanation of the last two experiments.

The experiments were all made in the Cavendish Laboratory, Cambridge, and I am much obliged to Prof. Clerk Maxwell for the kindness with which he has placed the resources of the Laboratory at my disposal.

Linnean Society, May 24.—Annual General Meeting.—Prof. Allman, F.R.S., president, in the chair.—The anniversary address of the president dealt with a *résumé* of the principal recent discoveries in the anatomy and development of the Polyzoa, and of the resulting important features in their systematic grouping. Much had been due to the labours of Busk and Nitsche. It was maintained that investigations were mainly in favour of the so-called "brown bodies" being merely the residuum of degraded and withered polypides, and that they have no real morphological or physiological importance. He coincided with the views of Nitsche, Joliet, and Busk, that the supposed "colonial nervous system" is but an irregular plexus of cellular and protoplasmic cords and filaments derived from the walls of the zoœcium or polypide cell, and not a true nervous system. Joliet's idea of its being the origin of new polypide buds and of certain minute free corpuscles found in the zoœcium is, however, too exclusive. *Cyphonantes* is a singular little free-swimming marine creature, of pyramidal form, the soft body of which is contained within a bivalve shell. Schneider has regarded it as a larval Polyozoon, and announced the startling fact that before its transformation into the adult it becomes totally disorganised and reduced to a homogeneous protoplasmic mass, though previously its structure had been complex. Thereafter arises a new polypide, and the whole is metamorphosed into the adult form. Strange as this history may seem, it has been confirmed by the researches of Nitsche and Joliet. Finally, the question of "individuality," or the relation to the polyzoal colony was taken up by the president, and the following opinion enunciated—that the zoœcium or cell in which the polypide is lodged must be regarded as having a zooidal individuality independently of the polypide, which has a zooidal individuality of its own, and that the two thus form a compound element which becomes associated with similar ones in order to form the colony. This compound element is thus composed of two zooidal individuals—zoœcium and polypide; on the zoœcium devolving the functions of sexual and nonsexual reproduction, and on the polypide that of nutrition.—Prof. Allman

also called passing attention to some living tree frogs (*Hyla arborea*) which he had obtained in the South of Europe. Those now exhibited to the Fellows showed the remarkable change of colour which this species of frog is known to possess, some being green, others bright blue. This change of hue is due to certain pigment corpuscles the precise nature of which he at present is investigating.—The Report on Publications was read by the Secretary, and that of the balance-sheet by Dr. R. C. A. Prior. Afterwards the Treasurer (Dr. J. Gwyn Jeffreys) laid his statement of accounts, &c., for the year 1877, before the meeting. This showed a very satisfactory financial position, a balance of 46*l.* 13*s.* remaining in hand, after all current expenses had been paid, while 700*l.* had been invested since the last Annual Report. The alterations in the bye-laws relative to an increase in the rate of payment for Fellows compounding, previously read at two successive general meetings, was put to the ballot and confirmed by the Fellows at large in the terms of the Charter. —The Secretary gave a notice of the Fellows and Foreign Members who had died during the past twelvemonth; of the former there were ten, and of the latter four, viz., fourteen in all. Among these Mr. Henry Adams, Dr. Elias M. Fries, Mr. Andrew Murray, Prof. Parlatore, Mr. Fox Talbot, Dr. R. Visiani, Dr. H. A. Weddell, and Mr. T. V. Wollaston, deserve mention as of considerable repute in the scientific world. During the year thirty-eight Ordinary Fellows and five Foreign Members had been elected.—At this meeting also the following Fellows were elected into the Council:—Mr. J. Ball, Dr. T. Boycott, Mr. F. Du Cane Godman, Dr. A. Günther, and the Rev. G. Henslow, in the place of Mr. J. G. Baker, Dr. W. B. Carpenter, Mr. Henry Lee, Prof. W. K. Parker, and Mr. S. I. A. Salter, who retired by rotation. The President and Officers were re-elected.

Physical Society, May 25.—The President, Prof. W. G. Adams, in the chair.—The following candidates were elected Members of the Society:—W. Kieser, T. McEniry, W. R. Philips, G. M. Whipple.—Mr. D. J. Blaikley read a paper on brass wind instruments as resonators, describing an attempt he has made to carry into some detail certain acoustical investigations of the late Sir C. Wheatstone. A method by which the positions of the notal points in a cone and in a bugle had been fixed was shown, and it was then shown that a complete cone cannot be used by the lips as a wind instrument, that conic frustra cannot give resonance to the same series of notes as complete cones, and that therefore the conical form must be modified; and, as this modification of form makes the position of a node for every note required more or less coincide with that of the lips, so will the instrument be more or less perfectly in tune. As the number of quarter wave-lengths in a cone or wind instrument is not directly proportional to the vibrational number of the note, as it is in free space or in an open tube, so the velocity of the wave of a given note is not exactly the same as that of another note of different pitch. Experiments were shown to illustrate the effect of varieties of form in producing different qualities of tone, and evidence was given of the existence of very high harmonic or partial tones in the low notes of wind instruments. In the trombone the ninth partial tone (three octaves and a tone above its prime) was thus proved to be sounding, and partial tones up to the sixteenth have been heard.—Sir W. Thomson pointed out the connection between the range of a musical instrument and the phenomena observed in a trumpet-shaped bay between high and low water; he also considered that an investigation of the overtones due to the cavity of the mouth would well repay research in explaining the influence its shape has on the vowel sounds.—Lord Rayleigh observed that in a conical musical instrument, the correction to be made on account of the cone not being perfect to the apex is equal to six-tenths of the radius of the open end, and he pointed out that with a bell-mouthed instrument much of the sound is diffused as spherical waves.—Dr. Guthrie placed on the table a communication on salt solutions and attached water and on the separation of water from crystalline solids in currents of dry air, in continuation of his researches, which have already been published. The results could not be usefully abstracted, but as an instance of the important results obtained it may be mentioned that Dr. Guthrie finds that when dry air is passed over chloride of barium at a temperature just above 25° C., the β molecule of water is given off, and that the α molecule of water is only separated at a temperature just above 60° C. He also showed the effect of a steam jet in boring through a block, mainly with a view of obtaining suggestions as to the use of such a method in the commercial preparation of ice.—Mr. Rutherfurd then showed a photograph of the solar spectrum from the line E to H, taken by means of a grating. By means of a heliostat he concentrated the rays on a lens within a collimator, which in relation to the observing telescope was of considerable length, in order to admit as much light as possible, and the grating was movable. The enlargement was effected by inserting a lens near the focal point of the observing telescope, and he used a sensitive collodion which gave the greatest sharpness of definition about the line G.—Sir W. Thomson, in continuation of the communication made to the Society at its last meeting, described the effect of torsion on the electric conductivity of a tube of brass. We have already given an account of this paper.

Anthropological Institute, May 28.—Major-Gen. A. Lane Fox, F.R.S., vice-president, in the chair.—Mr. Hyde Clarke exhibited a carved stone object which was considered as having come from Central America.—Col. Paske read a paper on Buddhism in Little Tibet. After a brief description of the route through the Kangra and Kulu Valleys to the high mountain passes leading into Lahore and Spiti, he gave particulars of the physical features of these countries, their products, &c., with some account of the habits and customs of the people, concluding with observations on Buddhism. Col. Paske gave an explanation of the modified form of Buddhism prevalent in the provinces of Little Tibet, and brought to notice the ritual and religious customs of the Lamas or Buddhist priests; described his visits to Buddhist monasteries, exhibiting specimens of Buddhist ritualistic instruments, and other curiosities, with a small painting representing the "Triumph of Buddhism," executed by a Lama recently arrived from Llassa.—Mr. Brabrook read a paper by Mr. Alfred Simson, entitled Notes on the Piojés of the Putumayo. A tribe of Indians occupying the middle and lower Aguarico and a considerable stretch of the left bank of the Napo are known as the Santa Maria Indians or Piojés, from the word in their language, piojé, and speak the same language, and have several traits in common with the Indians inhabiting the borders of the Upper Putumayo, who seem to have no special appellation, but which Mr. Simson proposed to call the Macaguajes or Piojés of the Putumayo. Mr. Simson's experience of these Indians extended only to those living on the banks of the main stream, during long journeys with a number of them selected from different villages, and visits and sojourns in most of these villages. Their dwellings, religion, and customs were freely described. Mr. Simson also communicated a vocabulary of the Zaparo language.

Geological Society, June 5.—John Evans, F.R.S., vice-president, in the chair.—William Santo Crimp and Joseph Richard Haines were elected Fellows of the Society.—The following communications were read:—On the quartzites of Shropshire, by Charles Callaway, F.G.S.—On the affinities of the Mosasauridæ, Gervais, as exemplified in the bony structure of the fore fin, by Prof. Owen, C.B., F.R.S.—On new species of *Procolophon* from the Cape Colony, preserved in Dr. Grierson's museum, Thornhill, Dumfriesshire; with some remarks on the affinities of the genus, by Harry Govier Seeley, F.L.S., Professor of Geography in King's College, London.—On the microscopic structure of the Stromatoporidæ, and on palæozoic fossils mineralised with silicates, in illustration of eozoon, by Principal Dawson, F.R.S.—On some Devonian Stromatoporidæ, by A. Champernowne, F.G.S.—On a new species of *Loftusia* from British Columbia, by George M. Dawson, F.G.S., of the Geological Survey of Canada.

Chemical Society, June 6.—Dr. Gladstone, president, in the chair.—The following papers were read:—Analogies between the action of the copper-zinc couple and occluded and nascent hydrogen, by Dr. Gladstone and Mr. Tribe. The authors have observed that finely-divided copper charged with hydrogen converts nitre into potassium nitrite and ammonia, and reduces potassium chlorate to chloride. The copper-zinc couple converts nitrobenzol in aqueous solution into anilin, a reaction which the authors have utilised for the detection of small quantities of nitrobenzol. The action of palladium-hydrogen, platinum-hydrogen, and carbon-hydrogen on various substances have been investigated, and compared with the action of the copper-zinc couple. During the reading of the paper, Dr. Russell took the chair.—On the alkaloids of the aconites, Part 3, by Dr. Wright and A. P. Luff. The authors have continued their researches on these alkaloids, and in the present paper investigate the saponification, &c., of aconitin, picraconitin, and have obtained two new bases, aconine and picraconin; acteyl and ben-

zoyl derivatives of several of the bases have been formed. The authors draw an important practical conclusion from their work, that it is quite possible to obtain crystallised alkaloids of constant composition from *A. ferox* and *A. napellu*, instead of the amorphous preparations which are now sold, and which often contain forty or even ninety per cent. of bases more or less inert.—On the alkaloids of the Veratrums; Part 1, Alkaloids of *Veratrum sabadilla*, by Dr. Wright and Mr. Luff. After discussing the conflicting statements which have been made by previous observers, the authors give details of the process of extraction, which consisted in percolating the crushed seeds with alcoholic tartaric acid, evaporation and extraction by numerous and prolonged shakings with ether. Three alkaloids were obtained: veratrine, $C_{37}H_{53}NO_{11}$, which, on saponification, splits up into veratric acid, and a new base, verin; cevadin, $C_{32}H_{49}NO_9$, splitting up, on saponification, into cevadic acid (methylcrotonic acid) and cevin; cevadillin, $C_{34}H_{53}NO_8$, which does not crystallise or form crystalline salts.—On the action of hydrochloric acid upon chemical compounds, by J. W. Thomas. The author has examined the action in several ways of hydrochloric acid on many salts, nitrates, sulphates, tartrates, citrates, chromates, oxalates, &c.—On the action of oxides on salts, Part 1, by Dr. Mills and Mr. Wilson. The object of the authors was to determine the law in consequence of which the action of oxides on salts leads in general to the formation of other oxides derived from the salts in question. They have studied the action of tungstic, silicic, and titanic oxides on potassic carbonate at a high temperature.—On a new test for glycerin, by Dr. Senier and Mr. Lowe. This test is founded on an observation of Iles, that borax, when treated with glycerin, gives to a Bunsen flame the green colour characteristic of boracic acid. By means of the test, one-tenth per cent. of glycerin, was detected in beer after concentration, &c.—On ammonium triiodide, by G. S. Johnson. The author has prepared this substance by dissolving iodine to saturation in a strong aqueous solution of ammonium iodide, and by stirring crystals of ammonium iodide and iodine with a small quantity of water, till the resulting black liquid refused to dissolve more of either ingredient. The liquid, on evaporation over sulphuric acid, gave dark blue prisms of the substance in question slightly deliquescent; specific gravity, 3·749.

PARIS

Academy of Sciences, June 24.—M. Fizeau in the chair.—The following among other papers were read:—On the displacement of the bubble in spirit-levels, by M. Plantamour. Movements were observed (both from day to day and in the course of single days) in levels placed on the massive table of the author's limnograph, at Geneva, on the beton covered ground beside it, on the ground in a tent, and also in the author's cellar, some distance from the lake. In certain periods there is a gradual rising in the east without notable return to the west; in others there is a certain horizontal immobility, and in others, lastly, of longer or shorter time, the ground undergoes oscillations both from east to west and from north to south, more or less pronounced and regular, the limits being, however, always narrow (the greatest movements did not reach twenty seconds). M. Plantamour does not at present try to explain these movements; (they are indicated graphically).—M. D'Abbadie recalled similar observations he had formerly made, and thought this phenomenon might be an important source of error in astronomical calculations (*e.g.*, in determining latitudes and the declination of stars).—M. de Lesseps presented a stone from Chalouf, 10m. above the sea, believed to belong to the tertiary epoch, and having incrusted on it a large shark's tooth, three times the size of teeth of sharks now caught in the Red Sea, and probably belonging to a species now gone.—The death of M. Ehrman, correspondent at Strasburg, was announced.—Results of application of sulphocarbonate of potassium to phylloxerised vines, by M. De la Vergne. He considers it indispensable for very young plants with small root system, and for all vines grown in a very thin layer of mould.—On the depolarisation of electrodes by solutions, by M. Lippmann. The property of depolarising a metal belongs exclusively to salts of that metal. Hence a method of detecting the presence of a particular metal in a solution. Thus, for copper; put a copper wire as negative electrode of a weak current in the liquid; it will be polarised if there is no copper dissolved in the latter, and it will not be polarised if the liquid contain $\frac{1}{5000}$ of sulphate of copper. For silver the sensibility seems to be greater.—On a new dielectric constant, by M. Neyreneuf. In comparing different dielectrics in a condenser, he finds that for glass of the same nature the ratio $\frac{e}{n}$, of the thickness to the number of sparks corresponding to a given quantity of electricity, is constant. For more insulating substances, as ebonite, caoutchouc, &c., this ratio or *condensing constant* increases considerably with the thickness; the condensing constant of air is much greater. There is no similarity between the condensing constant and the ordinary dielectric constant.—On an experiment in magnetism relative to the telephone, by M. Luvini. He filled a hollow electromagnet with water which he inclosed, a capillary tube being connected to show any variations in the capacity; but no such variations appeared with any kind of current. (The arangement would have indicated a change of volume of $\frac{1}{30}$ cub. mm.) He infers that the changes caused by magnetising action in a magnetic mass are wholly molecular. The sounds in resonant electro-magnetic rods discovered by Page are due to reactions of the two magnetic movements, and the current.—On the telepone, by M. Des Portes. This relates to experiments on board the *Desaix*; a telephone being at one end of the circuit and a telephone magnet, with coil uppermost, suspended vertically by silk thread. The latter magnet was struck with various substances, wood, soft iron, &c., and the sounds were heard. Peculiar effects were got on striking with a magnet or speaking. M. du Moncel added an account of similar experiments.—On electro-magnets, by M. Bisson. He winds in a new way: after each row he brings the wire in a straight line to the starting-point and recommences. He thus obtains a third more power.—On the efficacy of a vibratory movement for causing decomposition of explosive liquids and ebullition of superheated liquids, by M. Gernez. He rubs the tubes with a wet cloth, producing vibration and sound.—On organic dust in suspension in the air, by M. Miquel. The average number of microbes of the air, small in winter, increases rapidly in spring, remains nearly stationary in summer, and diminishes in autumn. Rain always causes their recrudescence.—On the pressure of the cephalo-rachidian liquid, by M. Bochefontaine.

CONTENTS

ERRATA.—Vol. xviii. p. 215, 1st col., 33rd line from bottom, for "sea" read "Lea." P. 216, 2nd col., 15th line from top, for "489 mm" read "4 mm. square."

THURSDAY, JULY 11, 1878

SCIENCE IN SCHOOLS

WE print with pleasure on another page a remarkable article from the *Times* of Monday. In itself the article may present nothing remarkable to the readers of NATURE, but as the deliberate utterance of the leading organ of opinion in this country, it marks a distinct stage of progress towards a more enlightened conception of what constitutes education. We hope that it is significant of the near approach of a radical change of the conception in this country of what subjects should be included in elementary education. We need not be surprised at the fate of Sir John Lubbock's Bill for the introduction of elementary science into schools, when such erroneous conceptions of what science is apparently exist in the mind of the Minister of Education in the House of Commons, Lord George Hamilton. The Vice-President of the Council has much to learn, when his idea of the Royal Society, one of the most venerable institutions in the country, is that of a kind of select Polytechnic, where "lectures" are delivered on "biology, chemistry, natural history, mechanics, astronomy, mathematics, and botany." But he is new to his work, and we must hope that the debate of Thursday last may lead him to obtain a more accurate conception of what is meant by elementary science.

Dr. Lyon Playfair, we believe, pointed out what is one of the great hindrances to the introduction of science into elementary schools; the mere name, "science," frightens ministers, inspectors, school-boards, and teachers; perhaps if the simpler phrase, "elementary knowledge," were used, the simple-minded individuals in whose hands are the training of our future citizens might find that they themselves had been compelled to become acquainted with it to their cost after they left school, and that it would have been much better for them had they had some little training in it before entering into the thick of the fight.

The most notable feature in the *Times* article, as well as in Thursday's debate, is the fact that it has at last dawned upon the leaders of opinion and the makers of our laws, that "education" and "instruction" are different things, and that a man may learn a great many "facts" at school, and have his education to begin when he leaves it. It is lamentable that we have to be continually reminded that we are the only one of the great European countries where this distinction is not recognised and practically carried out in education. Our whole system of education, hitherto, has been a mere cramming of the children's memories with words, words, words, to the weariness of children and teachers, and with results unsatisfactory to all concerned. As the *Times* puts it:—"To be taught something about gravitation, about atmospheric pressure, about the effects of temperature, and other simple matters of like kind, which would admit of experimental illustration, and which would call upon the learner to make statements in his own words instead of in those of somebody else, would be so many steps towards real mental development." Sir John Lubbock gave a most conclusive refutation of the idea that the teaching of science must be attended with hitherto unexperienced difficulties, and at the same time proved what a relief science-teaching would be to the ordinary dull routine of instruction, when he told the House that in the Scotch schools the authorities began to take alarm because science-teaching was found so comparatively easy and pleasant by the children. As to the argument that children who have been taught to know something about the objects and forces with which they every day come into contact contract a distaste for manual labour, we should have thought it had been long ago played out; it has almost as much force as the story told by another speaker of the boy who had been impudent to his master because the latter could not read his newspaper.

It is unnecessary for us to go again into the merits of the question which has been so often and so thoroughly discussed in these pages, especially as the *Times* has put it quite as forcibly as there is occasion for doing at present. It certainly seems sad, nationally suicidal, indeed, that a few more millions of those who will have the destinies of this country in their hands, are likely to be launched into active life, with all their education to acquire, ere legislation steps in to give us the advantages which nearly every other civilised nation gives to its children. Every day we hear of the ignorance of the working classes, every other month "congresses" are held to devise means to remedy the consequences of this ignorance—ignorance of the laws of health, ignorance of household economy, ignorance of the implements and objects of labour, ignorance of the laws of labour and production, ignorance of the nature of the commonest objects with which they come into contact every day, ignorance of almost everything which it would be useful and nationally beneficial for them to know—an ignorance, alas! more or less shared by the "curled darlings" of the nation. Yet while every day's paper shows how keen is the industrial competition with other nations, and how in one department after another we are being outstripped by the results of better—*i.e.*, more scientific—knowledge, the poor pittance of "elementary knowledge" asked for in Sir John Lubbock's Bill is refused by a minister whose own "education" leaves much to be desired. This state of things cannot long continue, and with such advocates for the children as the *Times* and Mr. Forster, we may hope that next time Sir John Lubbock brings forward his Bill it will meet with a happier fate.

THE JUBILEE OF UNIVERSITY COLLEGE

LORD GRANVILLE'S admirably reasoned and temperate speech at the jubilee of University College on Tuesday, reminds us how things move in this country. It records half a dozen great advances which are now accepted cordially and universally, with all of which University College is more or less identified, and in promoting which it has never failed to take a leading part.

To begin with, there is the absolute catholicity of its offer to the student—the invitation on its motto is *Cuncti adsint, meritæque expectent præmia palmæ.* When the University

of London began in 1828, and when University College was incorporated in 1836, Oxford and Cambridge had not dreamt of throwing open their doors to dissenters. To-day the whole world so absolutely recognises the propriety of the step which University College then took, that it is difficult to give it enough credit for a courage which was then without precedent in England. Only the other day its invitation has been widened still farther. *Cuncti adsint* is to be understood as including *Cunctæ adsint*, and women as well as men will henceforth have unrestricted access to the classes in the faculties of arts, law, and science. Women are at least as much influenced in character by the kind of education which is given them as men are, and the action of University College guarantees that women—in London, at least—will have at their disposal the best education which England can offer.

The Chancellor of the University of London reminded us that the new demands of teachers of science have compelled institutions like University College to extend their buildings and to provide laboratory facilities in addition to the class-rooms and lectures of old days. University College is not alone in recognising this vital fact. King's College has thoroughly developed its practical teaching; the Scotch Universities have latterly been very much alive on the subject; Owens College, as becomes a new institution claiming to take rank with the most advanced teaching bodies of the day, has given exceptional prominence to laboratory work. But University College has not been unconscious of the movement, and she purposes, if possible, to outstrip her rivals.

The last movement, in which Lord Granville reminds us that her action has counted for a great deal, is that to extend the English definition of a university. She considers herself entitled to that appellation. "Now the term 'University,'" says Lord Granville, "has been differently understood by different persons and by different nations, and I think that the exact construction of it is very likely to be still more minutely debated in consequence of the able efforts of one of the most distinguished students of your college, now the most successful president of Owens College, whom we see among us, who has raised the question whether the number of universities ought not to be increased. In France 'university' means an aggregrate of all the schools and colleges of the kingdom. In Germany their famous universities are really professorial schools. Our two elder universities are really academical institutions—an aggregate of colleges. You know that the London University is not a teaching body at all, and that it is only an examining body, depending upon other sources, of which this college is the most prolific in giving us candidates for our examinations." The institution of University College has done much, the movement of Owens College has done more, to widen our ideas of what a University might do. There is no reason why we should be confined to the existing type of "academical institutions—an aggregate of colleges," supplemented by an examining body depending on other sources. We do not aim at French centralisation, but there is no reason why one or two universities on the German or the Scotch model should not be added to the very limited list of our English Universities. An amalgamation of University College and King's College into a teaching University of London would be a natural consequence of such a movement.

WINDMILLS AND WATERFALLS

OUR readers may remember the consternation caused some years ago by the publication of Prof. Jevons's work on our coal supplies, and the alarmed inference drawn from his calculations that the days of Britain's supremacy and prosperity were numbered. Certainly, if our prosperity is entirely dependent on our coal supplies, there can be no doubt that ere very long the beginning of the end will have arrived. Abundant as our coal supplies are their consumption at the rate of about 150 million tons annually cannot go on for ever; and while we may have the ships and the money too, it would be a serious thing for England if she had to look abroad for her greatest source of physical power. It is certainly at present difficult to see how the work of the world could be carried on if the supply of coal were completely exhausted; still if man were compelled to find a substitute or relapse into savagery or even perish altogether, we think the chances are he would be able in some way, without detriment to his progress, to adapt himself to his new circumstances. These ideas have been suggested by an interesting lecture, just published, recently delivered at Glasgow by Dr. C. W. Siemens, "On the Utilisation of Heat and other Natural Forces."

"The supremacy," he justly says, "which man enjoys over the animate and inanimate creation, and for which Divine Authority may be quoted, cannot be said to be the result of his superior muscular development, for amongst the members of the animal kingdom there are many which are his superiors in strength, agility, swiftness, and in natural aptitude to provide themselves against the vicissitudes of cold and hunger. The compensating advantage in our favour is the intelligence with which we are enabled to call forces of nature not our own into requisition to do our behests. It would not be too much to say that the power of man consists really in his ability to direct the forces of nature, and that the degree of civilisation to which he has attained is commensurate with his command of those forces."

Could any more forcible argument be urged in favour of the national advantages of scientific research, or of the yearly increasing importance of scientific knowledge in the every-day life of humanity?

Fortunately Dr. Siemens, in his lecture, gives us a pretty wide glimpse of hope that we need not despair because of the prospective exhaustion of the existing means of producing utilisable heat. Other methods, he suggests, might be found of bringing into action this greatest source of mechanical power, and that even now one of these methods might be so used that the exhaustion of our coal might be postponed for a considerably longer period than has been calculated. Dr. Siemens traces the progress of our knowledge of the real nature of heat and of the methods of ascertaining its mechanical equivalent. He shows how gradually we have learned to produce a greater and greater amount of mechanical effect from the

consumption of a certain quantity of heat in the steam-engine, the blast-furnace, and other methods of utilising this source of power as derived from coal, and points out how, by means of electricity, this agent may be made to do a greater amount of work for us than ever. The wonderful results obtained from the Siemens' dynamo-electric machine are well known, and Dr. Siemens gives an example of the saving of power that might be effected by its use:—

"Let us suppose that at some central station 100 horse-power of steam or water power was employed to give motion to several dynamo-electric machines of the dimensions found most convenient in practice, and that by means of metallic conductors of suitable dimensions the electric current produced at the central station was conducted to a number of halls or factories requiring to be lighted, or to utilise mechanical power. If illumination were the only object in view, the total amount of light that could be thus produced would be equal to 125,000 candle-power. This would be equivalent to 6,250 Argand burners, each of 20 candle-power, at a consumption per burner of 6 cubic feet of gas per hour, or a total consumption of 37,500 cubic feet of gas to produce the same effect of light. This would require 3¾ tons of coal, and the electric light about as many hundredweights."

Now if the power to drive these machines could be obtained apart from coal the addition to our mechanical resources would be immense. While Dr. Siemens shows that the tides are a source of power that might be utilised directly or indirectly, he at the same time shows that the results would not be at all commensurate to the work expended. But the old sources of power, which have gradually given way before the universal introduction of steam—wind and falling water—might again be called into play, and with infinitely greater effect than ever. It is evident that within certain limits the rotatory motion required for the working of the dynamo-electric machine might be effected by means of the old-fashioned windmill, and to a much greater extent by means of falling water. Our readers may remember that Dr. Siemens, some months ago, in an address which he then gave, referred to the immense quantity of power which flowed ready made over the Falls of Niagara. In his Glasgow address he again referred to the subject, in order to show how this gigantic source of power might be utilised to produce action at a distance. "When," he says, "little more than a twelvemonth ago I visited the great Falls of Niagara, I was particularly struck with the extraordinary amount of force which is lost, as far as the useful purposes of man are concerned. 100,000,000 of tons of water fall there every hour from a vertical height of 150 feet, which represent an aggregate of 16,800,000 horse-power, producing as their effect no other result than to raise the temperature of the water at the foot of the fall

$$\frac{150}{772} = \frac{1^\circ}{5^\circ} \text{ Fahr.}$$

In order to reproduce the power of 16,800,000 horses, or, in other words, to pump back the water from below to above the fall, would require an annual expenditure of not less than 266,000,000 of tons of coal, calculated at an average consumption of 4 lbs. of coal per horse-power per hour: which amount is equivalent to the total coal consumption of the world. In stating these facts in my inaugural address on assuming the presidency of the Iron and Steel Institute, I ventured to express the opinion that in order to utilise natural forces of this description at distant towns and centres of industry the electric conductor might be resorted to. This view was at that time unsupported by experimental data such as I have been able since then to collect." Dr. Siemens then shows what has been done in conveying the electric light to a distance; and he points out that, "if mechanical force is required to be distributed, the arrangements are in every respect similar to those for the distribution of electric light; and it has been proved experimentally that the amount of power recovered at the distant station is nearly equal to half the power employed at the central station." Even as regards the consumption of coal, were that article used, Dr. Siemens shows that the magneto-electric machine is cheaper than the gas or steam-engine. But he rightly says:—"It would not be necessary to seek on the other side of the Atlantic for an application of this mode of transmitting the natural force of falling water, as there is perhaps no country where this force abounds to a greater extent than on the west coast of Scotland, with its elevated lands and heavy rainfalls. You have already conducted the water of one of your high-level lochs to Glasgow by means of a gigantic tube; and how much easier would it be to pass the water in its descent from elevated lands through turbines, and to transmit the vast amount of force that might thus be collected, by means of stout metallic conductors, to towns and villages for the supply of light and mechanical power!"

Dr. Siemens points out other directions in which the natural forces of the universe might be used for the purposes of man, without resorting, to such an extent as we do at present, to our coal supplies. While windmills are directly rather an uncertain source of power, still, he shows that a number of windmills, such as may be constantly seen working in Holland for the drainage of the land, might, for instance, be employed to raise water, by pumping, to an elevated lake or reservoir, whence the power could be drawn off by means of hydraulic motors when required, and might be conducted electrically to centres of habitation.

We ought to be grateful to Dr. Siemens for taking so much pains to lighten up the gloomy prospects which some pessimists have been presenting to us for some years past; and to those who may be cynical enough to reply that a return to the windmill and the water-wheel is simply a sign of the prophesied retrogression, we may fitly reply, in Dr. Siemens' own words:—

"It would be wrong to suppose that a resumption ot the use of natural forces would throw us back to the time of the windmill and the primitive water-wheel which used to give motion to isolated establishments. We shall have learned to store, to transport, and to utilise these forces in a manner adapted to our superior requirements; and who knows whether the time may not come when our descendants in the third or fourth generation will look back upon the indiscrimate users of coal with something like the same feeling that we look upon the users of flint and bronze implements. Indeed, without waiting for the extinction of our coal-fields, it

appears to me not improbable that natural forces will be resorted to simply on account of their comparative cheapness and convenience of application."

WEST YORKSHIRE

West Yorkshire: an Account of its Geology, Physical Geography, Climatology, and Botany. Part I.—Geology. By J. W. Davis, F.G.S., F.L.S. *Part II.—Physical Geography and Botanical Topography.* By J. W. Davis and F. Arnold Lees, F.L.S. With Maps and Plates. (L. Reeve and Co., 1878.)

THE merit of a work on local geology or natural history may be of two distinct kinds. The author may be an original investigator of a little-known area, and his book a positive addition to our knowledge; or the volume may be a tapestry, into which the scattered threads of information are worked by one who has the mastery of them all, and who presents us with the picture they have formed in his mind. It is to excellence of this latter kind the authors of "West Yorkshire" aspire. They have gathered from the contributions of all local observers, and have so assimilated the material with their own knowledge, as to render the substance of their book a useful outline of the geology and botany of the district they have chosen to illustrate. An area included within the region of Prof. Phillips's classical work on the mountain limestone districts of Yorkshire, and upon whose coal-bearing and associated strata so much good work has been done by the Government Survey, does not leave much room for novelty in its geology, though the botany, especially as treated in this book, is in rather a different case. The great merit of the work would therefore consist in the lucid and comprehensive manner in which it presents the scattered information to us as a whole. This, however, it scarcely possesses in as great a degree as most of the books of its class.

Examined in detail there are some points which call for remark. The introductory chapter is mostly occupied with a description of the boundary line of the riding—a most uninteresting subject—but it is enlivened by the accounts of the Pennine and Craven faults. The drawing of the latter, however, on the maps does not coincide with the description, and the former is scarcely indicated. This map —a most excellent one and derived from the best sources —unfortunately contains one or two other errors, viz., the Ingleton coal-field is wrongly coloured, and the words "upper Silurian" and "lower Silurian" are mismatched in the "references." In the next chapter the small Silurian area is well though shortly described, and we are then very admirably shown that the red conglomerates at the base of the mountain limestone in some places are the shore deposits of the early part of the latter epoch. The *pièce de résistance*, however, of this portion of the book is naturally the chapter which treats on the Carboniferous period, in which also are to be found the chief novelties due to the author. The lie and position of the great masses which form the backbone of the country are admirably given, and all the lately acquired information is incorporated. The lists of fossils, however, from the lower rocks, do not seem to have undergone much critical revision, and remarkably few additions seem to be recorded during the last thirty years. It is interesting to notice that, with respect to the lower coal measures, the author states his conviction that "the tendency of all the evidence in this part of the country is to show that the Gannister series and the Millstone grits form one natural division of the Carboniferous system," though he does not go so far as Prof. Hull in classifying them together. A valuable feature here is a set of comparative sections in different districts in which the locally-named coal beds are correlated, and which gives a very good idea of their changes. Some of the most interesting features of the Yorkshire coals, however, such as the convergence of many seams into the Beeston coal and the peculiar character of the Better bed coal, though mentioned, are scarcely made enough of, but the fossils associated with the latter have been very well worked at. The proof of the unconformity of the permian limestones and the true age of the picturesque Plimpton rocks, once associated with them, is admirably given, and then this part of the work concludes with an account of the superficial deposits, glacial striæ, and the Victoria Cave.

The second part, which, according to the preface, is only the introductory chapter to the complete flora of the Riding, to be produced in another volume, is more interestingly written. The area under consideration is divided into ten districts indicated on a map, and coinciding with the drainage areas. In each of these the relation of the flora to the general physical characters is pointed out, and lists of the most interesting plants found in selected and naturally separate localities are given, accompanied by remarks on the surrounding country. In each we are taken to the head sources of the river which runs through the district, and so pleasantly are we led along its banks that we seem to realise the several beauties of the neighbourhood while we learn their cause, and to collect all the interesting plants whose habitats are so graphically described. Great care seems to have been taken to exclude all doubtful statements, and to show the cause of the occurrence of particular sets of plants, where this is possible. It is therefore thoroughly reliable, and from the scattered nature of the little that had previously been done, contains more new matter than is to be found in the first part.

The book is accompanied by sixteen plates of geological woodcuts, which are rough, and add but little to its value. There are also five plates of coloured sections, mostly founded on those of the Survey.

We must certainly congratulate the authors on the completion of their task, which has been carried out in a creditable manner, and has resulted in a useful and instructive book.

OUR BOOK SHELF

Flowers. By J. E. Taylor. (Hardwicke and Bogue.)

THIS is a compilation in small octavo, illustrated with many familiar woodcuts and coloured plates. A feature is the index of 1,000 references. Very many of these, being only to the merest mention of names, might have been omitted in favour of a glossary of terms, and more especially a list of works of reference.

The principal aim throughout the book is to convey that all the many adaptations of flowers to secure fertilisation are due to a Divine Creator, and not thought out by the plants themselves. The theory of Natural Selection is hardly alluded to.

In glancing through the geological portion errors of fact, such as that palms are oolitic, are seen to be numerous. The confidence too with which the exact succession of dicotyledons in geological time is set out is not warranted by the present state of our knowledge. We read the oft-repeated theory, now stated as fact, that *Apetalæ* preceded *Polypetalæ*, and these *Gamopetalæ*. That this succession really took place, however probable in itself, is, it is well known, far from proved. The actual flowers discovered in the lowest eocene—almost the oldest dicotyledonous flowers known—are *Gamopetalous*, and have been referred to *Porana* and *Symplocos*. The abundance and differentiation of the *Papilionaceæ*, the *Cæsalpinaceæ*, and the *Mimosæ*, show how ancient are the *Polypetalæ*. Any preponderance we may fancy the wind-fertilised *Apetalæ* possess is due to the fact that most of them are forest trees, and it is the leaves of these which form the great mass of the known dicotyledonous floras. Were those divisions really produced in the sequence assigned them, the origin of all alike is far older than the eocene and at present unknown to us: so that even thus the writer of the book is in error.

A most unfortunate selection of illustrative genera of eocene plants has been made. Azaleas did not abound in the eocene, and have never even been met with in it. Neither did the cactus nor aroids, since they have been but recently noticed in the eocene, and then only in England. In like manner the "peculiar" feature attributed to the miocene, its gathering together in the same flora plants now only found at immense distances apart, is not a peculiarity of that formation, since it characterises eocene floras in at least an equal degree. Chapter IV., on the geographical distribution of flowers, deserves especial mention, but must be consulted itself should any one desire to learn (p. 80) how "the *Proteaceæ* became Australian, the magnolias and tulip-trees chiefly North American."

Looking at the more botanical part of the book, it is seen that the explanations of the modifications and appliances of flowers to insure fertilisation are in some cases not treated with the caution the subject requires. To select an instance: the theory that white flowers open more than any others at night, because they are the most visible to moths, seems probable at first sight; but the unscientific reader, to whom the work is addressed, wishing to see for himself, would reject it after his first walk down a hedgerow at eventide, when he found the dog-rose, the white convolvulus, and the daisy, all closed. Why, too, white flowers, if they rely upon their colour to attract, should be also the most powerfully scented, is not explained. It is likely that perfumes would be more necessary to the dark-coloured flowers which are open at night, unless we suppose, which from experience we of course should not, that only white flowers are fertilised by night-flying moths. Persons whose experience of flowers is confined to ordinary English gardens would remember the heliotrope, the mignonette, musk, yellow azalea, wall-flower, rose, coloured pink, hyacinth, violet, scented verbena, scented geraniums, as the most highly-perfumed plants, and would reasonably doubt that any exceptional attractions in this respect belong to white flowers. In comparison with perfume, the white colour may have little to do with it, but Mr. Taylor must have remarked that some law gives vastly superior brilliance to butterflies and day-flying moths and insects, and this law may also require that flowers which only open at night should, like insects which only fly at night, be white or comparatively very subdued in colour. J. S. G.

Elements of Descriptive Geometry. By J. B. Millar, B.E. (Macmillan, 1878.)

THANKS to Messrs. Kempe, Hart, and other writers on Linkages, we are able "Curvo dignoscere rectum," and "Parallels design Sure as Demoivre" could. The title of the work before us shows that it is not concerned with such elementary details as those which most naturally find a place in works on practical geometry. Chasles in describing the aim of Monge's great discovery, says:—"La géométrie descriptive, en effet, qui n'est que la traduction graphique de la géométrie générale et rationnelle, servit de flambeau dans les recherches et dans l'appréciation des résultats de la géométrie analytique; et par la nature de ses opérations, qui ont pour but d'établir une correspondance complète et sûre entre des figures effectivement tracées sur un plan et des corps fictifs dans l'espace, elle familiarisa avec les formes de ces corps, les fit concevoir idéalement, avec exactitude et promptitude, et doubla de la sorte nos moyens d'investigation dans la science de l'étendue." Mr. Millar's book is a very serviceable exposition of the subject as thus described, and he has prefixed a short introduction on solid geometry. A good English text-book on this branch (solid geometry) is yet a desideratum. The plan on which the figures are arranged and drawn is, we think, likely to aid the student in his working out the propositions in the text.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Hughes's Microphone

MR. EDISON finds a resemblance between his carbon telephone and my microphone.

I can find none whatever; the microphone in its numerous forms that I have already made, and varied by many others since, is simply the embodiment of a discovery I have made, in which I consider the microphone as the first step to new and perhaps more wonderful applications.

I have proved that all bodies, solid, liquid, and gaseous, are in a state of molecular agitation when under the influence of sonorous vibrations, no matter if it is a piece of board, walls of a house, street, fields, or wood, sea or air, all are in this constant state of vibration, which simply becomes more evident as the sonorous vibrations are more powerful. This I have proved by the discovery that when two or more electrical conducting bodies are placed in contact under very slight constant pressure, resting on any body whatever, they will of themselves transform a constant electrical current into an undulatory current, representing in its exact form the vibrations of the matter on which it reposes; it requires no complicated arrangement and no special material, and to most experimenters the three simple iron nails that I have described form the best and most sensitive microphone. But these contact points would soon oxidise, so naturally I prefer some conducting material which will not oxidise.

Mr. Edison's carbon telephone represents the principle of the varying pressure of a diaphragm or its equivalent on a button of carbon varying the amount of electricity in accordance with this change of pressure; it represents no field of discovery, and its uses are restricted to telephony.

The three nails I have spoken of will not only do all, and that far better than Edison's carbon telephone in telephony, but have the power of taking up sounds inaudible to human ears, and rendering them audible, in fact a true microphone; besides it has the merit of demonstrating the molecular action, which is constantly occurring in all matter under the influence of sonorous vibrations.

Here we have certainly no resemblance in form, materials, or principles to Mr. Edison's telephone. The carbon telephone represents a special material in a special way to a special purpose.

The microphone demonstrates and represents the whole field of nature, the whole world of matter is suitable to act upon,

and the whole of the electrical conducting materials are suitable to its demonstrations.

The one represents a patentable improvement, the other a discovery too great and of too wide bearing for any one to be justified in holding it by patent, and claiming as his own, that which belongs to the world's domain. D. E. HUGHES

London, July 2

Insects Corroborative of the Nativity of Certain Plants

WHETHER certain plants are, or are not, natives of Britain is a question that often exercises botanists, and any new evidence on the subject is always acceptable. It has recently occurred to me that a certain kind of evidence may be obtained by studying the insects attached to such plants. The question is one of interest not only to phyto- but also to zoo-geographers; for if the species of plant to which an insect is restricted is proved not to be indigenous then the insect cannot be indigenous either. If, on the other hand, the plant is only doubtfully an alien, and the insect is not one that might be easily introduced, then the probability is that the plant is a true native.

The plant that has suggested this idea to me is the wild or yellow balsam, *Impatiens noli-me-tangere*. This plant is reported from twenty-seven counties or vice-counties, but in most of these it seems to be admittedly an "introduction." Mr. H. C. Watson, the indefatigable author of the "Cybele Britannica," &c., seems to think that its claim to being indigenous is very slight, for he writes ("Topographical Botany," part 2, p. 607):—"If the *Noli-me-tangere* be really native here it must be so very locally: say, in North Wales and Westmoreland." Sir J. D. Hooker ("Student's Flora," first edition, p. 80) says, "Probably wild in North Wales, Lancashire, and Westmoreland;" Prof. Babington ("Manual," seventh edition, p. 72) does not mark it is an introduction, but Hooker and Arnott ("British Flora") regarded it with doubt; finally Hudson ("Flora Anglica," 1762, p. 332) thought it in his day truly wild in Westmoreland. It is evident, therefore, that the *Impatiens noli-me-tangere* is looked upon with suspicion by many of the present race of botanists, and probably rightly so in many of the "stations."

There are two species of Lepidoptera attached to this plant, and, I believe, restricted to it. One of these—*Lygris reticulata*—has been for a number of years known as a native of Westmoreland, where, on the banks of Windermere, it occurs very rarely. Its connection with the *Impatiens* in this country was not, however, known till very recently, when Mr. J. B. Hodgkinson, a well-known Yorkshire naturalist, traced it to its headquarters amongst the plant, where he also, still more recently, found the other Lepidopteron—*Penthina postremana*—which is attached to the balsam. Both of these insects are far from common (though *Lygris reticulata* is, like its food-plant, widely distributed—even as far as Siberia), and their occurrence in Westmoreland seems to me conclusive that the *Impatiens* is really indigenous there.

As apparently opposed to my theory, it must not be forgotten that there are several plants, certainly introduced into Britain, which have insects attached and restricted to them. Amongst others are the spruce-fir and the larch. On the spruce the following insects occur: *Eupithecia togata*, *Semasia nanana*, *Asthenia strobilella*, *Coccyx hercyniana*, &c., and on the larch *Eupithecia lariciata*, *Boarmia crepuscularia*, *Spilonota laricana*, *Coleophora laricella*, &c. But it must be remembered that the spruce and larch are perennial trees (while the *Impatiens* is an annual plant), and that they are frequently imported in the form of young trees, or as undressed timber, and sent hither and thither all over the country. Hence the insects attached to them have many chances of being introduced, and of establishing themselves where the conditions are favourable.

It is possible that some of the insects I have last mentioned may have transferred themselves from the native coniferæ to the introduced ones, but I do not think this is likely. A few species live on the introduced as well as the native trees, as, for example, *Myelois abietella*, upon scots-fir and spruce, and the rare beetle *Dendrophagus crenatus*, upon scots-fir and larch, as I noticed when investigating the natural history of Aberdeenshire some years ago.

It is desirable that all the "stations" in which there is any doubt about the introduction of the *Impatiens* should be searched for the insects mentioned above, for it is not likely that they are confined to Westmoreland; and should they be found in any other locality, the probability is, it seems to me, that there the plant is really indigenous. F. BUCHANAN WHITE

Perth, July 5

Physical Science for Artists

SOME years ago, in Madeira, we had been watching a glorious sunset from the hills above Funchal; and, on turning to go eastward, we saw the sky before us suffused with a bright rosy tint, which ended abruptly beyond the Desertas, at some little distance above the horizon-line of the Atlantic.

At first it did not occur to us what was the cold blue-grey form that rose into the pink flush above, slowly losing its definition of outline as it gradually grew higher.

But this strange silhouette had so distinctly mountain outlines that, almost at once, we recognised the fact that we were looking at the shadow of Madeira cast by the setting sun on the mist.

This phenomenon may not be unusual, but I do not recollect having seen it described; and it is perhaps sufficiently different from the phenomena described by Prof. Brücke and Mr. F. Pollock to be worth recording. G. HUBBARD

Remarkable Form of Lightning

I AM able to confirm the fact that lightning occasionally takes the "punctuated" form described by Mr. Joule in NATURE, vol. xviii. p. 260. Some forty years ago, in a thunderstorm which I had the good fortune to witness at Ampton, in Suffolk, the lightning (with heavy rain) was almost incessant for half an hour or more, and about a quarter of the flashes (speaking from memory only) presented this unusual appearance. I have often looked out for it since, but only once with success, and then it only showed itself in a single flash out of many. On both occasions the "punctuated" flashes presented in general a curved or sinuous line without sharp angles; and two or three of them in the first-mentioned storm *appeared* to my eye as closed curves, one an almost perfect figure of 8; but their dazzling brightness made it impossible to speak to this with certainty.

London, July 8 E. J. LAWRENCE

Microscopy. The Immersion Paraboloid

THE immersion paraboloid illuminator exhibited at the recent *soirée* of the Royal Society as designed by me, proves to have been anticipated in principle and construction by Dr. John Barker, of Dublin, from whom a paper on the subject will be found in the *Proceedings* of the Royal Irish Academy for 1870.

An immersion paraboloid illuminator was also described by Mr. Wenham in the *Transactions* of the Royal Microscopical Society for 1856. My paper on the subject appeared in the *Monthly Microscopical Journal* for August, 1877, but that journal being defunct, I ask you to allow me to credit these gentlemen with a priority which, on perusing their papers, I find to be due to them. I ought to add that, until the construction by Messrs. Powell and Lealand of my illuminator, the device had never come into practical use, and that, so far as I can learn, no reference to it exists in any optician's catalogue or text-book on the microscope. JAMES EDMUNDS

8, Grafton Street, Piccadilly

Review of Henfrey's Botany

ALLOW me to correct an error which Mr. Bennett has made in his review of "Henfrey's Elementary Course of Botany" (NATURE, vol. xviii. p. 217). He adds a note as follows:—

"Evidently by an error of the press, the continued fraction of which the most common angles of divergence are successive convergents, is given as $\frac{1}{2}+\frac{1}{1}+\frac{1}{1}$, instead of $\frac{1}{2+\frac{1}{1+1}}$, &c., a correction needful to render the sentence intelligible to the student."

My note (p. 44) is as follows:—

"The mathematician will observe that these fractions are the successive convergents of the continued fraction $\frac{1}{2}+\frac{1}{1}+\frac{1}{1}$, &c."

I subjoined it for the sake of *mathematical* students only, who would know what Mr. Bennett does not seem to be aware of, that the method of writing the continued fraction I have adopted,

is simply a convenient way of expressing it in one line; and it is *not* printed as he has misquoted it above.

GEORGE HENSLOW

6, Tichfield Terrace, Regent's Park, N.W.

[I am obliged to my friend Mr. Henslow for correcting my oversight in not accurately noting the form of his formula. The fact, however, that the sentence is, as, Mr. Henslow admits, put in a form which is adapted for "mathematical students only," in a work intended for beginners, seems to furnish a strong justification of the main point of my criticism.—A. W. B.]

Alumina

IT may interest your readers to know that pure alumina dissolved nearly to saturation before the blowpipe in an *acid* flux, such as a bead of phosphoric acid, invariably causes that to assume a pale but beautiful sky blue on cooling.

In an *alkaline* flux such as a bead of boric acid containing sufficient soda to dissolve it to saturation, alumina causes the bead to assume a pale red colour on cooling.

The greatest care has been taken to ascertain that the materials are absolutely free from any metallic or other oxide which might produce such colours, and the resulting beads have been shown to several gentlemen, as Messrs. Hunt and Roskell, Mr. Hutchings of Freiberg, and others.

Might not these facts then afford us some clue (so much wanted) to the cause of coloration in the sapphire and ruby?

London, July 1 W. A. ROSS

A Subject-Index to Scientific Periodical Literature

I HAVE been occupied for years in drawing up a classified index, not only to the titles of papers, but to what is still more wanted, to the facts contained in those papers. As yet I have met with scant encouragement. A. RAMSAY

Kilmorey Lodge, 6, Kent Gardens, Ealing, W., July 8

CLUB-ROOT

ALL our readers who are agriculturists or practical gardeners will be familiar with the disease called in England "Club-root," or "Finger and Toes," or "Clubbing." It seems almost to confine its ravages to cruciferous plants, and often causes great destruction to large crops of turnips, cabbages, cauliflowers, not to mention what disappointments it may occasion to the growers of wallflowers, Brompton stocks, candytufts, and many other favourite flowers belonging to this large natural family. Not only is it well known, but it has often been written about, as the pages of our contemporary, the *Gardeners' Chronicle*, and most works on the cultivation of gardens, will abundantly prove.

The question of what did it consist of was often asked, and the answer was that it was caused by some insect or another, and some poor beetles and flies were signalled out as those which laid their eggs in the tissues of the young roots of the plants attacked, and, if we are not mistaken, this is the general belief to this present moment. The explanation never was, however, satisfactory. True, in the advanced stage of this disease insect larvæ were to be found in the club-like swellings of the roots; but in the very early stages no trace of larva or egg of any insect was to be seen, and yet the club-root disease was clearly there.

In the *Botanische Zeitung* for May 14, 1875, there appeared a short abstract of a paper read by M. Woronin, before the Botanical Section of the Natural History Society of St. Petersburg, on the 5th of March of the previous year, on the cause of this disease, and within the last few weeks we have received the full memoir, illustrated with upwards of fifty figures. This memoir is in Russian, but, thanks to a colleague (Prof. R. Atkinson), the writer has been able to glean a notion of its most interesting contents, in which he has been much assisted by the beautiful figures. The disease is known in Russia as "Kapustnaja Kila" (Kapusta = Cabbage, Kila = Hernia). About three years since it was so extremely prevalent that the vegetable crops about St. Petersburg failed, and the government ordered an investigation, from which much information was obtained as to the means adopted in different countries for its cure: such as sowing the ground, before planting the crop, with common salt, wood ashes, or, before all, soot. Every one knows, too, that in transplanting the young crucifers into their permanent beds that it is customary to pinch off the swollen portions, and then, if favourable weather followed, the newly-formed roots could well keep ahead of any fresh appearance of the disease. But M. Woronin went scientifically to work, and he was not long in discovering that the cause of the disease was a parasitic vegetable which seemed to have some affinities with the Myxomycetes on the one hand, and the Chytridiaceæ on the other, and the result of constant researches carried on through 1875, 1876, and last year, have resulted in nearly the whole life-history of this new plant being discovered. It is called *Plasmidiophora brassicæ*, and is decidedly very nearly allied to the Chytridia, but the new forms of this group daily coming to light, appear so different in their development, that much more must be known about them ere any satisfactory classification can be attempted. One most striking feature in the new plant is indicated by its generic name; this will be best understood by a short history of the plant's life. Take an old well-developed knob off a club-root, and examine the tissue; most of the parenchymatous cells will be found enlarged, their starchy contents gone, and they themselves gorged with a mass of spore-like bodies; by the ordinary disintegration of the cellular tissue these spores will get released, and after a lapse of six days, out of each spore will proceed the whole of the contents, which, colourless, but nucleated, will move about like so many minute amœba; these plasmodia will then attach themselves to the delicate root-hairs of the nearest young cruciferous seedling. One end of the plasmodium is attenuated like a cilium. The spores soon penetrate into the cells, where they will look just like Myxamœbæ. Filling the cells up with delicate plasmodic projections, they will next soon develop lots of spores, which will further contaminate the cellular tissue of the root, and in process of time the formation of the clubbing will be seen.

Sometimes the ripe spores are spherical, sometimes they are twin-like, or lenticular. If cabbage or turnip seeds be sown in a watch-glass and supplied with distilled water, and shortly after the first appearance of germination, a number of spores of *Plasmidiophora brassicæ* be added to the water, these will be found to at first float freely in the water, but sooner or later will sink and attach themselves to the delicate root-hairs of the little seedlings, and in this way their whole history, so far as now known, may with facility be traced. It seems noteworthy that the whole mass breaks into spores all at once, as in Chytridium proper. There would seem to be as yet no conjugation detected, and the plasmodia would appear as if they absolutely engulfed the starch granules on which they feed.

It must be a matter of regret that this memoir is written in a language known unfortunately to so few scientific botanists. If the learned author knew only Russian it would be absurd and unreasonable to record this regret, but to one knowing French and German, as M. Woronin does, it would have been no trouble to have increased a hundredfold the grateful readers of this important memoir.

E. PERCEVAL WRIGHT

SCIENCE IN SCHOOLS

THE following article on Sir John Lubbock's Bill on the introduction of science in elementary schools appears in Monday's *Times*:—

The rejection of Sir John Lubbock's motion for the addition of elementary science, or, rather, as the matter was more happily put by Dr. Lyon Playfair in the course of the debate, of elementary knowledge of common things, to the subjects for which grants are given under the education code, although an inevitable and foregone conclusion, is not on that account the less to be deplored. As happens in many similar cases, the argument was all on the side of the minority, and Lord G. Hamilton, in opposing the suggestion on the part of the Privy Council, was only able to say that its adoption would, perhaps, entail some temporary uncertainty about the subjects in which inspectors would be required to examine and children to pass. If schools existed for the convenience of inspectors, or even in order that children might not be troubled by uncertainties, the objection would have been a valid one; but upon any other supposition it seems to tell against, rather than in favour of, the contention which it was intended to support. The nation is spending large and rapidly increasing sums of money upon schools, and it will every year become a matter of greater urgency that these sums should not be misapplied, either by the omission from the code of subjects which would be useful or by the inclusion of others which have no apparent tendency to promote the attainment of the ends to which education is supposed to be directed. These ends, in the case of a peasant child, are presumably to render him a more useful and a better conducted member of society than he would become by the unaided light of nature; and it is obvious that the means to their attainment are twofold—first, to cultivate the intelligence in such a way as to facilitate the acquirement and the application of knowledge; and, secondly, to impart the knowledge which has to be applied. Until a comparatively recent time, however, the imparting of knowledge was considered to be the sole purpose of education and to be in itself the best means of mental training; so that educationists occupied themselves more about the seed than about the soil, and were chiefly concerned to teach those things which they thought it most important that a child should know. The instruction given to the poor for many years was almost limited to reading, writing, arithmetic, and elementary religious instruction, while that imparted to the rich was laid upon the same foundation, and was only carried further because the pupils had more time at their disposal. In the employment of this time the instructors could only teach what they knew; the most famous public schools and the two great Universities restricted themselves to giving their pupils some knowledge of classics and mathematics.

As soon as physiologists had discovered that all the faculties of the intellect, however originating or upon whatever exercised, were functions of a material organism or brain, absolutely dependent upon its integrity for their manifestation, and upon its growth and development for their improvement, it became apparent that the true office of the teacher of the future would be to seek to learn the conditions by which the growth and the operations of the brain were controlled, in order that he might be able to modify these conditions in a favourable manner. The abstraction of the "mind" was so far set aside as to make it certain that this mind could only act through a nervous structure, and that the structure was subject to various influences for good or evil. It became known that a brain cannot arrive at healthy maturity excepting by the assistance of a sufficient supply of healthy blood—that is to say, of good food and pure air. It also became known that the power of a brain will ultimately depend very much upon the way in which it is habitually exercised, and that the practice of schools in this respect left a great deal to be desired. A large amount of costly and pretentious teaching fails dismally for no other reason than because it is not directed by any knowledge of the mode of action of the organ to which the teacher endeavours to appeal; and mental growth in many instances occurs in spite of teaching rather than on account of it. Education, which might once have been defined as an endeavour to expand the intellect by the introduction of mechanically compressed facts, should now be defined as an endeavour favourably to influence a vital process; and, when so regarded, its direction should manifestly fall somewhat into the hands of those by whom the nature of vital processes has been most completely studied. In other words, it becomes neither more nor less than a branch of applied physiology; and physiologists tell us with regard to it that the common processes of teaching are open to the grave objection that they constantly appeal to the lower centres of nervous function, which govern the memory of and the reaction upon sensations, rather than to those higher ones which are the organs of ratiocination and of volition. Hence a great deal which passes for education is really a degradation of the human brain to efforts below its natural capacities. This applies especially to book work, in which the memory of sounds in given sequences is often the sole demand of the teacher, and in which the pupil, instead of knowing the meaning of the sounds, often does not know what "meaning" means. As soon as the sequence of the sounds is forgotten nothing remains, and we are then confronted by a question which was once proposed in an inspectorial report:—"To what purpose in after-life is a boy taught if the intervention of a school vacation is to be a sufficient excuse for entirely forgetting his instruction?"

In order to avoid such faulty teaching, few agencies are more valuable than what are technically called "object" lessons, in which the faculties of the pupils are exercised about things instead of about words; and the suggestion of Sir John Lubbock would lead to object lessons of a very useful character. To be taught something about gravitation, about atmospheric pressure, about the effects of temperature, and other simple matters of like kind, which would admit of experimental illustration, and which would call upon the learner to make statements in his own words instead of in those of somebody else, would be so many steps towards real mental development. At the end of a vacation, even if the facts of any particular occurrence had become somewhat mixed, the pupils would nevertheless preserve an increased capacity for acquiring new facts, and would probably retain these for a longer period; and such are precisely the changes which it should be the province of education to bring about. We would even go further than Sir John Lubbock, and in elementary schools would give an important place to the art of drawing, which teaches accurate observation of the forms of things. The efforts of a wise teacher should always be guided with reference to the position and surroundings of a child at home, and should seek to supplement the deficiencies of home training and example. Among the wealthier classes the floating information of the family circle often, though by no means always, both excites and gratifies a curiosity about natural phenomena; but among the poor this stimulus to mental growth is almost, if not entirely, wanting. An explanation of the physical causes of common events, such, for instance, as the rising of water in a pump, would usually be a revelation to the pupils of a Board School, and would start them upon a track which could hardly fail to render them more skilful workers in any department of industry, and which might even lead some of them to fortune. A wise and benevolent squire set on foot many years ago a school for the children of his labourers, in which drawing and the elements of natural science were carefully taught; and the result was that the children educated there, instead of remaining at the plough's tail, passed, in an astonishingly large number of cases, into positions of

responsibility and profit. On every ground, therefore, we hope that Sir John Lubbock's proposal will at no distant time be adopted by Parliament; but in the meanwhile there is a still more important department of teaching which is wholly neglected, and concerning which the deficiencies of home instruction are at least equally manifest. We refer to a proper knowledge of the influence of conduct upon life. It should be the duty of every schoolmaster to try and make his pupils understand how production—that is to say, industry—leads to wealth, and how destruction—that is to say, idleness—leads to poverty. The reason why confidence in others is necessary to all enterprise, and the reason why honesty, in the largest sense of the word, is the only root of confidence, should in like manner be enforced by precept and illustrated by example; and such teaching, if it could only be made general, would do more to heal the breach between capital and labour than all the panaceas of all the politicians who have ever sought to figure as the "friends of the working man."

OUR ASTRONOMICAL COLUMN

TEMPEL'S COMET, 1873, II.—Up to the time of writing it would appear that this comet has escaped detection. Even if there be no great error in the calculated position, its faintness must render discovery difficult in the summer skies, but it may be hoped nevertheless that a vigorous effort will be made in the next period of absence of moonlight to recover the comet, as in the event of want of success in the present year, it will be probably lost, or in the same case as the short-period comet of De Vico of 1844, which, being missed at the second return in 1855, has not been again observed. M. Schulhof has communicated to the French Academy a further ephemeris of Tempel's comet, from which are extracted the places subjoined:—

At Paris midnight.	R.A. h. m.	N.P.D. ° ′	At Paris midnight.	R.A. h. m.	N.P.D. ° ′
July 13	15 31·6	93 18	July 29	15 47·6	100 8
,, 17	15 34·4	94 55	Aug. 2	15 53·8	101 57
,, 21	15 38·0	96 36	,, 6	16 0·8	103 46
,, 25	15 42·4	98 21	,, 10	16 8·6	105 36

During this interval the comet's theoretical intensity of light will be only three times that it possessed at the date of the last observation in 1873, when it was the faintest object that could be observed in a dark field with a 7-inch refractor. A few days' difference in the date of perihelion passage, which is probable enough, changes the geocentric path materially, so that the search must be extended to some distance on each side of the calculated place for the day of observation.

In its present orbit the comet cannot approach the planet Jupiter within 0·62, and with M. Schulhof's period of revolution it is easy to see that there will be no near approximation of the two bodies during the next twenty years—in such case the perihelion passages must always occur at a season of the year when observations of the comet would be barely, if at all, practicable. Hence an additional reason for a very close search in the present summer.

THE "TEMPORARY STARS" OF KEPLER AND ANTHELM.—The objects observed by Kepler in 1604 and by Anthelm in 1670, which Sir John Herschel was wont to describe as "temporary stars," but which there is, nevertheless, reason to believe to be still visible as telescopic stars, will not escape the attention of observers who are interested in the variables, at this season. As mentioned some time since in this column, Prof. Winnecke remarked, in 1875, a star of the twelfth magnitude on his scale, which is very near the calculated place of Kepler's famous star, and to the place of a star entered upon Chacornac's Chart, No. 52, as a *tenth* magnitude. We are able to state that no star was discernible in this position with 7-inches aperture on several occasions in 1872-74. The position of Winnecke's star for 1855·0 is in R.A. 17h. 21m. 49·3s., N.P.D. 111° 19′·3; it therefore precedes No. 16,872 of Oeltzen's Argelander by 33s. and is north of it 2′: Argelander's star is of 8·9m. and the best reference point in examination of the neighbourhood. For 1870·0 we have:—

	R.A. h. m. s.	N.P.D. ° ′	
Kepler's star 1604	17 22 51	111 22·0	Schönfeld's reduction from observations of Fabricius.
Chacornac's star 10m.	17 22 43	111 22·5	Read off from his chart, therefore only approximate.
Winnecke's star 12m.	17 22 43	111 20·8	Observed at Strasburg.
Argelander's star 8·9m.	17 23 16	111 22·8	

There is also a star of about 12m. in R.A. 17h. 22m. 57s., N.P.D. 111° 24′·4, and therefore as near to the calculated position of Kepler's star as Winnecke's object, which has not shown any variation during several years. The difference of magnitude noted by Chacornac and Winnecke rather points to their star as the one to be closely watched.

The place of the star discovered by Anthelm in 1670 has been calculated from the observations of Picard and Hevelius by Prof. Schönfeld, and from those of Picard only (as given in the *Histoire Céleste* of Lemonnier) by Mr. Hind, their results differing only 2s. in R.A., and 0′·4 in N.P.D. The telescopic star 11·12m., which is now visible almost in the same position, was meridionally observed at Greenwich in 1872, the result for 1880·0 being R.A. 19h. 42m. 45·1s., N.P.D. 62° 58′ 32″. Variation extending to more than one magnitude has been remarked in this object, during the last twenty-five years, thus, with the near coincidence of position affording strong indication that it may eventually prove to be the star which suddenly brightened up in 1670. A star of similar magnitude follows it 12·5s., about 3′ to the north, and another follows at 22·5s., about 2′ northerly. In the years 1872-74 the presumed star of Anthelm was judged to be at times sensibly equal to the first of these stars following it, at others decidedly fainter—even at the first glance.

JEREMIAH SHAKERLEY.—The transit of Mercury on November 2, 1651, it will be remembered, was predicted by Jeremiah Shakerley, a young devotee of astronomy, who, finding by the tables in his hands, apparently founded upon the observations of Horrox, that it would not be visible here, undertook the, at that period, great voyage to India for the purpose of witnessing the phenomenon, which he observed at Surat. Vincent Wing mentions this circumstance in his *Astronomia Britannica*, where the following passage occurs:—"Hanc conjunctionem prædixit idem D. Shakerlœus in *Colloquio* seu *Disceptatione*, *De Mercurio in Sole Videndo*, et postea ipse transmigrans in *Indiam*, conjunctionem hanc insignem ibi videbat, eamque amicis in *Anglia* communicavit, ut patet ex Literis ad *Christophorum Townlœum*, *Henricum Osbornum*, *Londinensem*, aliosque missis."

No work of Shakerley's exists in the libraries of the British Museum, the Royal Observatory, or the Royal Astronomical Society. His *Tabulæ Britannicæ* are in the possession of the Royal Society, and we believe are also found in the Cambridge University Library. The immediate object of this note is to inquire if any reader of NATURE has met with the other works of Shakerley mentioned by Lalande in his *Bibliographie*, or with a publication in which the transit of Mercury in 1651 was predicted.

THE GENESIS OF LIMBS

WHY are our limbs so much alike and yet so different? What do our limbs stand for as compared with the bodies of other animals? Whence have limbs such as ours arisen? What *is* a limb?

The word *limb* is the Anglo-Saxon word *lim*, most probably connected with the Latin *limbus*—the border, outer edge, or extremity of anything, and thence applied to any attached, projecting, or out-lying portion.

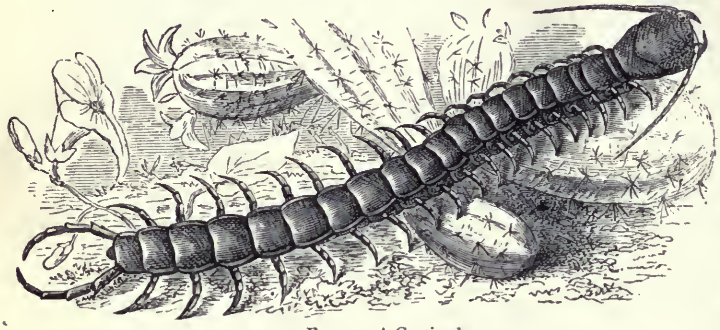


FIG. 1.—A Centipede.

But there are projecting portions of animal bodies essentially like our own body which are not called limbs, *e.g.*, the dorsal and anal fins of fishes, while yet that name is freely bestowed upon structures which have no relation to our limbs save a relation of analogy from similarity of use, as, *e.g.*, the legs of insects or the arms of star-fishes. Insects and their allies present certain resemblances and differences carried to a higher degree than in us, and which may be here adverted to. The difference in shape between the limbs of the right and left sides in us is minute and accidental. Our *bilateral* symmetry is complete, but in many crustaceans the shapes of the right and left great claws differ to a large extent. The resemblance between the thoracic and pelvic limbs in us is great, but the resemblance between the successive legs of many arthropods is much greater, especially in the class of centipedes, where the successive segments of the body, with their appendages, exhibit *serial* symmetry carried to the highest degree. The amount of likeness, as regards *serial symmetry*, which exists between our pairs of limbs is less than exists in many back-boned creatures, while at the same time there are a great many others in which it is not carried nearly so far as it is in ourselves. These varying degrees of serial symmetry are such that upon the theory of evolution we must suppose that if this serial symmetry originally existed, it must have been lost and reacquired perhaps several times to produce what we see before us in the existing creation.

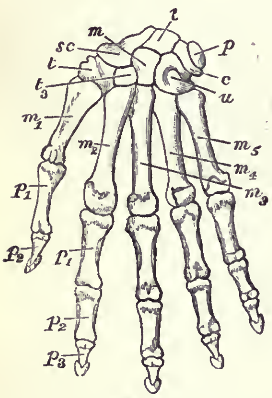


FIG. 2.

FIG. 3.

FIG. 2.—Anterior (palmar) surface of the skeleton of man's hand —*c*, cuneiforme: *l*, lunare; *m*, magnum; m¹, metacarpal of thumb; m^2–m^5, metacarpals of the four fingers; *p*, pisiforme; P^1, first phalanx of the thumb and four fingers—*i.e.* of the five "digits;" P^2, second phalanx of the five digits; *P*3, third, or ungual phalanx; *sc*, scaphoides; *t*, trapezium; *tz*, trapezoides; *u*, unciforme.

FIG. 3.—Dorsum, or upper surface, of skeleton of right foot.—*a*, astragalus; c^1, ento-cuneiforme; c^2, meso-cuneiforme; c^3, ecto-cuneiforme; *ca*, calcaneum; *co*, cuboides; *h*, distal phalanx of hallux; m^1, metatarsal of hallux; m^2–m^5, metatarsals of the four outer toes; *n*, naviculare.

Thus if we compare with the structure of the human hand and foot the same parts in apes, we find that in them the toes (or digits of the foot), instead of being short like ours, are long and mobile like our fingers, while the great toe (or hallux) is set out at an angle from the others, to which it is powerfully opposable. At the same time the main points of structure of the ape's foot remain like our own, and thus while it is morphologically a foot, it is functionally more or less of a hand. Here, therefore, serial symmetry is already more complete than in us.

If we descend to hoofed beasts, *e.g.*, the hog, the giraffe, or the horse, we find the number of digits equally and simultaneously reduced in both the fore and the hind limb, and while in the two former creatures the third and fourth digits of each extremity are increased in size at the expense of the others, in the horse there is but one digit so increased—the animal walking upon but four digits, which answer respectively to our two middle fingers and our two middle toes.

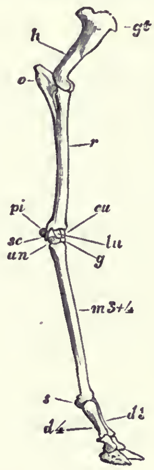


FIG. 4.

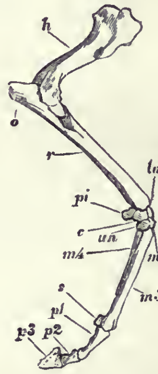


FIG. 5.

FIG. 4.—Right pectoral limb of a Giraffe.—*cu*, scaphoides; *d*3, proximal phalanx of third digit; d^4, proximal phalanx of fourth digit; *g*, magnum; *gt*, great tuberosity of the humerus; *h*, shaft of the humerus; *lu*, lunare; m^{3+4}, united metatarsals of third and fourth digits; *o*, olecranon; *pi*, pisiforme; *r*, radius; *sc*, cuneiforme; *un*, unciforme.

FIG. 5.—Right pectoral limb of Horse.—*c*, cuneiforme; *h*, humerus; *lu*, lunare; m^3, metacarpal of the third digit—the only one fully developed; m^4, rudimentary fourth metacarpal; *mg*, magnum; *pi*, pisiforme; p^1, proximal phalanx; p^2, middle phalanx; *p*3, third or ungual phalanx; *s*, sesamoid; *un*, unciforme.

It is, then, quite a mistake to regard the ox's hoof as answering, morphologically, to the horse's hoof "cloven;" each single hoof of the horse answers only to the inner division of each double hoof of the ox or giraffe. Now in all these creatures we find a still further increase in serial symmetry as compared with the apes and man.

If, however, we turn to such an animal as the mole we find a much decreased degree of such symmetry, the fore-limb being of enormous strength, with its bones shortened and broadened out, while the hind-limb is slender and deli-

FIG. 6.

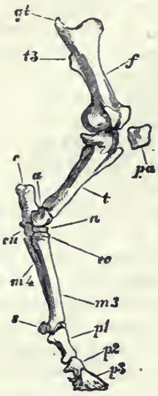


FIG. 7.

FIG. 6.—Right pelvic limb of Giraffe.—*a*, astragalus; *c*, calcaneum; *cu*, cuboides; *d*3, proximal phalanx of third digit; *d*4, proximal phalanx of fourth digit; *f*, femur; *f'*, rudiments of fibula (the line is not continued far enough—the rudimentary fibula is a small ossicle reposing on the upper surface of the calcaneum, as shown in the figure); m^{3+4}, metatarsals of digits 3 and 4 united into one "cannon-bone"; *pa*, patella; *t*, tibia.

FIG. 7.—Skeleton of right pelvic limb of Horse.—*a*, astragalus; *c*, calcaneum; *cu*, cuboides; *ec*, ecto-cuneiforme; *f*, femur; *gt*, great trochanter; *m*3, metatarsal of third digit; m^4, rudimentary fourth metatarsal; *n*, naviculare; *pa*, patella; p^1, p^2, and p^3, first, second, and third phalanges of the third and only digit; *s*, sesamoid; *t*, tibia; *t*3, third trochanter.

cate. The mole works underground with such exceeding rapidity that it has been said to fly beneath the soil, but in the beast which really does fly—the bat—serial sym-

FIG. 8.—Hand of Bat (*Pteropus*).—m^1–m^4, metacarpals of the four fingers; *p*, pollex, with a very short metacarpal; *sc*, scaphoides.

metry is still less developed. The framework of the bat's wing consists of the very same bones which exist in the human arm and hand, only exceedingly elongated and slender. The four fingers—wonderfully drawn out—are connected together (and to the body and legs) by a delicate web of skin. The foot is a striking contrast to the enormously enlarged hand, being small in size with short toes. And yet, though serial symmetry is thus disguised in the bat, it nevertheless shows itself in other ways more or less noteworthy. The outer bone of the fore-arm—the *ulna*, is incompletely developed, and the corresponding bone of the leg—the fibula, is also incompletely developed. But much more than this, in some bats we find outside the

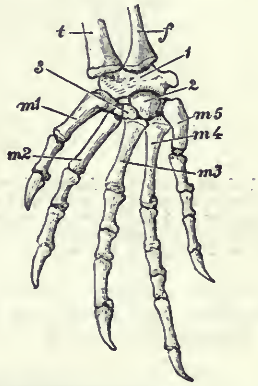


FIG. 9.—Left foot of a Monitor Lizard (*Varanus*).—*f*, fibula; m^1–m^5, the five metatarsals, m^1 being that of the hallux; *t*, tibia; 1, astragalo-calcaneum; 2, cuboides; 3, ecto-cuneiforme.

elbow-joint a distinct and separate little bone which quite answers to the knee-pan (or *patella*) of the leg—a most exceptional case of serial homology.

The creatures just referred to are all mammals, but birds and reptiles present us with some instructive examples both of serial homology and discrepancy. In ourselves and in all beasts, the motion of the foot upon the leg takes place between the long bones of the latter (tibia and fibula) and the tarsus. In the crocodile, or

FIG. 11.—Right foot of Emeu.—*a*, astragalus; d_2–d_4, second, third, and fourth digits; *m*, metatarsals anchylosed together except at their distal ends; *t*, tibia; t_2, distal tarsal element.

monitor, it is not so, but the upper part of the ankle, or tarsus (answering to our astragalus and os calcis), is firmly and immovably fixed to the leg bones, while the lower part of the tarsus is firmly fixed to the metatarsals. Thus in the crocodile, or monitor, motion does not take place between the whole ankle and the leg, but in the middle of the ankle (or tarsus) itself.

In the leg of a bird there at first sight seems to be no

tarsus at all, nor any bones which we can with certainty call "metatarsal." We have only one single long bone, at the lower end of which are three or four articular surfaces for the three or four toes. The study of the very young bird, however, has shown us that though no tarsus can be distinguished in the adult, yet such a part does exist for a certain brief period of the bird's life and then disappears.

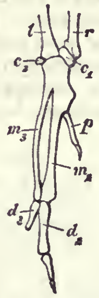


Fig. 10.—Right hand of Ostrich.—c^1, radial carpal ossicle; c^2, ulnar carpal ossicle; d^2, proximal phalanx of the index digit which has three phalanges; $d3$, phalanx of third digit; l, ulna; m^2 and $m3$, metacarpals of second and third digits anchylosed together and with that of the pollex; p, proximal phalanx of pollex; r, radius.

In its fate we have an interesting resemblance to the condition which we have already found existing in the crocodile, and which condition the bird exaggerates. The upper part of the tarsus becomes not merely firmly fixed to, but indistinguishably united with, the leg-bone, or tibia, while the lower part of the tarsus becomes as indistinguishably united with the coalesced metatarsals, and thus it comes about that no tarsus whatever is distinguishable in the adult. The apparent leg-bone (tibia) is leg-bone with part of the tarsus also; the apparent metatarsal bone is made up of metatarsal bones with the other part of the tarsus also. The movement of the foot on the leg takes place in the bird (as in the crocodile), not between the leg and ankle, but in the middle of the ankle itself.

In the skeleton of the bird's fore-limb, or wing, the hand is strangely different in aspect from the foot. There is hardly any carpus (or wrist) visible. The metacarpus is represented by a single complex bone formed of three metacarpals anchylosed together, and there are only three fingers, which are all more or less rudimentary.

Here serial symmetry is more disguised than ever in the bat, the difference between a bird's wing and a bird's leg being so great. And yet even here we meet with a curious example of the tendency to vary similarly which exists in serially homologous parts; for in the bird's carpus there is a similar arrangement, though less thoroughly carried out, to that which exists in the bird's tarsus. The distal part of the carpus coalesces altogether with the metacarpus (as the distal part of the tarsus does with the metacarpus), but the proximal part remains distinct in the form of two separate carpal bones.

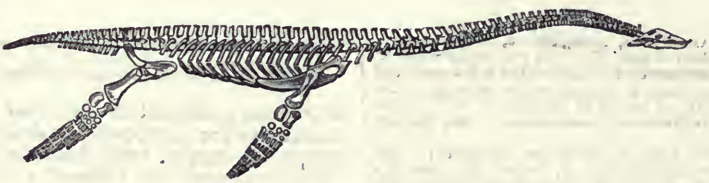


Fig. 13.—Skeleton of a Plesiosaurus.

(*To be continued*)

St. George Mivart

THE OBSERVATORY OF PARIS

ARRANGEMENTS for the future management of the Observatory at Paris are now complete. Contre-Amiral (until recently Captain) Mouchez is appointed director, with M. Maurice Lœwy as sub-director—these appointments taking effect for five years.

M. Mouchez was born at Madrid in 1821, but is the son of French parents. He joined the Naval School at Brest in 1837, and in 1839 commenced his nautical career in the *Fortune*, which was taking part in the blockade of Buenos Ayres. In 1840 he was appointed to the *Favorite*, which proceeded on a circumnavigating expedition extending over five years. Having shown an early aptitude for astronomical observations, he was intrusted with them. On this voyage he became aware of the imperfect determination of the latitudes and longitudes of some of the sea-ports visited, and his attention was directed to the construction of portable instruments for improving them. In 1850 he embarked on board the *Capricieuse*, also destined for a scientific voyage round the world, which, like that of the *Favorite*, occupied five years. He was charged by the Dépôt de la Marine with the survey of the Rio de la Plata and the Brazils, a survey which extended over about 3,000 miles.

In 1860 M. Mouchez was commissioned by the French Government to visit England, for the purpose of reporting upon the system of weather predictions organised by the late Admiral Fitzroy, Leverrier at the time contemplating the establishment in France of his own system of storm-warnings. M. Mouchez, who was enthusiastic in favour of the Fitzroy arrangements, suggested that the *Dépêche anglaise* should be posted in the French ports, and recommended a special meteorological organisation independent of the Observatory at Paris. The proposition, which was carried into effect, is said to have created differences between Leverrier and the Minister of Marine. M. Mouchez greatly interested himself from an early period in his naval career in promoting astronomical and physical studies amongst the officers of the Government marine, and observations while at sea. His views are noticed by Arago in his introductory work for scientific travellers.

In 1867 he commenced the survey of the coast of Algeria, a work which, in consequence of repeated interruptions from his employment on other urgent missions, was not completed until 1877. Thus in 1870, Contre-Amiral Mouchez was sent with the French fleet to the Baltic for the blockade of the Prussian coasts, but the attempted blockade proving a failure, he was recalled and charged with the defence of Havre, which place he succeeded in preserving from a threatened hostile occupation.

In 1874 he was sent to the Island of St. Paul for the observation of the transit of Venus, and next to that of M. Janssen his mission may be considered the most successful. At his suggestion the French Government established, in 1875, an observatory at Montsouris, where naval officers are practised in making astronomical observations, as also intending travellers, on the recommendation of the Société de Geographie. He is a member of the Academy of Sciences in the section of Astronomy,

and of the Bureau des Longitudes, and was for some time a member of the Council of the Observatory. It is understood that M. Bardoux suggested the adoption of the system in operation at the United States Naval Observatory at Washington, and to model the great Paris Observatory after that institution; and as stated above, the appointment of Admiral Mouchez as director, and M. Lœwy as sub-director, are at present intended to be limited to five years, with the same restriction as to future nominations.

M. Maurice Lœwy, who was born at Pesth in 1834, commenced his astronomical career at the Imperial Observatory of Vienna, under the late Prof. Carl von Littrow, on whose recommendation he was transferred to the Observatory of Paris by Leverrier, in 1860. At Vienna he was much occupied with the calculation of the orbits of comets, including the great comet of Donati in 1858, for which body he was one of the first to establish elliptical elements. He succeeded Laugier, as one of the astronomers of the Bureau des Longitudes in 1872, and since 1874 has been charged with the preparation of the *Connaissance des Temps*, the French national ephemeris, and the *Annuaire*, works which have greatly benefited by his energetic superintendence. Under Delaunay's rule, M. Lœwy occupied the position of sub-director of the Observatory of Paris, charging himself with the meridian observations.

The installation of Admiral Mouchez took place on Saturday by the Council of the Observatory, of which M. Dumas is president.

PROF. W. M. GABB

We greatly regret to hear of the death from consumption, on May 30, at his residence in Philadelphia, of Prof. William M. Gabb, who for many years has occupied a very prominent place among American naturalists.

He was born on January 20, 1839, in Philadelphia, and was educated at its High School, being one of the many graduates of whom that institution had reason to be proud. As a boy he was especially interested in mineralogy and palæontology, and at an early age was so fortunate as to secure an engagement with Prof. James Hall, where he had ample opportunity of indulging his tastes. Returning to Philadelphia, he became a member of the Academy of Natural Sciences, and soon commenced the critical study of the fossil invertebrates of the United States, especially those of the cretaceous formation.

In 1860 he entered the service of the Geological Survey of California, under Prof. J. D. Whitney, but returned to the East in 1868, and undertook the geological survey of their lands for the Santo Domingo Land and Mining Company, which, however, was made to cover a considerable portion of the Dominican Republic, and to which he subsequently made several successive visits for the purpose of continuing his work.

During his connection with the Geological Survey of California he made an extended exploration of the peninsula of Lower California, collecting much important geological and biological material.

In 1873 he became connected with the Costa Rican Government, undertaking a general geological and topographical survey of its territory, and combined with it very extensive researches into its natural history and ethnology, sending his collections to the National Museum in Washington. This labour occupied him for about three years. The results of his work have been given to the public in various forms. A full account of the topography, with an elaborate map, appeared in Petermann's *Mittheilungen*, and a paper on the ethnology of the native tribes, published by the American Philosophical Society, is one of standard value.

In the autumn of 1876 he revisited San Domingo, returning to the United States in March last. For many years he has been threatened with pulmonary disease, the extension of which has been checked by his abode in subtropical regions. The unfavourable symptoms, however, increased of late, and he succumbed shortly after his return to Philadelphia.

Dr. Gabb left an extensive manuscript on the geology and palæontology of Costa Rica, which will be published ere long under competent supervision, thus closing a career of energy and activity, not only in the prosecution of researches, but in the elaboration of their results, which has been seldom equalled by a man of his age. It is very rare, indeed, that one man has accomplished so much in so many distinct branches—in geology, geography, palæontology, ethnology, &c.—as the subject of our present notice.

ON THE ANATOMY OF THE ORGAN OF HEARING IN RELATION TO THE DISCOVERY OF THE PRINCIPLE OF THE MICROPHONE OF PROF. D. E. HUGHES, AND THE MAGNOPHONE OF MR. W. L. SCOTT, A.S.T.E.[1]

The two gentlemen whose names appear in the heading of this paper seem to have arrived at the same important result, viz., the extraordinary effect of mobile particles in transmitting sound under certain conditions, by quite independent research. In perusing the interesting accounts of the *microphone* in several scientific journals, but especially an article in the *Electrician* for May 25, in which number also will be found Mr. Scott's statement of the principle, it occurred to me that the transmitting power of the *otoconia* and *otoliths* in the ears of animals bore very pertinently upon this question. We find *otoconia*, or numerous minute particles in all the *Vertebrata*, with perhaps the exception of the bony fishes which have single concretions, or the union of many in one. *Otoconia* are also found in the *Tetrabranchiate Cephalopoda* (*Nautilus*, Fig. 1), the whole of the *Pteropoda*, in the *Pulmonifera inoperculata*, or rather the bisexual *Pulmonifera* (snails and slugs, Fig. 3), there being an operculum in *Amphibola*. On the other hand, in the *Dibranchiate Cephalopoda* (*Sepia*, Fig. 2), all the *Heteropoda* (Fig. 5) and the *unisexual operculate Pulmonifera* (Fig. 4) the ear-sacs contain single *otoliths*.

It will be thus seen that the nature of the auditory concretions is by no means an unimportant element in the classification of animals.[2] Prof. Huxley alludes to the genus *Polyophthalmus*, an Annelidan with eyes in every segment, as a remarkable fact, but this is excelled by his notice of otoliths in the tail of *Mysis flexuosa*,[3] a little pelagic crustacean which I have often had the opportunity of examining.

Every physiologist is aware that there are structural particulars in the ears of *Vertebrata* which show clearly that nature's philosophy is of a more profound character than that to which man has hitherto attained. Indeed if we study the simplest ears in creation, those, for example, of the common *Snail* and of the *Periwinkle*, a most interesting problem is presented to us to solve, namely, the precise function of the numerous otoconia in one case, and of the single otoliths in the other. It is commonly granted that these concretions augment the sonorous undulations by resonance, a view which is borne out by several considerations. If we take two stones and strike them together under water, the head also being immersed, the collision will produce a very loud and peculiar sound, but in order to make the minute otoconia impress one another

[1] By John Denis Macdonald, M.D., F.R.S., Dep. Ins. Gen. R.N., &c.
[2] See a paper by the author in the *Linnean Transactions* for 1860, in which a classification of the Gasteropoda has been attempted.
[3] See *Ann. and Mag. of Nat. Hist.* for May, 1851.

with the rhythmical flow of the undulations of sound, they must be poised off from the walls of the auditory sac. And this is effected, in some instances, by little tubercles on the inner surface as in the *cuttle fish* or as in the *Heteropod*, as shown in Fig. 5 (*h*), the auditory organ of *Cerophora*, in which also large vibratile cilia effect a continual rotatory motion of the spherical otolith. In other cases when otoconia are present, a fine ciliated lining not only prevents the contact of these minute particles with the walls of the sac, but keeps them in constant motion, jostling one another in a remarkable manner. Now when acoustic waves are passing over the auditory organ, it is easy to perceive how their impulses may be imparted to the otoconia, and thereby communicated with augmented effect to the auditory centre.

Whatever may be the intrinsic nature of nervous force it exhibits unmistakable polar properties which would

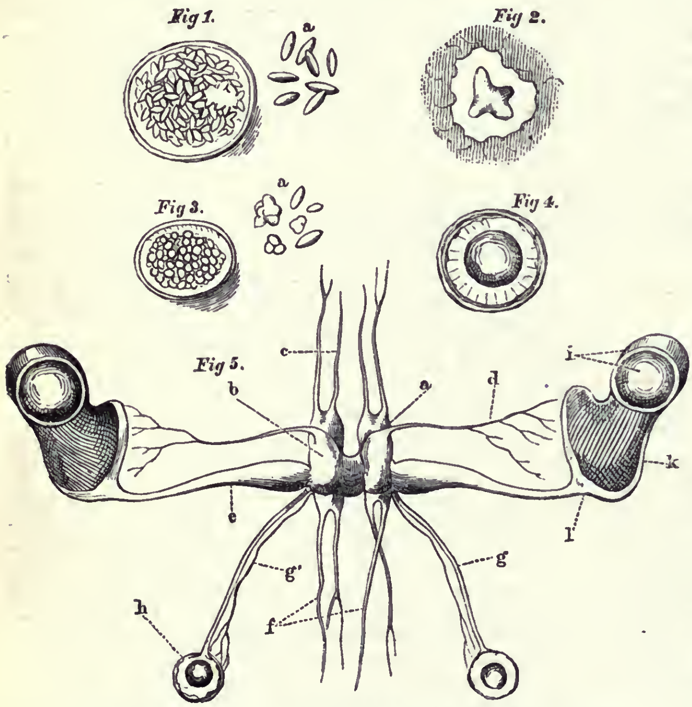


Fig. 1.—Auditory sac of *Nautilus*. *a*, otoconial particles, elliptical.
Fig. 2.—Section of auditory cavity of *Sepia*, with a somewhat cruciform otolith.
Fig. 3.—Auditory sac of *Limax atelis*, &c. *a*, otoconial particles, some of them compound, others of variable size.
Fig. 4.—Auditory sac of *Litorina*, *Geomelania*, or *Cyclostoma*.
Fig. 5.—Cerebroid ganglia and organs of vision and hearing in *Cerophora*. *a*, upper cerebroids; *b*, lower cerebroids; *c*, buccal nerves; *d*, motor nerve of the eye; *e*, optic nerves; *f*, trunks communicating with the pedal ganglia; *g*, pedicle of the auditory sac; *g'*, nerve distributed to the auditory sac; *h*, auditory sac with contained otolith; *i*, meniscus, and lens of the eye; *k*, body of the eye; *l*, retina.

thus place it in the category of the electric force, though no one now would attempt to reason out the identity of the two. It is, however, a new idea that nervous force in the *function of audition* plays an analogous part to the *electric force* in the case of the *microphone*. Moreover, if we look to the anatomy of the ear, we see that provision for a complete circuit is made. Take, for example, the auditory sac (*h*), pedicle (*g*), and nerve (*g'*) of *Cerophora* (Fig. 5). Nothing would appear to be wanting if we admit the nervous centre to be the equivalent of the battery, From all this it would seem to be a rational hypothesis that nervous force in traversing the circuit just indicated under the influence of sonorous undulations is actually transmitted from particle to particle of *otoconia*, or through the revolving *otolith*, as the case may be. But while the *microphone*, or the principle involved in it, affords us some additional light in relation to the physiology of hearing, the study of the anatomy of the ear might give electricians some valuable hints as to the construction of transmitting apparatus. One of Prof. Hughes' transmitters so exactly resembles the natural arrangement of the parts in the middle ear of the higher animals, that some few remarks on this subject may not be out of place here. Many years ago (1847-48) I noticed that a small piece of steel casually lying in the box of a square pianoforte reproduced, with great fidelity, any note, or number of notes, touched on the instrument, by the impact of its own weight meeting the vibrations of the sound-board beneath. Here was, in effect, the basis of the *telephone*, and I would indeed have anticipated the wisdom of the age had I known how to call in the aid of electricity in the simple way that this has been done by Prof. Bell. The fact, however, was made the subject of a paper published in the *Medical Gazette*, in which the *Membrana tympani* and the *Malleus* were compared with the sound-board and steel rod in the case referred to. Moreover, the inference was drawn that while the membrane communicates its vibrations to the ossicles, or small bones, it also causes the *malleus* to percuss the face of the *incus* responsively to the rapid and varied impressions made upon it, a view which borrows additional weight from the fact that in the frog articulation is still persistent where, from the absence of muscles for adjustment it might be considered to be quite unnecessary.

It should also be remembered the handle of the *malleus* extends like a radius from the centre to the circumference of the drum membrane, so as thus to include, without impeding, its three vibrating segments. The centre gives the *key-note* the circumference the *fifth*, and the intervening region the *third*. Only add to this the aural lens or lenticles, the *otoliths*, or the *otoconia*, with the light which the *microphone* has cast upon their function, and we are enabled to form a better conception of the physiology of hearing than has hitherto been possible. The accentuations, piano, forte, &c., in musical pieces, are marked with extreme accuracy, and should the parts of the music be deranged by a defect in time, an uneasy jog will be produced in the auditory apparatus, hence the antipathy of the mind to any erratic deviation in this respect. It is very remarkable that the *malleus* and *incus* (the hammer and the anvil) should correspond not only in figure, but also in function, to the objects from which their respective names are derived, for as we have already seen, the uses of the hammer and anvil, as employed in mechanics, are literally fulfilled by the *malleus* and *incus* answering very important ends in the faculty of audition. By the action of one upon the other, sounds are not only correctly transmitted to the auditory centre, but an accurate register of time, grace, and style, is effected in the manner above explained. Thus the physical organisation itself may be shown to be the natural preceptor of the mind.[1]

[1] Dr. F. de Chaumont writes as follows:—

Since Dr. Macdonald's paper was forwarded to you the appearance of Mr. Blyth's has added still stronger confirmation to the analogy between the microphone and the auditory apparatus of the mollusca, &c. One point in particular is the necessity of moisture of some kind or other, as a medium between the conducting particles, shown by the fact that even the watery vapour from the breath of the experimenter produced a sensible increase in the strength of the sounds elicited; and further, that the addition of simple water to the cinders was productive of still more striking effects. All this bears out the view taken by Dr. Macdonald that we have in the *otoconia*, *endolymph* and *vestibule* of the ear the most complete type of a microphone.

It would be well to try the free suspension of good conducting particles in a non-conducting fluid medium, sufficiently limpid to offer the least possible impediment to their movement.

The oscillation of the transmitter as a whole by mechanical means ought also to be tried in order to imitate the effect of vibratile cilia in the case of the auditory vestibule of animals.

F. de Chaumont

Army Medical School, Netley, June 20

WORK AND PROGRESS OF THE IMPERIAL GEOLOGICAL INSTITUTE OF VIENNA[1]

I. *THE Staff* has its full complement. M. D. Stur has been appointed sub-director, Dr. O. Lenz has returned from Africa with much information on the West Coast. M. Pilide, volunteer since 1875, has been appointed official geologist in Roumania. Two volunteers have joined, and there are four students in the museum and laboratory.

II. *The Building* has been considerably altered and enlarged, giving more space for laboratory, library, and museum.

III. *The Survey Operations* have been directed to (1) *the Special Map of the Empire.* Section 1. MM. Stache and Teller surveying the Central Alps south and east from the Cividale massif, the Oetzthal massif, &c. 2. MM. von Mojsisovics, Vacek, and Bittner—the Cima d'Asta, Sette Communi, and eastward to the Venetian plain, the Tertiaries of the Vicentin, and down to the valley of the Adige. 3. MM. Paul, Tietze, and Lenz, East Galicia and part of N.E. Hungary.

(2). *Local Surveys, &c.*, D. Stur—Review of Sternberg's and Corda's collections of Carboniferous Plants in the Prague Museum; Coal-bed of Upper Silesia; Fossil Plants of Lunz in Upper Austria. Stache—Palæozoic Schists of the Semmering on the Styrio-Austrian frontier. Von Mojsisovics—Trias in Upper Austria and Carinthia. Wolf—Railway Line in Upper Austria. Paul and Fr. Ritter von Hauer—Coal-beds of Aspang and Kladno. Bittner—Geological Map of the Archduke Leopold's Estates South of Vienna.

(3). *With Government Aid*, R. Hoernes—Devonian Strata near Gratz, Styria. Koch—Rhæticon and Selvettra group.

(4). *The Bohemian Commission.*—Krejci and Helmhacker—The Silurians of Central Bohemia. Laube—The Erzgebirg between Bohemia and Saxony. Fritsch—Palæozoic Saurians and Crustacea of Beraun. Nowak—Cypris-shales with Insects. Boritzky—Porphyries.

5. *Hungarian Geological Survey.*—Banat and South and West Hungary, surveyed by MM. Hofmann, Roth, Matiasovics, Boeckh, and Hantken.

IV. Rearrangement of and additions to, the *Museum.* Forty-one donors. Above 1,000 specimens, presented by Fr. Karrer, illustrative of the geology and fossils of the region traversed by the Francis-Joseph Aqueduct from the slopes of the Schneeberg to Vienna.

V. *Library.*—1. *Books:* Increase of 270 works in 281 volumes or parts; Periodicals, 422 volumes. Total at the close of 1877, 8,346 work in 22,496 volumes or parts; 766 Periodicals and Transactions in 13,261 volumes or parts. Various new Exchanges. 2. *Maps:* Arrangement completed. Total at the close of 1877, 933 in 3,825 sheets, besides the original maps by the Institute, and the special general maps of the Austro-Hungarian Empire reduced from them.

VI. *Laboratory.*—Newly established in a fresh locality. Enlargement of collection of artificial crystals, by Karl Ritter von Hauer. Analysis of eruptive rocks of the Ortler mountain-group, by M. John. Analyses of fossil fuel, ores, building-materials, &c.

VII. *Publications.*—1. The Transactions, vol. vii. part IV., and vols. viii. and ix., with fifty-four maps, sections, and plates, comprising Vacek's paper on the Mastodons of Austria; F. Karrer's Geology of the Francis-Joseph Aqueduct; and Stur's description of the Culm-flora. 2. The Annals: Ten contributors. 3. The Mineralogical Communications; Twenty-two contributors. These papers will for the future be published by themselves. 4. The Proceedings: Twenty-six contributors. 5. Other publications: MM. von Hauer and Neumayr's Guide for the Meeting of the German Geologists; M. Stache's Geological Map of the Maritime region of Austria; Fr. von Hauer's "Geology," second edition.

[1] From Fr. Ritter von Hauer's Annual Report, January 8, 1878.

METEOROLOGICAL NOTES

DR. OTTO KRÜMMEL publishes a paper in the current number of the journal of the *Gesellschaft für Erdkunde* of Berlin, on the distribution of the rainfall of Europe, illustrated by a well-executed map of seven colours, which show the regions where the annual rainfall does not exceed 9·8 inches (25 ctm.), is from 9·8 to 15·7 inches, from 15 7 to 21·7 inches, &c., the deepest tint covering all those regions where the rainfall exceeds 39·4 inches (100 ctm.). The map exhibits in a striking manner the small rainfall in the east and the heavy rainfall in the west; the markedly reduced rainfall of such mountain-sheltered plains as those which surround Paris, Clermont, Mannheim, Prague, Pressburg, and the great plain of Hungary; the large rainfall of the slopes of the Caucasus, which stands out in strong contrast with that of the arid regions all round; and the exceptional rainfall of Spain, which presents on the map a picturesque patchwork of all the seven colours representative of the wettest down to the driest regions portrayed on the map. The most important feature, however, is the partition of Europe into two divisions, by a waved line lying about the forty-third degree of latitude, the southern division being characterised by a rainless or all but rainless summer, and the northern by rain all the year round, where an absolutely rainless month is of rare occurrence. Slight exception may be taken to the rainfall set down for Iceland, Holland, and portions of the east of Scotland and west of Norway, as being a little too large, but on the whole the map is an admirable piece of work.

DR. HORNSTEIN, of Prague Observatory, has discussed the observations of the wind made there from 1849 with a Kreil's anemometer, and the results, which have been communicated to the Vienna Academy, disclose periodicities of velocity and direction generally accordant with Wolf's relative numbers of the sun-spots and with the well-known secular variation of the aurora. The mean annual velocity increases from the period of minimum to that of maximum sun-spots, and thence decreases with the diminution of the sun-spots to the minimum; and from the period of maximum to that of minimum sun-spots, the mean annual direction of the wind changes from a westward to a more southerly direction, while the change is in the opposite direction from the minimum to the maximum sun-spot period.

MR. BLANFORD, the Government Meteorologist for India, published quite recently a forecast of the weather of the monsoon season now set in. Reasoning from the unusually persistent high pressure then prevailing over Northern India, the singular absence of abnormal variations of pressure over the same region, and the heavy rainfall during the cold weather, he thinks it probable that the monsoon current will be below its average strength, that the rainfall will be more equally distributed than last year, and that the monsoon will commence later than usual in Upper India.

ON the occasion of the commemoration of the 400th anniversary of the founding of Upsal University in September last, the Swedish Government published an Atlas of fifty-one maps which had been prepared by Prof. Hildebrandsson to show the direction of the upper currents of the atmosphere during 1875 and 1876. About the same time the Meteorological Society (London) published thirty weather maps for March, 1876, prepared by Mr. Clement Ley, in illustration also of the upper currents. As regards the broad results arrived at, both authors are substantially agreed, the results being that while the surface winds blow inwards upon cyclonic areas

of low pressure and outward from anticyclones, the upper currents blow away from cyclonic and inwards upon anticyclonic areas. The most striking part of Mr. Ley's paper is the diagram in which he has summarised with no small amount of skill the facts of his cirrus-cloud observations. The point in the diagram is this: if the upper currents there depicted are to be regarded as tolerably close approximations to the movements of the cirrus-clouds of a cyclone, it follows that the region of the cirrus occupies a much higher level over the front portion of cyclone than it does over its rear—notably than over the north-west quadrant—a point of prime importance in relation to the theory of storms.

THE energetic way in which the Missouri (U.S.) weather service is being conducted may be judged of from the fact of the Report of the weather of May having reached us by post on June 24. This Report gives a statement of the rainfall for the month at from sixty to seventy stations, a map showing the distribution of the rainfall over the State for May, and a rapid sketch of the chief features of the weather. The rainfall was greatest in the central-southern districts, amounting to 8·00 inches at Bolivar, and least in the north-east, where at Canton it was only 1·77 inch. The increased efficiency of the system is well shown by the fulness with which the great storm of the 17-18th with its accompanying thunder and lightning and locally-developed whirlwinds has been accurately observed over Missouri, of which Director Nipher promises a full report. A separate sheet accompanies the Report, with all the instances of heavy rainfalls which have occurred during the past thirty years. Of these the most noteworthy as regards rate of fall was a downpour of 5·05 inches in an hour and-a-quarter on August 15, 1848. The heaviest continuous fall was 7·83 inches during thirty hours on June 18-20, 1859.

WE learn from the Mauritius Meteorological Report for 1876 that the rainfall of the whole island during that year was 12·63 inches less than the average, and that daily observations are now received from Seychelles, Rodrigues, and others of the neighbouring groups of islands. Valuable tables appear in the Report, showing the monthly means of pressure, temperature, and humidity from 1853 to 1874; but the noteworthy feature of the year's observations are the mean hourly values of the velocity and direction of the wind now published for the first time. These exhibit a well-marked daily period in the direction from E. 22° 15′ S., the most southerly point at 4 A.M., to E. 7° 0′ S. at 1 P.M., and thence back to E. 22° 15′ S. at 4 A.M., the daily variation thus being 15° 15′. Equally marked is the diurnal variation in the velocity, the minimum 9·7 miles per hour occurring from 2 to 3 A.M., and the maximum 18·5 miles per hour from 1 to 2 P.M. Hence, as regards this part of the south-east trades, the influence of the sun during the day is to double the velocity of the wind and to impress upon it a more truly easterly direction.

IN an eighth contribution to meteorology Prof. Loomis deals with the origin and development of storms, in which he shows that the great American storms are not confined in their origin to any particular locality, half of them originating on or close to the Rocky Mountains, and more than two-thirds north of 36° N. lat. The first stage in their development is the formation of an area several hundred miles in diameter, over which the barometer differs little from 30·000 inches, with areas of high barometer on the east and west sides, often another to the north, and occasionally a fourth to southward. The mean height of these different high areas examined was 30·310 inches on the west and 30·420 on the east side, that on the east side being thus the greater; and the distance of each from the central area of nearly uniform pressure which they surround is generally about 1,000 miles. A system of winds towards an intermediate or central point then sets in, resulting in a diminished pressure over the central area. The author supposes that the inflowing air escapes by an ascending current, carrying with it a large amount of vapour which as it is cooled is condensed into cloud and rain, and that the heat thus liberated further expands the air, thereby increasing the force of the inward movement of the wind. Rain is thus one of the conditions which increase the force of a storm. Prof. Loomis finds that an area of low barometer of considerable size may be formed and continue for several days with little or no rain, but in such cases the pressure did not fall so low as 29·250 inches. No storm of great violence has yet been found unaccompanied by a considerable fall of rain or snow. The general inward movement of the air towards a central area begins before any considerable precipitation of rain or snow has taken place. The easterly course of storms is considered to be occasioned by the general circulation of the atmosphere in that direction, and by the upward motion of the air taking place principally on the east side of the low centre as indicated by the position of the rain-areas. By this upward motion the air which presses in upon the east side of the low centre is prevented from restoring the equilibrium of pressure upon that side, and thus the low centre is steadily transferred toward the east, or the storm travels eastward. On the other hand, when the fall of rain or snow on the west side of the low centre is copious, widespread, and continued, the easterly progress of the storm is retarded, or arrested, or in some cases even retrogrades to westward, of which the storms of March 9-14, 1874, and January 1-18, 1875, were examples.

DR. WOJEIKOF sends to the Paris Exhibition new isobaric charts of the globe for January and July, which are rectifications of Buchan's isobaric charts, published in 1869, made by charting the large amount of fresh and fuller meteorological information collected since that time. Lake Baikal has recently been levelled, and its true height now ascertained to be 1,539 feet above the sea, instead of 1,342 feet, as given by Kropotkine. Correcting the barometrical observations for this height the mean pressure of this region in January is 30·630 inches, which is the maximum mean pressure for the globe at this season, and consequently 0·200 inch greater than was given in Buchan's chart. This extraordinarily high pressure in Eastern Siberia, which is 1·300 inch higher than that of Iceland at this season, is attributed by Dr. Wojeikof to the clear dry atmosphere of Siberia, and intense cold of the valleys and the high mountain barrier, which shuts off all communication, as regards the lower atmosphere, with the Pacific, where pressure in winter is low. A point of some interest brought out in the chart for July is the existence of two centres of low pressure controlling the wind systems of the Asiatic continent, the one being the Punjab and adjacent parts of Beloochistan, and the other the region around Lob-Nor. Dr. Wojeikof introduces an important feature into his charts in *not* tracing the isobaric lines over those portions of the globe which are at least 1,800 metres (5,906 feet) above the sea, some mountain-groups only being excepted. In this way the great plateau of Tibet, with its ramifications, is omitted, it being evident, for instance, that the winds of the Gangetic plains cannot be influenced by any differences that may obtain between the sea-level pressure there and that of the plains of Siberia, owing to the high, broad plateau of Tibet interposed between.

GEOGRAPHICAL NOTES

IN the course of the address which he recently delivered before the Geographical Society upon the subject of his travels on the western frontier of China, Capt. W. J. Gill, R.E., gave an interesting account of

his experiences on the borders of Thibet. He entered that land of mystery at Ta-chien-lu, whence the road at once ascends to the great plateau through a valley amongst granite rocks, capped at the summit with bare crags of limestone. Standing on the summit of the pass, by which the great upland country was reached, the traveller saw stretched below a fine valley closed in on both sides by gently sloping round-topped hills, covered with splendid grass. The road to Lithang was a succession of mountainous valleys, huge pine forests, and open glades. Capt. Gill found Lithang a cheerless place, some 12,500 feet above the sea-level. The natives told him that Ta-so, the last mountain-pass before reaching Bathang, was a very bad "medicine-mountain," the inconvenience caused by the rarefaction of the air at these great altitudes being attributed by them to subtle exhalations. On the road thither Capt. Gill passed the magnificent mountain Nen-Da, 22,000 feet high, and near the top of Ta-so he entered a little circular basin, surrounded on all sides but one by ragged precipices, with a pond of clear water at the bottom. On crossing the crest of the pass, he entered a large basin two miles in diameter, where a wild and savage scene presented itself to his sight: great masses of bare rock rising all round, torn into every conceivable shape by the rigour of the climate. The bottom of the basin was covered with the *débris* that had fallen from them, and some small pools of water in the hollows formed the sources of the stream, which eventually became a roaring torrent among the pine forests in the valleys below. Bathang, Capt. Gill found, had been recently rebuilt, after its destruction, a few years ago, in a frightful series of earthquakes, which, lasting for several weeks, devastated the whole neighbourhood. The town, he says, is chiefly remarkable for its immorality and its lamasery. Besides his description of the country Capt. Gill gave some interesting information respecting the habits of the Thibetans, contrasting them with those of the Chinese. Owing to their originally nomad mode of living they have no idea of inn accommodation, and the owner of a good house even will, as often as not, be found sleeping on the flat roof, whilst the hardy people in winter can sleep with their clothes half off and their bare shoulders in the snow; tables, chairs, and bedsteads are unknown in their houses. Thibet is a land flowing with milk and butter, the enormous quantity of the latter consumed by a Thibetan being very startling—butter in his oatmeal porridge, and huge lumps of butter in his tea. As a rule he does not drink much milk, which is mostly made into butter, but he is fond of sour cream, curds, and cheese; and this brings a Thibetan bill of fare to an end.

NEWS from Samarcand recently received gives some interesting descriptions of the district of Karatejin, which formerly belonged to Khokand but was afterwards ceded to Bokhara by the Russian Government. Karatejin, with the smaller districts of Dorwas, Waghia, and Shugnan, as well as the largest portion of Kojistan, are situated in the immediate neighbourhood of the plateau of Pamir. Karatejin in winter is completely isolated, and only during the summer months is accessible from the neighbouring districts. The manners and customs of its inhabitants are yet in the most primitive state. They have no idea of measures or weights, have neither markets, booths, caravans, nor indeed any institutions of public life. Theft is a thing unknown amongst them. Their occupation consists mainly in tending cattle, besides a little agriculture; everything is general property, as it were. If any family is short of provisions it is a matter of course that the next neighbour gives them what they may want.

THE *Pandora*, which is to be sent out by the *New York Herald* to the North Pole, has been re-christened at Havre the *Jeannette*. She leaves this week for San Francisco to complete her outfit, and starts next June for Behring's Straits. News has been received from Washington that there is no probability that funds will be appropriated this year for the intended Polar Colony of Capt. Howgate. No tidings have arrived yet from Capt. Tyson's preliminary expedition.

A CORRESPONDENT of the Hong Kong *Daily Press*, writing from Labuan, gives some interesting particulars respecting a scheme which, if carried out, may contribute much to the development of the resources of Borneo. An American company was formed for this purpose some time back, and obtained large concessions from the Sultan; but the policy of the United States government being to discourage in every way the extension of American commerce abroad, and the expenditure of any capital in foreign countries by its citizens, it has been deemed expedient to transfer the rights thus acquired to British merchants, and to leave to them the task of developing the enormous riches which now lie dormant in this beautiful island. For this purpose the steamer *America*, with representatives of both parties, went to Brunei, and the circumstances of the case having been explained to the Sultan, he not only consented to the transfer, but added to the former grants that of Gaya Island and the mainland opposite, including the magnificent harbour known as Gaya Bay, an enormous sheet of water said to be capable of sheltering the united fleets of the world. By this addition to the former grant the territory conceded now extends in an unbroken line from Kinarn's Bay, on the west coast, across the island to Sibuco, on the southern edge of St. Lucia Bay, on the east coast. This matter having been arranged, the *America* proceeded northwards, and, entering Maludu Bay, passed through the Malwalla channel to Sandakan. The approaches to this channel are very imperfectly surveyed, and abound with coral reefs and shoals not marked on the charts. From Sandakan the steamer went on to Sulu, and anchored in Membong Bay, about fifty miles south of the petty fort of Bhanuar, which has been held by Spain for the last two years. The Sultan of Sulu, when visited, expressed his hearty concurrence in any scheme which would tend to open up and civilise the rich and splendid provinces on the mainland now lying waste, and he at once confirmed the grants made by the Sultan of Borneo. Returning to Sandakan, the party proceeded up the Kina Batangan River in a steam launch, penetrating nearly two hundred miles into the interior, where no European vessel had ever been before, and then, having taken formal possession of their property at Sandakan, proceeded on their voyage to Labuan.

THE Rev. W. G. Lawes, the well-known New Guinea traveller and missionary, has communicated to the *Colonies* an interesting account of a visit which he paid, towards the close of last year, to the previously unknown village of Kalo, on the western bank of the Uanekela (or Kemp-Welch) River, which empties into Hood Bay, New Guinea, not far from Kerefunu. Mr. Lawes says that the village is laid out in streets and squares, all of which are kept scrupulously clean, being swept every day by the women. He induced one of the chiefs to accompany him some three miles up the river, which he found takes a sharp curve a little way above Kalo, and becomes narrower, but after about a mile it widens out again into a fine broad stream. It is said to be navigable for a long distance, and, according to native accounts, runs to Manumanu, in Redscar Bay. On the Kalo side of the river groves of cocoa-nut trees abound, and betel-palms are also plentiful, while on the east bank numerous and extensive plantations of bananas and sugar-cane were seen. Mr. Lawes states that the villages round and near Hood Bay are inhabited by a fine race of men, who are industrious and kindly-disposed, though at first shy and suspicious. They have a warlike character, but their hostility to each other would probably be soon removed

if more constant intercourse were established among them. Cocoa-nuts are at present the only article of any commercial value which the natives possess, and it is probable that some day large quantities of *copra* will be exported from this part of New Guinea; no doubt, too, the country has other resources which are as yet undeveloped.

NOTES

IN reference to our article (vol. xviii. p. 235) referring to the very unsatisfactory manner in which the publications of the Geological Survey are produced and distributed, we have received several communications professing to indicate the causes to which this unfortunate condition of affairs is to be attributed, and suggesting means by which it can be remedied. It would scarcely be within our province—even f it were in our power—to point out the particular departments or the individual officials with whom the responsibility for bringing about this almost perfect deadlock rests. We do, however, feel ourselves called upon to give expression to that dissatisfaction which is so widely felt in scientific circles, both in England and abroad, at the slowness with which the survey is carried on, the dilatoriness with which its results are published, the exorbitant prices charged for the maps and memoirs, and the parsimonious manner in which they are distributed. And in doing so we are acting no less in the interest of the overworked and often underpaid officers of the survey, whose efforts are frequently wasted, and whose patient labours fail to obtain proper recognition, through the neglect of the publishing department in making known the results of their work.

AS an instructive comment on the above, we may state that we have just received a magnificent series of maps illustrating the geology of Wisconsin and Colorado, along with a thick descriptive volume relating to the former state, full of beautiful chromo-lithographic illustrations of the peculiar geological phenomena to be found in the state. In execution and scientific accuracy these maps are equal to anything of the kind we have seen produced in Europe, and their liberal distribution by the Central and State Governments ought to make our own Government ashamed of its "penny-wise and pound-foolish" parsimony. The Colorado maps are issued, under the care of Dr. Hayden, by the Department of the Interior, while the Wisconsin volume and maps have the names of Messrs. Chamberlin, Irving, and Strong attached to them.

DR. JANSSEN has succeeded M. Puiseux in the astronomical section of the French Bureau des Longitudes, thus leaving vacant the post of geographer to the Bureau.

AT its session of July 1, the French Academy of Sciences elected Prof. C. Friedel to the vacancy in the chemical section resulting from the death of V. Regnault in January last. His chief competitors were MM. Cloez and Schutzenberger. Prof. Friedel occupies the chair of mineralogy at the École des Mines. His time is devoted, however, chiefly to chemical research, and he is at the present day the most prominent representative of the modern school of French chemists, who have grown up under the eye of Prof. Wurtz. His activity as an investigator began in 1856, and since that time he has chronicled a large number of valuable results won in various departments, but more especially in organic chemistry. His name is associated chiefly with extensive and elaborate researches on acetones, and on silico-organic compounds, and with the remarkable series of syntheses in the aromatic series by means of aluminium chloride, which for some time past he has been carrying out in company with Prof. Crafts, of Boston. Although his hair is streaked with grey, Prof. Friedel possesses a vivacity, energy, and devotion to his science, unexcelled by any of the younger chemists of the day, and promising a long-continued activity in the future.

AMONG recent deaths abroad we notice those of Prof. J. L. Chateau, of Ivry-sur-Seine, Prof. Labat of Bordeaux, and Prof. Ehrmann, formerly Dean of the Medical Faculty of Strasburg, who was aged eighty-six at the time of his death.

PROF. VIRCHOW is following up the cranial investigations which led him to assign a Turkish rather than a Slavic origin to the Bulgarian race. For this purpose he has recently received fifteen Bulgarian skulls from the battle-field of Kadikiöi, which have been carefully prepared by the red-cross surgeons.

PROF. VIRCHOW has decided to resign his seat in the German Parliament. He takes this step solely because his parliamentary duties interfere with his scientific labours; and, while he may be a good enough politician, he thinks himself a better *savant*.

WE briefly alluded recently to the annual session of the Vienna Academy of Sciences. At this session Baron von Rokitansky was re-elected president for the coming year, and the Crown Prince Rudolph of Austria was named honorary member. The class for mathematics and natural sciences has lost by death during the past year among its regular members K. v. Littrow, and among the corresponding members, the astronomer, Santini, of Padua, and the physicists Weber and von Mayer. These vacancies were filled by the election of Prof. E. Weiss of Vienna to Littrow's chair, and by the election to corresponding members of the zoologist, Prof. v. Brauer, of Vienna, the physicists, Prof. G. T. Fechner, of Leipzig, Sir William Thomson, of Glasgow, and Prof. J. Schwann, of Lüttich. The triennial prize for the most fruitful contribution to physics was assigned to Capt. A. von Obermayer, for his researches on the influence of temperature on the friction coefficients of gases. Prizes for the discovery of comets have likewise been assigned to MM. Winnecke, of Strassburg, Coggia, of Marseilles, Tempel of Florence, and Swift, of Rochester, New York. The Academy has appointed during the past year a standing committee for ethnographical researches in Austria. Prof. Doelter, of Graz, who was recently sent by the Academy to make a study of the extinct volcano, Monte Ferru, on the island of Sardinia, has recently submitted to the academy a detailed report of his investigations. The analyses of the lavas would tend to place them among the more modern eruptive formations. Monte Ferru exhibits a variety in the character of its lava deposits rarely found among volcanoes thus far examined. The chief species described are normal phonolite, trachytic phonolite, sanidine-plagioclase trachyte, sanidine-augite trachyte, felspar with and without olivine, leucite-basalt, trachyte,-tufa, rhyolite and hornblende-andesite.

ARRANGEMENTS are being made in Paris for an interesting sequel in 1879 to the present exhibition, which shall be entitled "Exposition des Sciences appliquées à l'Industrie." It will occupy the old Palais de l'Industrie. Assurances of co-operation on the part of leading scientific and industrial personages have been so numerous, that the success of the undertaking is already well guaranteed. The programme defining the aims and limits of the exhibition will appear at an early date.

THE *Medical Times and Gazette* regrets to learn that Dr. Burdon Sanderson has resigned his post as Professor at the Brown Institution. The work he has done in this position has been of a kind that is above praise. It has been mainly directed to the investigation of the phenomena of contagion, and, coupled with that of Dr. Klein, also connected with the Brown Institution, has done much to instruct us in the structure, functions, and characteristics of the lymphatic system—to mention only one series of researches. We sincerely hope that the post will be filled by one who will continue and expand the work already commenced in this invaluable Institution.

THE new historical museum of Frankfort-on-the-Main was opened on June 14. The new museum now contains all the objects of antiquity and art which were hitherto distributed in various public buildings of the old city. Several local scientific societies have presented the whole of their collections to the new establishment.

THE foundation stone of the Chadwick Natural History Museum was laid last week. The building is to be in the public park, Bolton. At present the town, although the third in importance in Lancashire, does not possess a museum.

A LETTER from the Préfet de la Seine has been sent to M. Mascart, the Director of the French Central Meteorological Bureau requesting that arrangements be made to post the weather warnings at the Bourse, Pont St. Eustache, and Halles Centrales, three places where monumental barometers have been placed at the expense of the municipal exchequer. It is curious that Paris does not yet receive a single warning although warnings are sent daily to 1,500 parishes, most of them of the smallest description.

THE first number of *L'Electricité*, a French semi-monthly periodical, has been issued.

A GENERAL meeting of the Mineralogical Society of Great Britain and Ireland was held at 116, Victoria Street, S.W., on Thursday, July 5, R. H. Scott, F.R.S., in the chair. Prof. Harkness read a paper on "Cotterite," a new variety of quartz from Ireland. A paper by Prof. Heddle, of St. Andrews, on "A New Manganesian Garnet from several Localities in Scotland," was read by the Secretary. The Secretary also read papers on "Youngite," and on "The Artificial Production of Psilomelane," communicated by Mr. J. B. Hannay, of Owens College, Manchester; and on "Penwithite, a New Cornish Hydrous Manganesic Silicate from Cornwall," analysed and described by himself. The annual meeting was fixed for Wednesday, August 14, at 2.30 P.M., to be held at Dublin.

BESIDES those whose names we gave last week, Mr. A. Cowper Ranyard, Mr. F. C. Penrose, and Mr. Giles Loder have also sailed for America, to observe the eclipse of the sun on the 29th inst.

WE have received the July number of the *Pantiles Papers*, a monthly literary magazine and review published at Tunbridge Wells. We are glad to see that the journal pays some attention to science.

TWO aeronautical ascents have been made from the Paris Cour des Tuileries in balloons of 450 cubic metres filled with hydrogen gas; the first took place on June 30 by MM. Gaston Tissandier and Jules Godard, and the second on July 7 by MM. de Fonvielle and Albert Tissandier. Both balloons were guided by a N.W. wind. On July 7 the balloon travelled at a regular rate of nine metres per second. Some interesting observations were made. It was noticed that cumuli have a height sometimes twice as great as their horizontal dimensions. The summit was observed to reach an altitude of 3,500 metres when the base was floating at an altitude of 800 metres. These clouds play the part of humid conductors connecting inferior with superior strata, and their dissolution in the form of rain is connected with electric phenomena.

MR. BRYCE M. WRIGHT has reprinted in a separate form from the *Journal de Conchyliologie* his "Description of the New Genus *Delphinulopsis*, and of the New Species *Delphinulopsis lesourdi*."

THE Paris Jardin d'Acclimatation has just made a most extensive and valuable acquisition of animals from Nubia. It includes fourteen giraffes, seven elephants, ten lions, two young hippopotami, seventy dog-faced baboons, and a number of antelopes, panthers, birds, &c. Herr Reiche, of Hanover, who captured these animals on the banks of the White Nile, receives for them the sum of 10,000*l*.

THE ancient records of the monastery of Fulda, and other German cloisters, which have been recently published among the *Monumenta Germaniæ*, give detailed accounts of a visitation of grasshoppers in the year 873, surpassing in point of destructiveness even those prevalent of late years in America. The grasshoppers appear to have come from the East, and, after having devastated nearly the whole of France, perished in the Atlantic. They are described as having hidden the sun, and having been able to eat everything green on a hundred acres in the course of an hour. Spanish monastic archives relate likewise the appearance of grasshoppers in 873, which appears to be the first record of an invasion by these insects in Europe.

WE are asked to state, on behalf of the Sunday Society, that, through the praiseworthy liberality of Sir Coutts Lindsay, the proprietor and director of the Grosvenor Gallery, New Bond Street, the Summer Exhibition at this institution will be open on the three following Sunday afternoons, between the hours of two and six:—On Sunday, July 14, the gallery will be opened free to the subscribers and members of the Sunday Society, and on July 21 and August 3 to the public, by tickets, which will be issued by the Sunday Society. These will be forwarded by post on receipt of stamped envelope. All applications to be by letter to the Honorary Secretary, 19, Charing Cross.

WE give the following extract from a letter sent us by Mr. R. Chartres on recurring decimals of the form—

$$\cdot\dot{a}\, b\, c \,..\, k\, a_1\, b_1\, c_1 \,..\, \dot{k}_1, \text{ where } a + a_1 = 9 = b + b_1 = \&\text{c.}$$

$$\tfrac{1}{19} = \cdot\dot{0}5263157894736842\dot{1}.$$

Here we observe a remarkable connection between the figures. Beginning with the last figure 1 we notice that each figure is double of the one to the right of it, one being carried when the double is over ten. Thus the eighteen recurring figures can be written down in a moment. Similarly the twenty-eight recurring figures of $\frac{1}{29}$ can be written down at once by multiplying by 3, thus:—

$$\tfrac{1}{29} = \cdot\dot{0}34482758620689655172413793\dot{1}.$$

Generally,
$$\frac{1}{nr - 1} \text{ (where } r = 10)$$
$$= -1 + \frac{nr}{nr - 1} = -1 + \frac{1}{1 - \frac{1}{nr}}$$

= Sum of the terms after the first of a geometric series *ad infinitum* whose first term is unity and common ratio $\frac{1}{nr}$.

Now, to divide a decimal by nr is simply to divide by n, and remove the figures one place to the right; and since the last recurring figure will be unity, we can get the whole period by beginning with 1, multiplying by n, and placing it to the left.

M. BOREL, the French Minister of War, has prepared a decree, which has been signed by the President of the Republic, for establishing in the army a high school of war. Besides tactics and special lectures on military topics, pupils will be taught geodesy, topography, geography, and telegraphy. They will be taken from among officers who have been commissioned for some length of time.

ARTICLE No. V. of the third volume of "Hermathena" is an exceedingly able and interesting sketch of "Greek Geometry from Thales to Euclid," by Dr. G. J. Allman. At the outset it is stated that the present century is "characterised by the importance which is attached to historical researches, and by a widely-diffused taste for the philosophy of history." In mathe-

matics Dr. Allman points to such works as Bretschneider's "Die Geometrie und die Geometer von Euklides"; Hankel's "Zur Geschichte der Mathematik in Alterthum und Mittel-Alter" (we are glad to find that our author's opinion of this work harmonises with the judgment we ventured to pass upon it in these columns); to Hoefer's "Histoire des Mathématiques" (1874), and to some others with which we are not acquainted. Dr. Allman opens his remarks with stating that "in studying the development of Greek science, two periods must be carefully distinguished. The founders of Greek philosophy—Thales and Pythagoras—were also the founders of Greek science, and from the time of Thales to that of Euclid and the foundation of the museum of Alexandria, the development of science was, for the most part, the work of the Greek *philosophers*. With the foundation of the school of Alexandria, a second period commences; and henceforth, until the end of the scientific evolution of Greece, the cultivation of science was separated from that of philosophy, and pursued for its own sake." In the course of forty-seven pages the investigation of what discoveries and advances are due to each geometer is most carefully and discriminatingly done, and the reader is put in full possession of the several authorities, and is thus in a position to try the correctness of Dr. Allman's deductions. We shall look forward to the continuation of the present paper which the writer promises.

AT a recent meeeing of the Birmingham Microscopical and Natural History Society, Mr. A. W. Wills exhibited the curious rotifer *Melicerta pilula* (figured by Mr. Charles Cubitt in the *Monthly Microscopical Journal* of July, 1872), which coats its tube with a wall of pellets consisting of *its own excremental pilules.* Mr. Wills gave an interesting description of the rotifer, and of his experiments with it. One of the specimens he exhibited had commenced the wall of its tube with the natural pellets, and had finished it first with blue pellets, and lastly with scarlet, according as he had fed it with indigo and carmine. After the meeting Mr. Wills gave the remainder of his specimens to Mr. Bolton for distribution among his correspondents.

IT is stated on the authority of a native Japanese paper, that the Hakubutsu Kioku (Exposition Bureau) of the Home Department proposes to erect a permanent exhibition building at Uyano, on the site of the National Exhibition held last year. It will cover about 700 tsuba of ground, and the frontage is to be 360 feet by 75 feet. On its completion it is intended to close the exhibition at Yamashita.

DR. MANZONI, of Bologna, has recently established the identity of the marl deposits of Upper Austria with those of the Renodale near Bologna, and describes eight varieties of echinoderms common to the two formations. Of these one still exists, and another is likewise found in chalk deposits.

THE philosophical faculty of Göttingen has offered two prizes of 1,700 and 680 marks for the best works on the causes affecting the changes in chemical composition of plants of the same species, such as climate, soil, fertilisation, &c. They must include a critical review of all facts hitherto gathered on this subject, and suggestions as to the best methods for completing our knowledge in this department, accompanied by the results of independent research in the directions indicated. Competitors must forward their work before August 31, 1880, and the decision will be announced March 11, 1881. They can make use of Latin, German, French, or English.

W. LANGE has sought to answer the question whether the silicium present in the sap of plants is in the form of silico-organic compounds, or not, and finds (*Ber. d. deutsch. chem. Gesell.*, vol. ii.) that it exists exclusively as a hydrate of silicic acid in very dilute solution.

FURTHER RESEARCHES ON THE SCINTILLATION OF STARS

THE results at which M. Ch. Montigny had arrived with regard to the influence of the atmosphere upon the scintillation of stars (see NATURE, vol. xiv. p. 562) have since been thoroughly confirmed by his further researches on this subject. The series of observations now comprises no less than 447 evenings, and the predominant influence of rain upon the intensity of scintillation may now be recognised as proved beyond doubt. We may here remind our readers that the intensity of scintillation is measured by the number of changes of colour which the star shows in the scintillometer during one second, and that M. Montigny has first proved that approaching moist weather increases this intensity. The frequent occurrence of wet days in the year from August, 1876, to August, 1877, has increased the average intensity from 71 to 76; but the following very dry autumn of 1877 brought down the average to 68 for that season.

M. Montigny has also given continual attention to the relation between the scintillation and the nature of the spectrum of any particular star. He has, as before, classified the 41 stars observed according to the three types of Father Secchi (of which type I. comprises the stars with four lines in the spectrum, type II. those with a number of fine lines or indistinct bands, and type III. those with broad bands and black lines), and for each type the new average intensity of scintillation is now given, each star in these comparative researches being reduced to an altitude of 60°. It appears now that the average for the first type has remained exactly the same as found before, while those for the other two types have changed but very little, although the number of observations has now risen from 611 to 3,025. These slight changes arise, doubtless, from the circumstance that the recent observations extend to 108 stars instead of 41. All these observations confirm, in the most definite manner, that fact which has already resulted from the first observations, and which M. Montigny expresses as follows:—"The stars possessing spectra with dark bands and black lines scintillate less than those with fine and numerous spectral lines, and considerably less than those possessing spectra with but a few principal lines."

Reserving the special data regarding the scintillation-intensities and the details of the stellar spectra for a further communication, M. Montigny now publishes a series of results respecting the colours of stars, which are of extreme interest.

The colours which the stars show in the scintillometer change in frequency from one type to another, and even between stars of the same type. For the same star the colours in their particular shades, in their frequency, and in their brightness, are further affected by temperature, the degree of atmospherical moisture, and the altitude of the star above the horizon. On the same evening, and under the most favourable atmospherical conditions, the number of colours and their brightness decrease steadily as the star rises in the east, and at a certain altitude they are no longer seen. In the west the reverse takes place, *i.e.*, the number of colours and their brightness increase the lower the star sinks, down to a certain altitude above the horizon, which changes according to the clearness of the atmosphere. If the star rises or sets, the limit at which the colours cease to be distinct is all the lower, both in the east and in the west, the finer and warmer the weather happens to be at the time. If the star has passed beyond this limit in rising or has not reached it in setting, it shows only a circle of a constant colour in the scintillometer, *i.e.*, of the colour peculiar to the star, and thus this apparatus offers an excellent means for determining the colours of stars.

The colours observed in scintillation are: red, orange, yellow, green, bluish green, blue, and violet. The difference in these colours is characteristic for the different star types, if we neglect the influence of the star's altitude and the condition of the atmosphere. Thus the red, which is the most constant colour for the three types, generally approaches the shade between the lines B and C of the solar spectrum in stars of the two first types, while stars of the third type give either a very dark red, or a bright cherry red, or very deep pink. The blue in stars of the first type is bright, and resembles steel blue in shade, while the blue in stars of the third type often shows a very dark shade, so dark sometimes that it becomes difficult to recognise it. When the weather was rainy the blue seemed generally to predominate amongst the other colours in all stars. Pure green was not so frequent than the other colours. Violet was also very rare amongst all the stars, but particularly amongst

those of type III.; if this colour was visible in stars of the other two types, they were always near the horizon. Yellow was rarely absent; yellow of greater or lesser brightness predominated completely in a large number of stars when they were high above the horizon and had ceased to scintillate. Orange is very frequent amongst the colours of the stars of the third type, while they scintillate.

If we reflect upon this short sketch of the changes of colour of scintillating stars, we see at once how complicated this phenomenon is, and that, in order to obtain data of tolerable certainty, at least two series of observations must be made, one in dry and the other in rainy weather; and further, that the influence of the star's altitude must be determined, which can be done by dividing the observations into separate zones of five degrees each.

With regard to the colours of scintillating stars Arago has expressed the opinion that the colour observed at any special moment is the complementary colour to those rays of the light peculiar to the scintillating star which at that moment are absent in the eye or in the telescope. M. Montigny agrees with this view and confirms it by an observation which he made when, on some evening specially favourable for observation, he inserted a prism just when the circle produced in the scintillometer by the light of the star Capella was very sharp and showed bright colours; he then found the two arcs of the circle divided into different colours, and this could not have been the case if the colours seen without the prism had actually been present. M. Montigny, however, intends further to examine this question by means of the spectroscope.

Arago also raised the well-known question whether the scintillation of stars is the same for two observers stationed at different places. M. Montigny replies to this question in the negative, and this was Arago's opinion also; he found the colours of a star to be different for those rays which are differently refracted by the two halves of the object-glass. In the same sense M. Donders has noticed that the scintillation does not always show the same peculiarities for each eye of the same observer. Another circumstance may be cited here as another proof, viz., that a double star, both components of which are of the same colour, such as Castor and its companion, which are both white, does not always show the same colours in the scintillometer at the same angles of position. Although the angle separating these two stars amounts to but five seconds of arc, it yet suffices to produce different colours. This shows how greatly the appearance of stars in the scintillometer is affected by the smallest differences in the conditions under which they are observed, since two separate pencils of rays of the same colour travelling side by side and at the same moment may yet be changed to different colours on their passage through the atmosphere.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

The subject of the use and abuse of examinations is beginning to be agitated in Austria. Within a few weeks past, two students, one at Pest and the second at Graz, have committed suicide after failing to pass examinations for the doctor's degree. The latter of the two had completed a lengthy course of study, and was an assistant of recognised ability in the university.

During the past few years the educational institutions of Rome have been entirely reorganised. The university, as constituted at present, is without a theological faculty. It numbers sixty-four professors, twelve in the legal faculty, twenty-four in the medical, sixteen in the scientific, and twelve in the philological-philosophical. Nearly all the conveniences of a well-appointed university are now enjoyed by the students: a botanical garden, laboratories for physics, chemistry, and physiology; the new observatory on the Capitoline, with special institutes for geology, mineralogy, mathematical physics, pharmacy, comparative zoology and anatomy, pathological anatomy, and six clinics. During the past month the Minister of Education has issued a decree for the foundation of a school of archæology, which shall be amply equipped, and meet a want long felt in this centre of archæological investigation.

SOCIETIES AND ACADEMIES

London

Royal Society, May 23.—"Experimental Results relating to the Rhythmical and Excitatory Motions of the Ventricle of the Heart of the Frog, and of the Electrical Phenomena which accompany them," by J. Burdon Sanderson, M.D., F.R.S., and F. J. M. Page, B.Sc., F.C.S.

This paper, although of some length, is a condensed statement of experimental results, so that it scarcely admits of being abstracted. These relate, as stated in the first paragraph, to (1) the order and duration of the rhythmical and excitatory motions of the heart of the frog, (2) the normal electrical condition of the surface of the heart and the influence thereon of mechanical, chemical and thermal injuries, and (3) the characters of the normal and of the excitatory electrical variations, and the modifications of those characters which are induced by injuries of the surface, and under the temporary influence of radiant heat. As we have not space to reproduce the whole, we will confine ourselves chiefly to the very interesting experiments contained in the two last sections.

The authors begin this part of the subject with the statement that they have confirmed, by repeated experiment, the observations made by Engelmann in 1873, that all parts of the surface of the "resting" heart are equipotential, and that the electrical inequalities which are usually found whenever the surface of the organ is investigated, when in this condition, owe their existence to slight injuries; they then proceed to discuss the conditions which lead to the existence of electrical differences. These are (1) permanent injury of the surface, however superficial and slight in extent, and (2) the temporary influence of radiant heat. As regards permanent injury, their observations are substantially in accordance with the conclusions of Hermann as regards other excitable and contractile tissues, viz., that the death of a part renders it *negative* to all living parts of the same organ. Substituting for the word death (which, in order to express the whole truth, must be understood to include every degree of local lesion, however limited in extent or slight in degree) the expression "permanent injury," this proposition becomes adequate for its purpose.

The authors further find that the influence of radiant heat produces a modification of the electrical condition of a part, of which the sign is opposed to that of the electrical change produced by injury. They were led to this result by the consideration that if arrest of the chemical changes which constitute the life of a part renders it negative, it is probable that a momentary intensification of these changes will render it positive.

The fundamental experiment by which both facts are established is as follows:—Two points on the surface of the heart, which may be as near to each other as two millims., are connected with a galvanoscopic circuit, and found to be equipotential. A loop of platinum wire, heated by a current, is brought into the neighbourhood of one of them for one second. After an interval of about a second the warmed surface becomes positive: in a few moments this effect subsides. If, then, the hot wire is brought nearer so as to scorch the surface, however slightly, and then removed, the opposite effect—that of permanent injury—manifests itself. The same spot, which was before positive, now becomes negative in a very much greater degree; for whereas the temporary "positivity" scarcely exceeds 1-2,000th Daniell, the "difference" of potential produced by injury may amount to 3-100ths Daniell. On the physiological meaning of these effects the authors do not enter. An indication of their bearing is, however, given by the observation in the next section, which relates to the so-called variation of the heart. By variation is meant the electrical disturbance which accompanies, or rather precedes, each contraction of the ventricle. The fact that such a disturbance exists has been known for several years. It has also been recognised that it precedes the visible change of form by which the systole discloses itself. The authors now show that the disturbance consists of two phases having opposite signs—that in the first phase, which is of short duration, parts near the apex become positive to parts further from it; that in the second phase the opposite condition is observed; further, that the first phase is entirely over before the ventricle begins its contraction, whereas the second phase corresponds in duration with the period during which the ventricle is doing the greatest amount of mechanical work, and ceases at the moment of decline of the muscular contraction of the ventricle. These time relations of the two phases suggest the inference that in all probability the first phase corresponds with that of the negative variation of ordinary muscle, with which it agrees in sign, and that the second phase is more immediately associated with the muscular contraction. That this is so appears to be shown by the observation that if any two points of the rhythmically pulsating heart a and b, of which a is nearest to the apex, are investigated by

suitable means during the period of systole, it is found that the variation observed (in which, so long as the surface is in its normal state, *a* becomes negative to *b* in the second phase) is modified by momentarily warming (in the manner already described) as follows:—When *b* is warmed *a* still becomes negative in the second phase; but the extent of the deflection is four or five times as great as before. When *a* is warmed, instead of becoming negative in the second phase, it becomes positive; in other words, the second phase is reversed. Permanent injury (as *e.g.*, by the closer approach of the hot platinum wire) produces similar results, with this difference: that whereas the modifications produced by radiation are transitory, and in fact pass off in a few seconds, those caused by injury are of much longer duration. All these facts seem to show that there is an intimate relation between the second phase and the act of contraction. The nature of this relation is matter for further investigation.

Linnean Society, June 20.—Prof. Allman, F.R.S., president, in the chair.—Mr. W. Catell was elected a Fellow of the Society.—Dr. J. Gwyn Jeffreys exhibited and made remarks on specimens of a new species of *Virgularia*, dredged by himself and the Rev. A. M. Norman in the Osterfjord, Norway, and which Dr. Danielssen will further describe.—Some gourds, probably the fruits of *Lagenaria vulgaris*, from Pekin, were shown by Mr. J. R. Jackson, of Kew. These, which were quite ornamental in form, had had their figure given them while in the living state by their being inserted into moulds, thus growing to the pattern desired.—A notice of some shells dredged by Capt. St. John, R.N., in the Korean Strait, was read by Dr. J. Gwyn Jeffreys. Of fourteen species enumerated six are now, for the first time, found living in the North Pacific as well as the Atlantic. *Nucinella ovalis* and *Kellia pumila*, supposed extinct, are thus shown recently living in the Korean region. Six other species are already known as inhabiting both oceans. No less than nine of the fourteen species are Coralline Crag fossils. The author finds this collection supporting his formerly expressed view, that mollusca common to the North Atlantic and North Pacific oceans may have originated in high northern latitudes, and have found their way to Japan on the one side and Europe on the other by means of the bifurcation of the great Arctic current.—Chas. B. Clarke read a paper on two kinds of dimorphism in the Rubiaceæ. The group in question is well known to be largely dimorphic, the variations chiefly consisting in the length of the style and stamens. The author now records dimorphism as follows:—1. Where the point of insertion of the stamens is altered, being situate in one form high above the middle of the corolla tube, and in another form at the base of the corolla tube —that is, subepigynous instead of epicorolline. 2. Where there are two kinds of fruit, viz. (*a*) a large fruit corresponding to a sessile flower, &c., and (*b*) a small fruit corresponding to a peduncled flower.—The Secretary read, for Capt. W. P. Armit, some notes on the presence of *Tachyglossus* (= Echidna) and *Ornithorhynchus* in North and North-East Queensland. It is shown that the Echidna occurs at Bellenden Plains, 18° S. lat., which habitat appears to be the most northern limit yet recorded in the Australian continent. The Ornithorhynchus is also met with 150 miles west of Georgetown and on the Leichardt River, about 18° S. lat.—Some remarks on the Echidna skull accompanying the above paper were made by Dr. J. Murie. Its comparison showed that, in all particulars, it agreed with that of the common *E. hystix*, and that supposed to be specifically distinct, to wit, the *E. setosa*. The New Guinea Echidna (*Acanthoglossus Bruijnii*) presents marked characteristic differences from both.—Prof. Oliver communicated a paper by Mr. N. E. Brown on the stapeliæ of Thunberg's herbarium and descriptions of new genera. Of eleven species of stapelia of Thunberg's determination, five only properly belong to the genus as now understood, and six belong to five different genera, two of which (*Trichocaulon* and *Diplocyatha*) are now characterised for the first time. *Sarcocodon speciosum* from the Somali land, and *Huerniopsis decipiens* from South Africa, are curious plants, the genera and species receiving a formal description.—The abstract of a paper on the shell of the bryzoa, by Mr. Arthur W. Waters, was taken as read.—The main points of observations on the white whale (*Beluga leucas*) exhibited at the Westminster Aquarium, were given orally by Dr. J. Murie. These notes chiefly related to the times and manner of breathing, certain outward peculiarities, visual organs and movements of body and tail in progression round the tank, along with other physiological topics. Respiration in and out of the water is not identical as to times and manner. A fungus (*Saprolegnia ferax*, Smith), that lately known as the "salmon disease," grew in abundance on the body of the whale, and no doubt acted prejudicially to the animal's health.

Mathematical Society, June 13.—Prof. H. J. S. Smith, F.R.S., vice-president, in the chair.—Mr. T. R. Terry was admitted into the Society, and Mr. J. D. H. Dickson was proposed for election.—Dr. Hirst, F.R.S., communicated a paper by M. Halphen, on the characteristics of systems of conics.—Mr. J. J. Walker read a paper on a method in the analysis of plane curves. This last paper was the development of a method of treating the intersections of a transversal with a plane curve which occurred to the author some years since; it contained, *inter alia*, a discussion, by the use of the method, of the problem of the inflexion-tangential curve for the quartic, with the determination of the co-ordinates of the tangential point in terms of the co-ordinates of the corresponding point of inflexion.—Mr. Tucker (hon. sec.) communicated the following papers:—On the calculus of equivalent statements, II., Mr. Hugh McColl; On the flexure of spaces, Mr. C. J. Monro; On the decomposition of certain numbers into sums of two square integers by continued fractions, Mr. S. Roberts, F.R.S.; On a new method of finding differential insolvents of algebraical equations, Mr. R. Rawson. —Questions were asked by Prof. Cayley, F.R.S. ("Has a solution been given of the statement that in colouring a map of a country, divided into counties, only four distinct colours are required, so that no two adjacent counties should be painted in the same colour?"); by Mr. Merrifield, F.R.S., on the uniform distribution of points in space; by Mr. Tucker, in connection with the announcement made in NATURE (vol. xvii. p. 104) of a second exception to Fermat's statement that all numbers of the form $2^m + 1$ are primes. The two exceptions now known are for $m = 5, 12$, in which cases $5 . 2^7 + 1$, $7 . 2^{14} + 1$ are factors respectively. Mr. Tucker suggested that $9 . 2^{21} + 1$, $11 . 2^{28} + 1$, &c., might be factors when $m = 19, 26$, &c.

Physical Society, June 8.—Prof. W. G. Adams, president, in the chair.—The following candidate was elected a Member of the Society:—Mr. R. H. Solly.—The Secretary read a paper by Prof. Hughes on the physical action of the microphone.—Sir John Conroy, Bart., M.A., read a paper on the light reflected by potassium permanganate. After referring to the results obtained by Haidinger and Stokes, and more recently by Wiedemann, he proceeded to describe his own experiments, which have been made by means of a very complete Babinet's goniometer provided with a vertical as well as a horizontal stage, so that the reflecting surface could be placed directly over the axis of the instrument. Sunlight, unpolarised or polarised in any plane by a Nicol, was used, and the moving arm of the instrument carried a direct-vision spectroscope with a "bright-spot" micrometer and a reflecting prism for bringing a second spectrum into the field. The colour of a surface, obtained by rubbing the crushed permanganate into a surface of ground glass with an agate burnisher, was found to vary with the nature of the light and its angle of incidence, and it further varied as the surface was immersed in benzene, bisulphide, or tetrachloride of carbon. With light polarised perpendicular to the plane of incidence, the dark bands in the reflected spectrum are far more distinct than when unpolarised or polarised perpendicularly to that plane. In the first of these three cases four bands are observed at angles less than 40°, and the blue end of the spectrum is very weak; as the angle of incidence increases the intensity of the blue rays diminishes: the dark bands gradually shift towards the blue end of the spectrum, and at about 60° a new band appears near D. With a still greater angle more of the blue rays are reflected and the bands fade away, those in the more refrangible part disappearing first. This displacement amounts approximately to 0·006 tenth-metre.—Prof. S. P. Thompson exhibited and described a cheap and efficient form of optical bench. Two straight oak bars, about two metres in length, are clamped together, as in a lathe-bed, and a number of slides carrying various appliances slide easily without shake, and can be fixed in any position by wedges. The several frames carrying the diffraction grating or edges, the eye-piece (with an engraved glass micrometer), &c., are so made, in wood, as to be capable of adjustment in any plane; and the instrument can also be employed for making photometric or other measurements. The mean of two determinations for the wave-length of certain red light gave 0·000629 as compared with Fresnel's figure, 0·000640, while the total cost did not exceed 3*l*.—The Secretary then read a paper by Prof. Ayrton, of the Imperial College of Tokio, Japan, on the electrical properties of beeswax and lead chloride. The

index of refraction of the former substance increases in passing from the liquid to the solid state, and it therefore seems important, in connection with the electro-magnetic theory of light, to carefully measure the specific inductive capacity of a condenser made of wax, as it is cooled through its solidifying point. The rise in capacity, as the temperature falls from 80° C. to 60° C., is very striking, and the entire change was found to be in exact agreement with the changes known to occur in the index of refraction for light. An elaborate series of experiments was made, which sufficed to show that the results obtained were not due to any change in the distance apart of the plates (of copper) between which the wax was placed, caused by any shrinkage of this wax on solidifying. In consequence of a remark of M. Buff that lead chloride behaves as a metal, Prof. Ayrton has studied it as a dielectric, and he found a diminution of resistance by electrification; but as this result was not confirmed on subsequent experiment, the question was more fully investigated, when he found that, with an electromotive force under 1·75 volts, there is an increase in resistance, and above that amount there is a regular or irregular diminution. This limiting force is about that required to decompose water, and he concludes that the results obtained must be due to the damp contained in the lead chloride.

Entomological Society, June 5.—H. W. Bates, F.L.S., F.Z.S., president, in the chair.—Mr. J. A. Finzi exhibited a remarkable hermaphrodite specimen of *Anthocaris cardamines*.—Mr. Rutherford exhibited a series of large cocoons from Mount Camaroons, formed by the larvæ of a species of Bombyx allied to *Anapha panda*, Bdv. These cocoons varied in diameter from four to seven inches, and each one contained from 130 to 150 smaller cocoons, all of which were tenanted by a larva or chrysalis in various stages of development. It would appear that *Anapha panda*, like some other species of *Bombycidæ* is social, and that the larvæ unite to form an aggregate cocoon of sufficient strength to withstand the attack of enemies and probably extreme changes of temperature.—Mr. Rutherford also exhibited a specimen of a *Papilio* as a case of so-called "hermaphroditism" with asymmetrical markings on the wings which approached respectively *Papilio cynorta* and *Papilio Boisduvalianus*, thus creating an impression that those two forms were but the sexes of one species. These specimens were from the collection of Mr. F. J. Horniman.—Mr. Meldola exhibited photographs of two species of tropical orthoptera sent to Mr. Darwin by Dr. Zacharias as an illustration of protective resemblance in the very perfect leaf-like appearance of the fore-wings; and some small beetles of the genus *Spermophague* and their cocoons, which had been found in a packet of seeds of *Cassio neglecta*, sent from Brazil by Dr. Fritz Müller to Mr. Darwin. The full-grown larvæ had emerged from the seeds, leaving the latter in a damaged condition, and had spun the small cocoons from which the beetles had issued, the insects having reached this country alive. Mr. Meldola also exhibited the proboscis of a sphinx moth caught by the narrow tube-like nectary of a pale yellow *Hedychium*, which had likewise been received from Dr. Fritz Müller, who states that sphinges are frequently found caught in ths manner.—Sir Sydney Saunders communicated notes by Mi. M. Lichtenstein, on new ideas as to the life-cycle of aphidians, giving the results of considerable breeding experiments.—The Secretary read a paper from Dr. Fritz Müller entitled "Notes on Brazilian Entomology," in which the author gave the results of his observations on the odours emitted by butterflies and moths, as well as facts bearing on various other subjects more or less connected with the theory of evolution. In reference to this paper the wings of *Antirrhœa archœa* from Brazil, and of *Mycalesis drusia* from the Nicobars were exhibited, in illustration of the author's theory of "scent-fans."—The following papers were also communicated: "On some longicorn Coleoptera from the Hawaiian Islands," by Dr. Sharp. "On the Larvæ of the Tenthredinidæ, with Special Reference to Protective Resemblance," by Mr. Peter Cameron; and "On *Macropsebium coterelli*, and other New Species of Coleoptera from Lake Nyassa," by Mr. H. W. Bates. The author exhibited the remarkable longicorn beetle above designated, which possessed some prominent characters of the Prionidæ.

Royal Microscopical Society, June 5, 1878.—H. J. Slack, president, in the chair.—Major O'Hara and Dr. J. Edmunds were elected Fellows of the Society.—A paper by Prof. Keith, on the results of a computation relating to Tolles' ⅛-objective, was read by the Secretary.—Prof. Stokes read a paper on the question of a theoretical limit to the apertures of microscopical objectives, in which he showed that, theoretically, a pencil of rays from a radiant in glass (or under equivalent conditions) of 180° could be refracted by a single refraction at a spherical surface, so as to present to the second lens a pencil of about 81° free from spherical aberration; and, while not asserting the possibility of utilising the whole of the pencil of 180° in glass, he thought a very large part of it might be available in a practical construction—a far larger part than can be used with dry lenses. The subject was further discussed by Messrs. Ingpen, Stephenson, and Mayall.—The other papers were on the measurement of the diameter of the flagella of *Bacterium termo*, by the Rev. W. H. Dallinger; on the framework of the mastax of *Melicerta ringens*, by Mr. F. A. Bedwell; a translation by Mr. Kitton of a paper by M. Petit on some new genera and species of diatoms; and a note by Mr. J. H. Stephenson on the effect produced on *Pleurosigma angulatum* by stopping out the central dioptric pencil. Mr. Stephenson exhibited, after the meeting, *Pleurosigma angulatum*, with his new oil-immersion lens, under the conditions explained in his paper; and Mr. Mayall demonstrated the aperture of Tolles' ⅛-objective, by Abbe's apertometer, to be largely in excess of the maximum possible for dry lenses. Some extremely good slides of the mastax of *Melicerta* and *Conochilus*, mounted by Lord S. G. Osborne, which had been sent by Mr. Bedwell, were also exhibited.

Meteorological Society, June 19.—Mr. C. Greaves, F.G.S., president, in the chair.—J. C. Philips and W. S. Rawson were elected Fellows.—The following papers were then read:—The climate of Lundy Island, by A. J. H. Crespi, B.A., F.M.S. Lundy Island, from its geographical position, might be expected to have a mild, damp climate, with cool summers and warm winters, and a small diurnal range of temperature, and so, no doubt, it has, although certain local circumstances in addition to its peculiar configuration make the climate remarkably inclement, windy, and unpleasant. The island runs nearly due north and south, having an extreme length of four miles and a breadth of from 200 yards to 1,600 or 1,800; there is a nearly flat table-land or "top" running due north and south, having an altitude of 450 feet; shelter there is none, every current of wind sweeps the whole table-land. From the edge of this table-land the ground slopes away to the sea; sometimes the descent of the side-land is extremely abrupt, at other spots more gradual, while the side-lands are deeply cut by caves, precipices, small bays, and glens. All around the island the water is deep a few hundred yards off, while the currents are formidable, and tremendous seas break upon the rocks almost every day in the year. The *one* drawback of the place is the wind, so furious and continuous are the blasts first from one quarter, then from another, for days and even weeks. When gales occur, as they generally do at short intervals, the force of the wind becomes incredible: walls are torn down, gates and doors wrenched out of their fastenings, and the few buildings which can be blown down are more or less injured. Fogs are remarkable for their frequency and density, and are nearly always drenching. The rainfall is nearly 50 inches per annum. February and March are said to be the coldest months and August the hottest; the mean temperature of the year is about 50° or 51°.—On the auroral or magnetic cirrus, by the Rev. S. Barber, F.M.S.—Contributions to the meteorology of Natal, by Dr. R. J. Mann, F.R.A.S. This paper is a discussion of the observations taken at Maritzburg (2095 feet above sea-level) during the six years 1860–65; from it we learn that the summer of Natal is a season of copious rain and the winter a season of relative dryness; also that the former is a time of abundant and frequent cloud, and the latter a time of preponderant sunshine. The summer is consequently cooler in a material degree than it would otherwise be, on account of the frequent prevalence of cloud and the abundance of the rainfall; and the winter has its temperature materially raised from the constant occurrence of clear skies and bright sunshine. The mean annual rainfall was 31·13 inches, of which amount nearly 28 inches came down during the six summer months (October to March), and scarcely more than 2 inches during the four midwinter months (May to August). Thunderstorms are of frequent occurrence, the average exceeding seven per month from October to March. The thermometer rarely rises above 85° in the shade even in the summer months, unless a hot wind is blowing; it then mounts to somewhere between 85° and 97° according to the strength of the sirocco. The degree of humidity indicated

by the dry and wet bulb thermometers when a hot wind is blowing varies from 25° to 52° of moisture. The highest temperature recorded during the six years was 97°·6, the lowest 29°, and the mean 63°·3.—Note on the mean relative humidity at the Royal Observatory, Greenwich, by W. Ellis, F.R.A.S. In this paper the author gives the mean relative humidity in each month of the year at 9 A.M. and 9 P.M., and the mean of the twenty-four hourly values, derived from the photographic records of the dry and wet bulb thermometers for the twenty years 1849-1868. The 9 A.M. value is smaller than the mean in summer and larger in winter; and the 9 P.M. value is larger than the mean throughout the year, but most in summer. The mean monthly values change little from April to August, and from October to February; and there is a great decrease between February and April, and a corresponding great increase between August and October. The mean for the year is 80·7.—On a method of sometimes determining the amount of the diurnal variation of the barometer on any particular day, by the Hon. R. Abercromby, F.M.S.—On the relative duration of sunshine at the Royal Observatory, Greenwich, and at the Kew Observatory, during the year 1877, by G. M. Whipple, B.Sc., F.R.A.S. The author having instituted a comparison of the amount of sunshine recorded at these two observatories, finds that the totals show that for the whole year the excess in the number of hours the sun shone at Kew over the number at Greenwich, amounted to 171. This difference is no doubt due to the direction of the wind, for Greenwich lying to the south-east of the chief part of London, and having also large manufacturing establishments on its northern side, is greatly shaded by cloud, probably in a great measure due to smoke, when the wind blows from W., N.W., or N., while at Kew, which is situated to the west of London, and is remote from factories and shipping, enjoys a larger percentage of sunshine with these winds. With winds from the N.E., S., and S.W., Kew has but slight advantage over Greenwich. With E. and S.W. winds the London smoke is driven over Kew, and its presence in reducing the transparency of the air is evident in the diminished amount of sun recorded, the quantities being only 81 and 65 per cent. of those registered at Greenwich.—Account of the atmospheric disturbance which took place in lat. 21° N., and long. 25° W., on January 27-28, 1877, by J. H. Cardew.—Notes on some remarkable cloud formations accompanying sudden and frequent changes of temperature and wind, by Capt. W. Watson, F.M.S.

VIENNA

Imperial Academy of Sciences, February 28.—The following, among other papers, were read:—On some sensations in the region of visual nerves, by M. Brücke.—On the galvanic polarisation of platinum in water, by M. Exner.

PARIS

Academy of Sciences, July 1.—M. Fizeau in the chair.—The following among other papers were read:—On sulphuric saponification, by M. Fremy. He expresses his satisfaction because this process, *without distillation* (which discoloured the fatty acids), is now become an industrial operation, as the Exhibition proves.—On a system of telephone without electromagnetic organs, based on the principle of the microphone, by M. du Moncel. A reference to recent results by Blyth, Hughes, &c., in this direction. He thinks some of the sounds heard in telephones connected with telegraph lines may arise from friction of the lines on their supports.—On diphtheria in the East and especially in Persia, by M. Tholozan. There is no record or tradition of croup, scarlatina, or diphtheritic or gangrenous angina in Persia previous to 1869. In that year and the next a small epidemic of scarlatina appeared. In 1874 an epidemic of diphtheria broke out in the south of Persia and spread towards the north and west. Particulars are given.—M. Friedel was elected member in the section of chemistry in room of the late M. Regnault.—Thermal researches on the chromates, by M. Morges. The electrolytic decomposition of the chromates is not comparable to that of the alkaline sulphates. The heat confined does not exceed 12,500 calories. The chromates are rather comparable (thermally) to the carbonates.—*Trombe* of May 15, 1878, in the department of Vienne, by M. De Touchimbert. This appears to have been very violent, uprooting strong trees, damaging houses, lifting railway carriages, and throwing persons down. Its course (about forty kiloms.) was from S.S.W. to N.N.E. and E. Its width was 1,000 to 1,200 m.; velocity about 44 m. per second; pressure about 220 kilog. per square metre.—On the deformations of the disc of Mercury during its transit, by M. Lamez. Calculation shows that an advance of 8 sec. on the theoretical instant of contact may have been produced by the ellipticity; and an advance was observed.—On a single liquid pile depolarised by the action of atmospheric air, by M. Pulvermacher. The exciting liquid, (dilute sulphuric acid, caustic potash, or sal ammoniac) is placed in a porous cylindrical vessel. The positive metal is a rod of amalgamated zinc placed in this vessel, and the negative consists of long bell-springs of fine wire (silver or platinum) coiled round the cylinder, their coils sufficiently apart to avoid capillary action. The wire is thus in contact at a great many points with the liquid which transudes the porous vessel, and the external air has a continual oxidising action on these numerous small surfaces of tangence, thus effecting depolarisation.—On a new mode of formation of glycolate of ethyl, by MM. Norton and Tchermak. This is by action of glycolide on ethylic alcohol.—On the action of chlor-hydrates of amines on glycerine, by M. Persoz. On heating, *e.g.*, glycerine with chlorhydrate of aniline, one easily obtains phenylised derivatives of glyceramine along with secondary products.—On anærobiosis of micro-organisms, by M. Gunning. With the aid of ferrocyanide of ferrosum, which he found an extremely sensitive reagent for oxygen, he had shown that the apparatus and media commonly used for culture of micro-organisms cannot be freed from oxygen by the methods recommended for this purpose. In a memoir to the Amsterdam Academy, he gives arguments for attributing the cessation of putrefaction solely to the death of the bacteria, caused by absence of free oxygen. M. Pasteur considers that putrefaction is sometimes stopped because the small organisms have passed into the state of germs.—On the "piedra," a new species of parasitic affection of hair, by M. Desenne. This consists of hard nodosities (visible to the naked eye) at regular intervals on the hair. It is met with in the province of La Cauca, Columbia. It is cured by greasing the head well, and is not contagious.—On the explanation of the effects of irrigations practised in the south of France, by M. Barral. These are important, not only on account of the matters brought by the water, and the satisfaction of the need of moisture, but on account of the reactions they favour in the layer of earth necessarily moistened, aërated, and put in contact with mineral or organic compounds.—Letter from Prof. Du Bois Reymond presenting two new volumes of his researches on the physics of the muscles and the nerves. They contain memoirs published since 1855.—A memoir by Prof. Villari was presented, on emissive power and the different kinds of heat which some bodies emit at 100°.

CONTENTS

THURSDAY, JULY 18, 1878

THEODORE SCHWANN

On Sunday, June 23 last, a very interesting ceremony took place at the University of Liége, in Belgium, in honour of Schwann, the famous author (with his fellow-worker Schleiden) of the so-called "cell theory." So rapid has been of late years the progress in our knowledge of the minute structure of animals and plants that Schwann's name seems already to belong to the distant past, and not a few biologists appear to have been, up to the last few months, under the impression that the distinguished author of the "Microscopical Investigations into the Identity in Structure and Growth of Animals and Plants" had long ago been laid in the grave. We rejoice to say that, on the contrary, he is alive and to outward appearance hale and vigorous, though he has had some warnings which have led his *confrères* at Liége to celebrate this year, as a sort of premature jubilee, the fortieth anniversary of his professoriate rather than wait till the full tale of fifty years had been told.

Theodore Schwann was born at Neuss, near Düsseldorf, on December 7, 1810. In 1829 he entered the University of Bonn, first as a student of philosophy, but afterwards as one of medicine. The illustrious Johannes Müller was at that time a privat-docent at Bonn, and Schwann, like so many other of the distinguished biologists of the present day, owes much of his success in life to the vivifying influence of that distinguished teacher. From Bonn Schwann migrated to Würzburg and thence to Berlin, whither Müller had been called to fill the chair left vacant by the death of Rudolphi. Here Schwann, working with the support and under the guidance of Müller, carried out several physiological investigations, the most notable of which were those on the respiration of the chick in the egg, on artificial digestion, on the structure of muscular fibre and of elastic tissue, on the contractility of the arteries, on the mechanics of muscular contractions, and on spontaneous generation. All these researches made their mark and added to our knowledge. Many readers will doubtless remember the ancient myograph employed by Schwann, which was exhibited at the collection at South Kensington two years ago. Besides this, Schwann appears to have largely assisted Müller in the experiments and observations necessary for the construction of the well-known text-book of physiology. Lastly, in 1838, he began to publish, in Froriep's "Notizen," the views which had arisen in his mind concerning the cellular structure of organic beings, and in 1839 laid them before the world in a complete form under the title of "Microscopische Untersuchungen über Die Uebereinstimmung in der Structur und dem Wachsthum der Thiere und Pflanzen." It is unnecessary here to point out the immense effect which the "cellular theory" has had on the progress of all branches of biology. It has made itself felt throughout the whole domain of physiology and pathology, and in a very remarkable manner prepared the way for the more recent doctrines of evolution. During the forty years which have elapsed since it was first enunciated, it has undergone considerable changes: it has been hammered by the blows of repeated labours into a form more durable than that in which it first appeared; but it still remains as the cellular theory. And this at least may be said, that Schwann himself, in expounding his views, kept remarkably clear of the many vagaries in which his immediate followers indulged, and which for a while threatened to make the cellular theory a reproach rather than an honour.

While engaged in these labours Schwann held at Berlin the post of assistant to the Museum, giving as well private courses on histology, and he was on the point of being made Professor Extraordinary when he received an invitation to the chair of General and Descriptive Anatomy at the University of Louvain. This he accepted, and entered upon his duties at the close of the year 1838. In 1848 he was made professor of the same subject in the University of Liége, and here he has remained ever since, exchanging in 1858 his chair for one of Physiology.

During his stay in Liége his life has not been an idle one, but the fame of his earlier labour somewhat throws his later work into the shade, and he is now enjoying the repose which is not only fitting to his age, but which has in every way been most thoroughly earned. The enthusiastic reception which he received at his jubilee from the students of the university showed very clearly how dearly he is loved and how highly he is honoured by those who have still the privilege of being taught by him.

The ceremony of the 23rd began at one P.M., when the Rector of the University delivered in the Aula an oration laudatory of Schwann, at the close of which he unveiled a very successful bust. A large audience was present, among them a considerable number of ladies and students. Prof. Edouard van Beneden then gave an admirable account of the scientific labours which were that day being honoured; after which a student, in a speech which was repeatedly applauded with much enthusiasm, described how greatly Schwann was admired and beloved by his pupils. Then followed the presentation of addresses from various universities and learned bodies. It would be impossible to enumerate these, for they came from all parts of the world, those from Berlin, Vienna, and Heidelberg being especially elaborate; a goodly number arrived from Great Britain. The time of year chosen for the ceremony being unfortunately in the middle of the academical summer session very few representatives were able to attend in person. There were present, however, as bearers of addresses which they delivered with suitable speeches, Prof. Waldeyer, from Strassburg, Prof. Gussenbauer, from Prague, Mr. F. M. Balfour from Cambridge, England (charged personally, in the unavoidable absence of Dr. M. Foster, with congratulations from the Royal Society, from Trinity College, Cambridge, from the Physiological Society, and from the professors and lecturers of the University of Cambridge), and Prof. Pilar, from Agram.

The ceremony was concluded by a genial speech from Schwann, in which he gracefully acknowledged the compliments which he had received. A banquet followed at which the toasts of "The King," "Schwann," "The Strangers," &c., were given, the latter being responded

to by the distinguished biologists present. In the evening a pleasant excursion was made into the surrounding country.

A splendid album had been manufactured at Vienna for presentation to Schwann, containing the photographs of almost all living biologists. Unfortunately it arrived too late to be formally presented at the ceremony. The expense of the bust was defrayed by subscriptions in Belgium, though a few strangers (among them Mr. Darwin) had an opportunity of contributing.

The whole ceremony was extremely interesting and successful, and we trust the hero of it may still live many years in which he may have the pleasure of looking back upon his jubilee, and of feeling that his labours have been appreciated by his age.

A TRANSLATION INTO GERMAN

Grundzüge der Anatomie der wirbellosen Thiere. Von Thomas H. Huxley, LL.D., F.R.S. Autorisirte deutsche Ausgabe, von Dr. J. W. Spengel. (Leipzig, 1878.)

SO far as we know, amongst the many German text-books on anatomy and physiology there is not a single one which is at all carried out on the plan of Huxley's Manual of the Anatomy of Invertebrated Animals. The great merits of the work appear to us to be, firstly, that it combines up to a certain point the features of a treatise on comparative anatomy and on zoology, and secondly, that by the introduction of a description of a type selected from each group, the learner is both greatly assisted in the practical study of animal morphology and also supplied with certain definite centres round which to group the multitudinous facts which he learns in the course of his reading. We flattered ourselves that by the translation of this work into German we should to some extent repay our Teutonic neighbours for the many text-books we have received from them. Our belief that this work was likely to be appreciated in Germany has, however, been very rudely dispelled. We learn from the distinguished naturalist who has undertaken the translation, and whose large experience (we believe his name has been before the public for so long a period as two or three years) gives corresponding weight to his opinion that the work is neither a handbook nor a text-book. He informs us in his preface that "he has decided not to give the work the title of handbook, in order to avoid labelling it with a title which it does not deserve" (um dem Buche nicht einen Anspruch unterzuschieben, den es nicht erheben will). "It is," he goes on to say, "no handbook in the sense customary with us, and indeed can be regarded as a text-book (Lehrbuch) only in the sense that it is intended for learners." In fact, on the unimpeachable authority of Dr. Spengel, Prof. Huxley's Manual of Invertebrata, which has already become the acknowledged handbook in England, is quite unworthy of such a position. In this country we have been accustomed in our simplemindedness to think that Prof. Huxley possesses a singular talent for exposition, while his reputation amongst us as an anatomist is based on our belief that his knowledge of anatomical facts is as wide and extensive and as well kept up as his critical judgment is acute, and his treatment of morphological problems broad and original. We have for some time past been under the idea that Prof. Huxley has had a good deal to do with the progress of animal morphology during the last twenty or thirty years. But we live to learn, and we feel very grateful that a man of Dr. Spengel's standing should show us how imperfect and unequal (lückenhaft und ungleichmässig) is Prof. Huxley's treatment of the subject to which he has devoted his life.

So impressed apparently was Dr. Spengel with the faults of the work which he had obtained permission to translate, that, as he explains in his preface, he asked Prof. Huxley to rewrite the work, in order that the German translation might appear more worthy of the translator's reputation. Singularly enough, Prof. Huxley, with an indifference to the appearance of any translation at all, which must have seemed strange to the translator, declined this modest request. And we gather that he invited Dr. Spengel to modify the earlier chapters (written long ago) in accordance with the views based on later researches, and expounded in the later chapters. The labour involved in such a change was apparently, not congenial to Dr. Spengel, whose energies seem more at home in writing prefatory remarks.

It is with the illustrations, however, even more than with the text of the original, that Dr. Spengel is offended. He expresses the view that the choice of these must have been made on grounds of economy. The larger number are, he says, "derived from the older works of Huxley, and the remainder from the well-known handbook of Owen (aus dem bekannten Owen'schen Handbuche), and other sources." We find some difficulty in understanding the translator's preface at this point. We presume that by "the handbook of Owen" he refers to Owen's "Lectures on the Comparative Anatomy of Invertebrates." We should very much like to know what illustrations are referred to, since, as far as the editions of Owen's lectures obtainable in this country are concerned, none of the figures of that work have been borrowed for Prof. Huxley's Invertebrata.

The translator informs us that he has thought fit to set aside many of Huxley's figures and to add new ones from well-known sources. He has, moreover, had a considerable number of the figures redone. In some of these cases we admit that some improvement has been effected by the alteration of the figures. The two figures copied from Ludwig to illustrate the anatomy of Comatula are excellent, and the substitution of Butschli's figures of Pilidium, for the somewhat erroneous ones of Leuckart and Pagenstecker, effects a decided improvement. In other instances the translator, in his zeal to make the figures clear, appears to have forgotten that it is also desirable to make them true to nature. Thus Fig. 77 does not appear to us to be so true a representation of the appendages of Astacus as the original figure of Huxley, which the translator has set aside; and in Fig. 80 the heart and vessels of Astacus are very far from being as true to nature as they should be. We think also the translator, in adding new figures, should be careful about the references. In the first two figures he has substituted for those of Huxley—Fig. 9 and 35-37—we find wrong references. On the whole the improvement is not so great as might have been expected. Every one is aware that for years past the illustrations of German scientific works have been far superior to those of English ones.

Such a superiority is less obvious than usual in Dr. Spengel's production. Either Dr. Spengel was generously unwilling that the difference should be too striking, or Prof. Huxley's malign influence has extended to the German engraver and printer.

Considering the view which the translator appears to take of Huxley's "Manual," we were rather surprised that he should jeopardise his great reputation by undertaking the translation of so inferior a work. Our astonishment may easily be imagined on finding on the back of the work that the *authorship is attributed to Spengel as well as to Huxley*. The outside of the book, as seen on the book-shelf, reads thus:—

HUXLEY-SPENGEL
ANATOMIE
DER
WIRBELLOSEN THIERE.

The only explanation which occurs to us of this unusual blending of the names of author and translator is that Dr. Spengel felt that the prominence of his name was necessary in order to ensure, for the production of so feeble an anatomist and so imperfect a writer as Prof. Huxley, a circulation large enough to bring about the pecuniary result for which the translation was made. Men have been known to make translations for the sake of a sort of parasitic, or rather "commensal" reputation; but in this case, since Dr. Spengel seems to be the superior of Prof. Huxley, some other object must have been foremost in view.

Seriously speaking, we hardly think Dr. Spengel can have fully realised the effect which such a preface would have upon the ordinary reader. Had he done so his behaviour towards Prof. Huxley would have been of a kind for which we should hesitate to use adjectives adequately descriptive. F. M. B.

MERRIMAN'S "METHOD OF LEAST SQUARES"

Elements of the Method of Least Squares. By Mansfield Merriman, Ph.D., Instructor in Civil Engineering in the Sheffield Scientific School of Yale College. (London: Macmillan.)

THE method of least squares has an extensive literature of its own. Our author, in a sketch appended to his work, gives the titles of forty-seven of the most important memoirs and books which treat of this subject and of the law of errors of observation. He further "takes the wind out of the sails" of his reviewers by saying: "It would be easy to greatly extend the limits of this list. The titles have, in fact, been selected from a list of about four hundred, which I hope some time to publish, accompanied by historical and critical notes." Though this is an unkind cut, inasmuch as a reviewer will hardly care to bring forward any references of his own, we yet trust Dr. Merriman will be sufficiently encouraged to bring out this promised contribution to the history of a particular branch of mathematics. The writer's objects are "to present the fundamental principles and processes of the method in so plain a manner and to illustrate their application by such simple and practical examples as to render it accessible to civil engineers who have not had the benefit of extended mathematical training; and secondly, to give an elementary exposition of the theory which should be adapted to the needs of a large and constantly-increasing class of students." Hence the book is both a practical and a theoretical one. The first part is concerned with the adjustment and comparison of engineering observations in which, after giving an introduction on the principles of probability and the method of least squares, he treats of direct observations upon a single quantity and independent observations upon several quantities, conditioned observations, and the discussions of physical observations.

The second part is devoted to the theory of least squares and probable errors; in this, after a deduction of the fundamental principles, he proceeds to the development of practical methods and formulæ.

In an Appendix he gives Gauss's method of solving normal equations, a list of literature (referred to above), remarks on the theory of least squares, and a few other short notes. A full index is given at the end. There is frequent evidence that the writer has carefully consulted the memoirs he cites in his list, so that while there is nothing of novelty in his treatment that treatment is founded upon the best authorities.

"As I have not written for mathematical experts, they will doubtless find considerable (*sic*) in the book at which to grumble." He points out what may be considered blots in his book. One is that he has adopted Gauss's development of the law of probability of error as the best adapted to an elementary presentation; "If this be objected to as defective, I claim at least the credit of knowing and of pointing out just what and where those defects are."

A consequence, perhaps, of having the work printed in this country is the list of errata. We would suggest in the event of the publication of the historical list, that the dates of reading of the memoirs should be given rather than (or at any rate in addition to) the dates of their publication.

We welcome this work as an evidence of the increasing attention that is being given to mathematics by the author's fellow-countrymen, and hope he will be encouraged by its reception here to follow up its publication with a promised work containing extended applications of the method to higher geodetic surveying and the other problems to which it can be and has been applied.

OUR BOOK SHELF

Holmes' Botanical Note-Book, or Practical Guide to a Knowledge of Botany. By E. M. Holmes, F.L.S., Curator of the Museum of the Pharmaceutical Society of Great Britain, late Lecturer on Botany at Westminster Hospital. (London: Christy and Co., 1878.)

FROM the author's experience at the Pharmaceutical Society, together with that gained during the time he held the lectureship at Westminster Hospital, he is likely to know pretty well the requirements of the students at the pharmaceutical and medical schools. It is not always, however, that a teacher, well acquainted though he may be with what is wanted by the students, is capable of providing the best material to supply those wants. In this note-book we think Mr. Holmes has succeeded in smoothing the path of the botanical course, often so uninteresting and consequently amounting to drudgery to many a student. The plan adopted of

arranging one part so as to work in with another, or rather to lead up to it, is a good one. The aim has been, not to simplify terms, which has often been attempted with varying success, but to reduce as far as possible the difficulty always attending a clear understanding of the meaning of the terms, and indeed to simplify the whole system of teaching. "To this end," the author says in his preface, "two charts of the natural orders are given, in which the diagnostic characters are reduced to a minimum, those which are most easily observed having been chosen as far as possible in preference to the more minute, while all the exceptions have been indicated in an appendix. It is hoped that in this way, the student being familiarised with all the exceptions likely to be met with in this country, some of the difficulties attending a practical study of botany will be removed." The three diagrams of scarlet geranium, daisy and dandelion, and narcissus will be found very useful, as each part of the plant is very distinctly named on the plate itself and is furthermore minutely described in two and a half pages of letterpress. The glossary with the Latin terms accented will be a great help to a young beginner and the interleaving of this part is a good point. Altogether we think the book is very satisfactory. We should, however, have preferred to see the sixty schedules placed at the end of the book rather than in the middle. Placed where they are, one is led to suppose there is no further matter beyond them, which is not the case, the charts and a very useful "Floral Calendar" being placed at the end.

Grundzüge der Electricitätslehre. Zehn Vorlesungen von Dr. W. v. Beetz. (Stuttgart: Meyer and Zeller.)

Dr. von Beetz has published a series of lectures delivered to the members of the medical association in Munich. These lectures do not pretend to contain an exhaustive treatment of the subject. They are meant to illustrate the fundamental principles of the science by a series of well-devised experiments, and they amply fulfil the object for which they have been written. The little book contains much matter in a small space, and is throughout clear and to the point. It will be useful to a wide class of readers especially as an introduction to more detailed treatises. Objections might be raised against some incidental and more speculative remarks; but these are very few in number, and do not affect the chief aim of the book. A. S.

LETTERS TO THE EDITOR

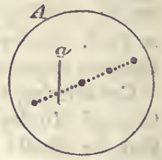
[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Measuring Scales for Pocket Spectroscopes

In using small spectroscopes, such as miniature pocket instruments, to examine coloured flames, and to discover to what particular substances they owe the characters of their radiation, the power of such discrimination which these instruments possess is for many reasons very limited and circumscribed if they are unprovided with some description of dark-field illuminated scale of regular divisions. I have found the following description of measuring scale, when adapted to such small spectroscopes, answer ordinary purposes of recording bright-line positions with them very well, although it was for viewing faint lines in auroral spectra that it was originally devised, for which I have not yet had an opportunity to test its suitability with the same success.

The circular disc A is a piece of copper foil in which a fine slit *a* is punched, and oblique to it a row of twenty holes is punctured, five on the left and fifteen on the right of the slit, the highest and lowest holes of the row being level with its top and bottom points. The actual size of the disc is just half the size of this figure, and to puncture the holes at equal distances apart of about $\frac{1}{80}$th of an inch, either a dividing engine must be used and a needle-point drawn along the sloping face of a straight edge is pressed down with the equal pressure of a weight upon the disc laid on zinc or ivory, to puncture it; or a rack of fifteen or twenty of the finest sewing needles, side by side, may be so fixed in fusible metal as to produce the whole row of punctures by a single pressure. But the first method, even with a roughly made dividing engine for the purpose, I have found the easiest and the most successful plan. The disc takes the place of the jaws of a pocket spectroscope, being dropped into a recess where it is covered by a glass plate and held in its place by a brass ring or perforated cap screwed upon the end of the spectroscope. When viewed through the prisms by sodium light it is seen magnified by the eye-lens, and the punctures form a scale of bright yellow points to the right and left of the yellow sodium line produced by the slit.



With sufficiently large punctures it is probable that the monochromatic yellow-green auroral ray would render the punctured scale visible in the same way that the sodium light does, so as to supply a measuring scale on which other spectral rays of the aurora's light besides the greenish one produced by the slit may be observed and recorded in their actual positions of distance from that leading line. Even the chief green ray of the solar corona in a total eclipse would not improbably illuminate the oblique scale sufficiently to allow the positions of other rays occurring in its spectrum to be recognised and mapped with ease and with considerable accuracy with a pocket spectroscope.

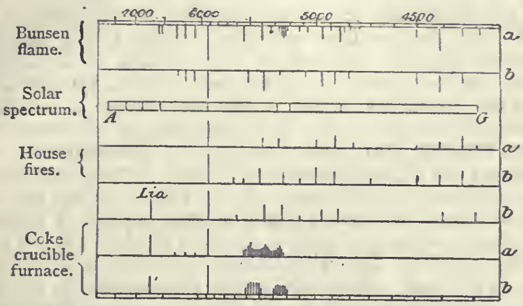
The object of inclining the row of punctures obliquely from a horizontal line is that other coloured images of it besides the principal or brightest one chosen for reference may not mix with and confuse its divisions. There is no means of varying the width of the slit in the arrangement, and I have not succeeded in obtaining microscopic scale photographs on glass sufficiently dense and opaque to replace the metallic punctured scales, and the focus of the eye lens for the yellow sodium points is not exactly the same as for very refrangible blue lines that coincide with them in position in the field of view; but an assortment of discs can be used and may be placed at pleasure in the cell, and the objection of the unequal focus is at least removable by using an achromatic lens. With these drawbacks to its use, however, the punctured scale has one essential advantage over laterally reflected ones, that its relation to the spectrum which it is used to measure always preserves an invariably fixed adjustment.

The presence of sodium light is so easily supplied where it is wanting that there are very few conditions (even if another bright spectral line cannot be chosen) in which the punctured scale is not available. The wave-length curve for each spectroscope is then easily constructed from observations of known elementary metallic lines in flame spectra in which the sodium line is always available for a line of reference. I have obtained this curve for both a punctured scale spectroscope and also for a Browning's miniature spectroscope with a reflected scale, and have examined several commonly occurring flames in furnace and other fires with the result of detecting in them some metallic spectra. Lithium is thus very often found, sometimes almost as bright as the sodium line, in coke furnace fires; and I have frequently observed in ordinary house fires a flame of rich blue colour with a very characteristic spectrum, which I now recognise by measurements as agreeing with that of copper chloride obtained by moistening copper foil with hydrochloric acid in a Bunsen-flame.

The accompanying figure (projected as nearly as the small size of the drawing permits, in tenth-metre wave-lengths) shows the appearance of the natural and artificial spectra, and their close resemblance, showing very clearly that some compound of copper is the cause of the very brilliant blue colour of the natural flame. Its finest exhibition in house-fires (where it quite filled a grate) arose from blazing wood-logs, which were described to me as broken-up ship-timber, in which traces of copper chloride might reasonably be expected to occur, and it is in the wood of fires that its hyacinth-blue flame has usually attracted my attention; but the same spectrum with three neat pairs of

lines, green, blue, and indigo, was noticed also in jets of flame projecting from above the lid of a coke crucible furnace in which copper and brass were being melted (the first of the three furnace spectra in the figure), where the presence of a chloride would not be so readily suspected. The same furnace was revisited, and fired for experiment in different ways, but it only showed the copper-flame spectra drawn in the last two lines of the figure, whose total colour is olive, or tawny green, quite different in aspect from the rich blue of the flame first seen and identified with that occurring in ordinary fires. As refuse brass and copper articles are consigned for melting to these crucibles, it is, however, not improbable that copper chloride may in this instance have been introduced among them.

Attempting to discover the cause of the blue fire-flame by examinations of artificial spectra led me to try that of the blue flame found by Prof. Barrett (NATURE, vol. v. p. 483) to be produced when a burning jet of hydrogen is allowed to play upon



Spectra of the blue flame of copper-chloride, naturally and artificially produced; observed, *a*, with Browning's miniature spectroscope with reflected micrometer scale; *b*, with punctured-scale pocket-spectroscope.

the surface of sulphuric acid, or upon the surface of any object, indeed, which has contracted dust by exposure to the air. It is very abundant, and readily produced everywhere, and not less intensely blue than the copper flame; but no distinct measurements or notable appearances of its spectral bands could be obtained. I could, however, corroborate Prof. Barrett's observations of the extension of the blue colour only on the surfaces touched, and its want of penetration into the body of the flame; and I noticed that metal surfaces rubbed with sulphur which remain cool exhibit it more brightly than wood or other sulphurised surfaces which are quickly heated in the flame. A paste of coke-dust and sulphur wetted with water ceases to tinge the flame (as does also sulphuric acid) when it grows warm, and it fails to colour it when dry. The brightest blue flame was obtained by directing the burning jet upon a mixture of snow with coke dust and sulphur, pounded together in a mortar, the reason apparently being that great attenuation of the sulphur-vapour, and therefore a low temperature of the sulphur, is required to enable the sulphur compound formed to exhibit its characteristic blue-coloured spectrum in the flame.

A. S. HERSCHEL

Zoological Geography—Didus and Didunculus

MY use of the expression "a near congener" seems, from Prof. Newton's letter in your number of July 4, to have diverted attention from the point to which I desired to direct it.

If the bird of the Navigator group had presented such near congenerity to the Dodo as does the other of the ground birds (*Pezophaps*) known from the Mascarine Archipelago, this would have been startling, but as it is the degree of relationship (I avoid the word "congener" which gave rise to Mr. Newton's correction) seems to me to bear out the argument as to former geographical connections which I so long ago advanced.

Both birds belong to the *Columbæ* and to that all but extinct branch of the family to which the term "ground dove" has been applied; and the wingless condition of the Dodo has been by one great living authority accounted for on the hypothesis that by being confined to islands and so secured against enemies, and finding food on the ground, this queer pigeon gradually lost the necessity for, and with this the use of its wings, and thus acquired its bulky form and ground habits. Instead of this, however, I believe that both the *Dididæ* and *Didunculus* are survivors from mezozoic times, of a great family in which the characters that connect these ground birds with the winged *Columbæ* were those common to a large order of wingless birds that, like other orders of mezozoic life, have since perished. From this ancient order of life, or from some yet more ancient stem combining their common characters, the winged *Columbæ* may have been evolved; but of the order itself *Didunculus* has, I believe, survived at the eastern side (longitude 170° W.) of the ancient continent to which I in my first letter alluded, and the Dodo and its kindred at the western (long. 58° E.), in both cases by the protection afforded by insulation.

The application which I sought to make of this to the case of the tortoises was that the presence of those reptiles in the Mascarine and Galapagos archipelagos is due to the same geographical change. The osteological differences between the tortoises of the two regions may, perhaps, be less than those between *Didus* and *Didunculus;* but if so, this, in a lower and cold-blooded grade of vertebrate life, would not weigh much; and my contention is that the tortoises of the Galapagos are insulated there by survival from the eastern extremity of this ancient continent, and those of Aldabra, in the Mascarine region, by survival from the western, instead of from land extending across Africa and the Atlantic to South America, as supposed by Dr. Günther. The fossils of the Himalayan and Mediterranean regions prove that the great tortoises lived on the Europeo-Asiatic continent in miocene and older pliocene times (becoming extinct during the latter), but this does not appear to me to negative the conclusion drawn above.

To prevent misconception, I should, perhaps, add that the land tract from the submergence of which the Mascarine archipelago and the differentiation of the *immediate* kindred of the Dodo originated (as suggested by Prof. Newton in the memoir in the *Phil. Trans.* of 1869, to which he refers), was in my view but a fragment of the more ancient and far more extensive continent which (in 1860) I attempted to show occupied the southern hemisphere in mesozoic times; and that such fragment, again, was but a remnant of a still larger portion of this great southern continent, which, as far back as the triassic period, had become separated from the Australian portion, and, so late as the earlier part of the tertiary, occupied much of the Indian Ocean, where, during the eocene or miocene periods, it formed the cradle of the human race.

SEARLES V. WOOD, JUN.

July 13

P.S.—Dr. Forbes speaks of *Didunculus* being somewhat plentiful still in Upolu.

Smell and Hearing in Insects

IN NATURE (vol. xvii. pp. 45, 62, 82, 102, 162–3), which has just reached me, I see a discussion as to the senses of smelling and hearing in moths, to which I add my mite as an old observer.

I do not see how any one can doubt the first. What but the sense of smell directs nocturnal insects to their food? At this moment I have in my verandah a parrot, which is daily regaled with a portion of a banana. Every evening I see a dozen, or more, of the large *Sphingidæ* and *Noctuæ* trying to effect an entrance into the cage to get at the rotting fruit, *which is generally invisible* from the outside, being behind the flap of wood that serves for a door; the cage is only a rough box. I have always found bananas the best bait to attract the night-flyers, *but only when they began to rot.*

Again, how about "sembling"? Here the odour must be very subtle. A virgin female is instantly detected, while not one "gay Lothario" will visit a captive matron.

It is harder to say whether insects *hear* sounds, or *feel* them, as the same effect would be produced on them by either faculty. I have seen both moths and butterflies turn towards sound, and direct their antennæ to it, moving them to and fro. I have noticed larvæ—as remarked by one of your correspondents—assume certain attitudes on being affected by sound; these attitudes are, I think you will find, generally those assumed for protection or concealment; the creatures are, in fact, alarmed at the unusual—noise? or vibration?—which?

I will adduce one remarkable case in support of the smelling power. Years ago I had (while residing in the North of Ceylon) a lot of living *Achatina panthera* sent to me by the late Mr. Blyth. I placed them in a breeding-cage, and, to secure them from rats, suspended it to the ceiling of my drawing-room. We soon noticed that every night the floor of the room was covered with glow-worms, at which, never having seen them in that part of the island before, and they being of unusual size and brilliancy, we were much pleased.

At last it occurred to me why they came, and on placing the cage on the floor the onslaught on the contents was a convincing proof that *A. panthera* would never be naturalised at St. Pedro.

I could adduce many other instances of the smelling and the "perception of sound" (to phrase it), but will not intrude on your space. E. L. LAYARD

British Consulate, Noumea, New Caledonia, April 26

P.S. Since writing the above I named the subject to Père Montrousier, the celebrated French naturalist so long resident in this colony. He detailed the following experiment that he had made. He immersed a long-snouted weevil (*Octhorinus cruciatus*) so as to cover it, all but the tip of the antennæ, with a coating of wax. On presenting to it oil of turpentine it became violently excited and endeavoured to escape. Another now had the tips of its antennæ only coated with the wax, and neither turpentine nor any other strong smelling substance at all affected it. He places the faculty of the "perception of sound" equally in the antennæ. Since the discovery of the telephone who shall say to what extent these delicate organs can recognise the vibrations of the air? And after all, what is our own "perception of sound" but the appreciation of a vibration? E. L. L.

On the Lichen Gonidia Question

THE morphological side of Schwendener's theory may be now regarded as fully proved. The opponents have confined themselves on the whole to *à priori* arguments, and of those who have applied themselves to carefully working out afresh the relations of hypha and gonidium, several, if not all, have been converted to the new views. No one can have much faith in the haphazard style of preparation and examination known, *par excellence*, as the "lichenological." However, one or two points remain which have not as yet received much attention. These are the beautiful symmetry of the lichen as a whole, the rareness of the application of the hypha to the gonidium, and the generally healthy look of the gonidia themselves. All this contrasts greatly with what we find, as a rule, in the relations of parasitical fungus and host.

With regard to the first objection I may call attention to the equally beautiful and symmetrical forms we find in galls, such as the spangles of the oak, the rose bedeguar, and the exquisite rosettes of certain *Dipterocarpeæ*.

It is, however, well known that many of the lower fungi can build up their protoplasm and live perfectly, if in addition to the salts needed for the growth of all plants (including nitrates or ammonium salts), there be present a tartrate or sugar. Now the gonidia, like the algæ with which they correspond, excrete as a cell-wall a thick layer of gelatinous consistency but giving reactions which show it a form of cellulose. It is in contact with, or through this that the hypha ramifies, and from this it can take up the necessary complement of the mineral food supplied by the substratum and medium. It can grow freely; and the gonidium, with its protoplasm intact, can go on growing as comfortably as the oyster infested by *Clione*. Perhaps like the oyster it may be stimulated to a more active secretion of envelope, but its health is unimpaired. Hence, too, there is no physiological need for the hypha to come in contact with the gonidia, and the last argument of the old school becomes untenable

Owens College, Manchester MARCUS M. HARTOG

The Phonograph

IN experimenting lately with the phonograph it occurred to me to try whether, after a series of musical or articulate sounds have been recorded, other series could successively be superimposed on the same tinfoil and reproduced. I found that if the instrument be simply reset to the starting-point, and sung or spoken to a second time, it will afterwards faithfully reproduce both series of sounds as though two persons were singing or speaking simultaneously, and by repeating the same process, a third and fourth voice may be added, or one or more instrumental parts, all of which will be reproduced. This experiment forms a striking commentary on Helmholtz's theory of the mode in which the ear recognises different tones in a chaos of sound, by analysing the compound wave, which it receives, into its component simple vibrations. Here the aggregate impressions on the tin-foil produce, so to speak, a compound indentation capable of reproducing a wave of sound which the ear can resolve into the original constituents.

Temple GEORGE P. BIDDER

Remarkable Form of Lightning

DURING a thunderstorm on Sunday afternoon, August 24, 1873, I saw a flash of lightning here exactly answering to Mr. Joule's description of "punctuation." The note of the storm in my diary says:—"Lightning and thunder very frequent but not violent. *One flash, very near, had the appearance of a chain of alternate links*, and remained visible, I should think, for half a second, gradually fading out." This persistence was, no doubt, mainly an optical illusion, but it shows the definiteness of the form. The flash was from cloud to cloud, and followed a very sinuous line, as described by Mr. Lawrence. Is not this what old books describe as "chain lightning?"

B. WOODD SMITH

Branch Hill Lodge, Hampstead Heath, July 12

OUR NEW PROTECTORATE

WE have only to do with politics in this journal in so far as they concern science; but without pronouncing any opinion on the wisdom of the action taken by our government in the Eastern question, it may not be amiss to say a few words from a scientific standpoint on the interesting territories which have just been brought into close relation with this country. To England the region included in Asiatic Turkey is in some respects the most interesting in the world. If not exactly the cradle of our family, there is good reason to believe that it is in close proximity to it; and no doubt it was one of the pathways by which the early Aryans sought their way to Europe. Historically and prehistorically, for the student of religion and the student of science, Turkey in Asia possesses features of the highest interest, and we may hope that one result of our new connection will be that our very imperfect knowledge of it in its various aspects will be rapidly filled up. Its shores—the Black Sea on the North, the Ægean on the west, the Mediterranean on the south, and, may we say, the Euphrates and Tigris rivers on the east—teem with historical associations. A careful investigation of its mountains and valleys, its rivers and numerous salt lakes, would doubtless yield the geologist a rich harvest of results, bearing everywhere as they do unmistakable evidence of former powerful volcanic action.

Asiatic Turkey, in its five great divisions of Anatolia or Asia Minor, Armenia, Kurdistan, Mesopotamia, and Syria, may be regarded as a western extension of the great central Asian plateau, with its surface much broken up by mountain chains and isolated ranges. This great plateau narrows very considerably as it approaches the Turkish territory in Asia, but increases in elevation. Here begins the Alpine region of Persia with Kurdistan; here are the lakes Urumiyeh and Van, and the sources of the rivers Zab, Tigris, Aras, and Euphrates. The table-land is broken up into and replaced by mountains, which rise to a great height, and by elevated valleys between them. On the north-east of Turkey-in-Asia both the mountain-ranges and the table-lands are united in the compact mountain-region and high table-land of Armenia, the country to the west resembling Europe in structure rather than Eastern Asia. Physically there are four divisions of this region, corresponding nearly to the divisions referred to above. The first is the elevated and mountainous table-land of Armenia, which extends in the form of a triangle between the angles of the three seas—the Caspian, the Black Sea, and the Gulf of Alexandretta on the south. Its central plain, on which stands Erzeroum, about which so much has recently been heard, rises to 7,000 feet above sea-level, and the highest peak of Ararat rises to above 17,000 feet.

The second great division is formed by the Caucasus, which is beyond the range of our present subject. The third separate mass is formed by the peninsula of Anatolia, or Asia-Minor, in the interior, a table-land of an average height of 3,000 feet, and joined to Persia by the mountain-chain of the Taurus. The Syrian mountains

form the fourth division, culminating in Mount Libanus and terminating in the isolated mountain-mass of Sinai. The whole extent of Turkey-in-Asia is estimated at 660,000 square miles, and its population variously estimated at from ten to twenty millions.

The most extensive and altogether most remarkable mountain-chain of Turkey-in-Asia is the Taurus, with its offshoot the Anti-Taurus, belonging mainly to the division of Anatolia. The Taurus begins on the east, by the Euphrates, where one of its peaks rises to nearly 10,000 feet, and runs irregularly westwards not far from the Mediterranean coast, through Caramenia and Lycia, ending in the islands of the Greek Archipelago. Both to the north and south it sends out shoots, the island of Cyprus itself being really a spur of the great mountain-mass. The northern arm, the Anti-Taurus, runs north-east; and at the Chain may be said to turn in a westerly direction along and at no great distance from the Black Sea to the Bosphorus, sending out a south-west spur culminating in Mount Olympus, near Broussa, and further south, on the Gulf of Adramyti, in Trojan Ida.

The separate portions of the Taurus inclose many plains and valleys, which lie terraced above each other in the line of the meridian. On the south side of the mountain lies the plain, formed from augitic rocks, of Diarbekr in Kurdistan, 1,800 feet above sea-level; in the middle of the Taurus is the cultivated valley of Alendah, and Lake Gorjik Gol, 4,000 feet high; on the north side is the plain of Liwas, 4,000 feet, and that of Baulus, 3,000 feet, above sea-level, from which the land sinks rapidly to the Black Sea. In the south-east part of the peninsula rises the isolated peak, having little connection with the main range, the Erdjas Dag—the Mons Argæus of the ancients. It stands on the plain of Kassarieh (Cæsarea), its foot being 3,300 feet above sea-level, and its summit, the culminating point of Anatolia, close on 13,000 feet above the sea. It consists entirely of volcanic products, and its summit contains two craters, long ago extinct. The whole inner plateau, west to near Kutaya and thence to the plain of Sardis and even to the west coast at Smyrna, bears evident traces of volcanic activity. Extinct volcanic cones, often of considerable height, lava-streams and other unmistakable signs of subterranean activity, extend over a considerable space. Earthquakes are of frequent occurrence, and warm sulphur springs are numerous in Anatolia.

In the Anatolian peninsula the rivers flow mostly north-westwards into the Black Sea, though the courses of not a few of them remain to be explored. The most considerable of these is the Kisil-Irmik (the ancient Halys), though one of the most interesting is the Mendereh (ancient Meander), celebrated for its luxurious valley and winding corners, and for the fact that since Homer sang of it, the action of its current, combined with the action of the sea, has altered the whole aspect of the coast about Besika Bay.

The climate of Asiatic Turkey presents so many variations on account of the great inequalities of its surface, that any general view of it is impossible. In one day the traveller may go from the cold of winter to a heat almost tropical, and *vice versâ*. In the Mesopotamian and Bagdad regions, at the head of the Persian Gulf, and along the banks of the Tigris and Euphrates the heat in summer is quite tropical. Sir Frederic Goldsmid in his "Telegraph and Travel," a work which contains many valuable notes on the features and condition of the country in 1864, found it average 96° in the shade near Mosul in the beginning of June.

The Anatolian peninsula gradually blends eastwards into the highlands of Armenia, which unite the mountains of Asia Minor with the great system of Central Asia, and give rise to the two great rivers of Asiatic Turkey, the Euphrates and the Tigris. Armenia is a land of terraces. Between the rivers rise dividing mountain-ranges; within and between these ranges are wide, mostly level, steppe-like plateaux of various heights, which lie like terraces over each other; deep-cut valleys, gloomy, towering mountain masses; extreme climate, with severe winter and dry hot summer; in the valleys and on the mountain-slopes luxurious vegetation, but scanty on the plateaux; on the eastern border the landscape is Alpine, and forms the immediate connection between Armenia and the great table-land of Iran. The Armenian mountains are continued southwards into Kurdistan, gradually shading off into the great plain of Mesopotamia. In the north of Kurdistan lies the romantic salt lake Van, 1,200 square miles in area, at a height of 4,000 feet above sea-level. The two streams which water Mesopotamia, the Euphrates and Tigris, have a generally parallel course, sometimes approaching and sometimes receding from each other. At Bagdad they approach most closely before uniting, not far above the outlet in the Persian Gulf, giving the included land the shape of an hour-glass. It was this included land which the ancients appropriately named Mesopotamia, the northern half being now known as El Jesireh, or the island, and the southern Irak Arabi, or the Arab Irak, to distinguish it from the neighbouring Persia, or Irak Ajemi. The delta of the united stream begins about forty miles above its outlet, and there is evidence that since the time of Alexander the Great, the land must have encroached considerably on the Gulf. Lying between Mesopotamia and the coast region of Syria, and its southern part, Palestine, is the great Syrian desert, a chalk plateau of about 1,800 feet above the sea, bound on the west side by a great depression.

The flora of Asiatic Turkey, as might be expected, is very varied, partaking of a combined temperate and sub-tropical character. As to its fauna, the lion has disappeared from the countries west of the Euphrates, while in Mesopotamia are found the hyæna, panther, buffalo, and wild boar; jackals, bears, wolves, and wild hogs are met with in Asia Minor. The leopard is still found in the interior of Palestine, the Syrian bear in Lebanon, while European animals are found nearly everywhere. The whole territory is included in the Mediterranean sub-region of the Palæarctic Zoological region. (See Wallace's "Distribution of Animals.")

With regard to Cyprus a volume might be written on its history, from the time of the Phœnicians (it is supposed to be the Chittim of the Old Testament) till now, and we quite recently noticed Gen. Cesnola's remarkable work on the antiquities of the island. The ancients appropriately compared the shape of the island to that of a deer's skin or a fleece spread out. In length it is about 140 miles, and about 45 in breadth, much about the size of Skye and the Long Island from Barra Head to the Butt of Lewis together. The centre of the island is a plain or table land, while mountain-ranges occupy the west and south-west, and the northern coast is mountainous along its whole extent. In the northern range the highest summit does not exceed 3,340 feet, while among the southern masses, Mount Olympus (Trodos or Troödos) reaches a height of 6,590 feet. Other summits range from 2,000 to 5,000 feet. In the time of Titus a volcanic outburst from the northern range did great damage, and destructive earthquakes seem to have been at one time frequent. The streams are few and small; rain is almost unknown from May to October; the heat of summer is excessive on the plains, though the winter is mild, and the climate on the whole may be regarded as healthy. At one time the island appears to have been thickly wooded and to have yielded valuable mineral and vegetable products. The island is said to have then sustained a population of a million, but now the inhabitants do not exceed 180,000. Speaking of the flora of the island Drs. Unger and Kotschy in their work "Die Insel Cypern," say:—

"In Cyprus prairie or meadow land does not exist; the

'Ackerland' takes the place of it. After the rains, but only for a short time, cereals give a satin-like green to the landscape; and among them grow a profusion of flowers; but these artificial rather than natural fields fade more quickly than the flowers, and scarcely last a few weeks beyond the last spring rain. There is only one small corner of the island where the vegetation resembles ours. The great heat of the summer destroys all the tender plants; only those plants survive which through their anatomical construction, or hard substance, or in consequence of growing near water, can resist the effects of the heat.

"There is great resemblance in the vegetation throughout the island to the Mediterranean. In February and March there is on all the river edges a profusion of lilies; in April and May on the land side is one carpet of flowers. During the heat, however, the land assumes a yellow tint. Pine forests abound, olives, myrtles, and laurel trees. As far as the island has as yet been explored we know that there are 1,000 different sorts of plants. No eastern island can show such a rich forest growth as Cyprus.

"The *Pinus maritima* in Cyprus covers the hills and mountain regions to the height of 4,000 feet as one of the commonest trees. The *Pinus laricio*, which covers all the heights to 4,000 feet above the sea, rises on the western mountains of the island to 6,000 feet, and gives it a dark appearance from the sea. The wild cypress (*Cupressus horizontalis*) is the third tree which grows commonly in the eastern part of the island and in some places forms by itself whole woods. On the whole of the northern chain of mountains this wild cypress grows often to the height of from 2,000 to 3,000 feet above the sea. Great forests of wild cypresses must also have covered the whole of the south of the island, as also a shrub, the *Juniperus phœnicea*. In the north several varieties of oak are found, and throughout the island the arbutus abounds; the carob-tree and olive flourish on the banks of all the rivers and up to an elevation of 1,000 feet above the sea."

Dr. Unger's work gives a catalogue of the fauna of the island, which includes a considerable number of troublesome insects. Copper, gold, silver, and precious stones were at one time found in considerable quantities, and the mineral resources of the island are probably capable of great development. Doubtless one of the first cares of the new proprietors will be to obtain an accurate survey and estimate of its resources.

No less important than the physical are the ethnical conditions of the vast region to which we have just undertaken the responsibility of introducing the blessings of good government. Indeed, from the administrative point of view, a correct knowledge of the inhabitants of any country is almost more necessary than is that of their outward surroundings. Yet the most profound ignorance too often prevails regarding the affinities and characteristics of the peoples, the direction of whose destinies has been either assumed or thrust upon "the Mother of Empires." How few of our Indian administrators have yet succeeded in grasping the difference between *Aryan* and *Dravidian*, not to speak of *Kolarian*, and how many still affect to speak collectively of all the natives as "Niggers"! If it is so with a country which has been under British rule for upwards of three generations, no very general or accurate knowledge can be expected of the ethnography of Asiatic Turkey, with which our relations have hitherto been of a purely commercial character. Hence no apology will be needed for here submitting a few notes on the subject, for which we are indebted to Mr. A. H. Keane, B.A.

Apart from the question of the Autochthones, if any still survive, three distinct stocks are at present in possession of Turkey-in-Asia, taking the term in its widest sense, so as to include parts of the Arabian peninsula, as well as Syria, Mesopotamia, Armenia, and Asia Minor proper. These stocks or racial families are the Ural-Altaic, Aryan, and Semitic, each of which, omitting such minor distinctions as Juruks, Gipsies, Samaritans, Nestorians, Chaldæans, may be said to be represented by three separate offshoots, as clearly shown in the subjoined scheme. Here the various nationalities are grouped in the first, second, and third columns, according to their ethnical, linguistic, and religious connections respectively, while in the fourth an approximate estimate is given of their numbers, say twenty millions altogether.

		Language.	Religion.	Population.
I. Ural-Altaic Stock.	Turks	Turkish	Muhammedan	12,000,000
	Turkomans ...	Tatar dialect...	Muhammedan	300,000
	Kysyl-Bashes	Turkish	Pagan	?
II. Aryan Stock.	Hellenes ...	Greek	Orthodox and United Greek	2,000,000
	Armenians ...	Armenian ...	Orthodox and United Armenian ...	3,000,000
	Kurds	Kurdish; Zaza	Muhammedan mainly... ...	1,000,000
III. Semitic Stock.	Arabs	Arabic	Muhammedan	1,500,000
	Maronites ...	Arabic	United Syrian	30,000
	Druses	Arabic	Pagan	40,000

Mention should also be made of the few Circassians still surviving of those who, some years ago, fled from the sword of the Russians, and of the few thousand Lazes still left to Turkey by the Berlin Congress. Both belong to the southern branch of the CAUCASIAN STOCK, which is entirely distinct from any of the foregoing. Nor should the Jews be overlooked, who, though still numerous in some of the larger cities (10,000 in Jerusalem alone), have almost disappeared from their original homes.

But of the really representative peoples in these regions the Turks are undoubtedly entitled in every respect to our first consideration. Anatolia, that is to say, all the country between the Upper Euphrates and the Ægean Sea, and from about the 36th parallel northwards to the Euxine has for centuries been the true home of this race. Although even here intermingled in the west with the Greeks, in the east with the Armenians, Kurds, and Arabs, they form, on the whole, the great bulk of the population of Asia-Minor within the specified limits, presenting a compact and homogeneous mass—homogenous in every sense of the word, in race, speech, and religion. They are unquestionably of pure Tataric descent, their Muhammedan prejudices having enabled them to keep aloof from the surrounding populations ever since they entered the country as conquerors in the eleventh century. Hence it is that Anatolia has long been the true backbone of the Turkish rule, a backbone reaching even across the Bosphorus, and that in Anatolia alone is it possible profitably to study the true character of the Osmanlis.

Doubtless the word "Turk" itself is now eschewed in Asia-Minor, where it has become almost a term of reproach corresponding to our "clod-hopper" or "yokel." But this simply means that the Anatolian Turks have become essentially a rough peasant people, as contrasted with their more refined kinsmen of Roumelia and Constantinople. It would be difficult to imagine a greater contrast than is presented by the Asiatic and European branches of this race, though it is of the last importance that the difference should be thoroughly realised before a just estimate can be formed of the Turks as a factor in the calculations of statesmen. They have, unfortunately, been too often judged from the polished and somewhat effeminate Effendis of the Capital, as many superficial observers are apt to confound the gay, frivolous *jeunesse dorée* of the Paris boulevards with the plodding and really thrifty agricultural people of France.

The Anatolian Turks are a lusty, stalwart race, of rude manners and harsh utterance, still speaking nearly in its purity the primitive agglutinating Turkish tongue, which in Stambûl has become a sort of Arabo-Perso-Tatar medley. They are not, perhaps, over-industrious, cultivating little more than is needed to supply their modest wants, and showing a preference for the fig, the vine, and the olive, plants yielding bounteous returns for the little care bestowed on them. Though by constitution extremely frugal, with few and simple belongings, and living in the humblest of dwellings, they are still generally oppressed with debts, and at the mercy of the usurer and the tax-gatherer, the former relentlessly exacting his pound of flesh, the latter often farming the public revenues, forestalling the tithes before harvest-tide and basing his estimates on calculations not always realised even in more favoured climes. Hence many yearly give up their holdings, sinking to the position of proletariates, the day-labourer's life being in many respects preferable to that of the small tenant farmer left unprotected by the authorities and an easy prey to the unscrupulous in a country where the administration of justice leaves much to be desired.

Fortunately for their rulers, past and to come, the Anatolian Turks are a patient, much-enduring race, kindly, hospitable, and tolerant in religious matters. Of an earnest, taciturn temperament, with much sound understanding and shrewd observation, they are yet devoid of foresight and business habits. Hence they make, as a rule, indifferent merchants, so that most of the wholesale trade has fallen into the hands of the rival races. In the country districts they are simply tillers of the land and stock-breeders, in the towns dealers in small wares mostly of home production, or else craftsmen employed in such industries as are needed to supply the few wants of Turkish life. Their seafaring qualities, however, have been unjustly decried, for in the hands of efficient officers they make excellent sailors, while as organisers and conductors of caravans they are unsurpassed. Their greatest shortcomings are perhaps a certain apathy due mainly to the universal belief in *Kismet*, or "the Inevitable," combined with the absence of progressive ideas and indifference to the future. Heedless of the morrow they will often pay exorbitant interest to escape from present pressure. Hence where mingled with other peoples they have fallen somewhat behind in the race, though never sinking to abject want, so modest are their needs, so rich their lands in varied resources. Military service, also, as is well known, weighs heavily on them, and on them alone, helping with polygamy, and all its accompanying evils, to account for the steady decrease of the Turkish element for some years past, especially on the coast.

Here the somewhat indolent Osmanli has had to confront the more versatile Greek, still clinging to his old Ionian homes along the eastern shores of the island-studded Ægean, and it is not, perhaps, surprising that under such circumstances his quick-witted rival has largely succeeded to his inheritance. For the second time in the history of the Hellenic race, *Græcia capta feros victores cepit.* An industrious trader, a shrewd calculating merchant, an excellent seaman, an intelligent agriculturist, the Greek outstrips the Osmanli in his own special province, while monopolising the learned professions. Smyrna has thus again become a Greek city, and the Greek race has in modern times everywhere displayed a praiseworthy zeal for the spread of education, while fostering among the people a healthy national sentiment. A well-directed and widely-ramifying association, radiating from Athens, encourages a movement which has tended more than anything else to maintain the influence of the Hellenic race in western Anatolia, despite their numerical inferiority.

Next to them in importance are the Armenians, sparsely diffused throughout the east from Constantinople to Calcutta, and still existing as a distinct nationality in the north-eastern highlands of Anatolia. Intellectually almost on a level with the Greeks, out-rivalling them in commercial enterprise, the Armenians present certain distinctive physical, social, and moral characteristics by which they are readily recognised wherever met. Conspicuous amongst these traits, besides their speech, belonging to the Eranian branch of the Aryan family, are their national dress, their bushy close-set eyebrows, and a decidedly unlovable disposition, which has earned for them the dislike and contempt of their neighbours. Their trickery and avarice have become proverbial throughout the East, and after making all due allowance for exaggeration, they cannot be altogether acquitted of a certain moral obliquity. Deprived for generations of all political rights, they have taken eagerly to trade like the Jews in Europe, like them in many places monopolising it, ruling the money market, and notwithstanding mutual family jealousies ever ready to band together and make any sacrifices for the common good. As traders they certainly display an amount of keenness and cunning, though of a somewhat low order, dealing by preference in "the cheap and nasty," and retailing their "shoddy" and "Brummagem" wares at exorbitant prices to an ignorant *clientèle.*

Constitutionally timid and reserved they may on the whole be regarded as a feeble race, rarely appealing to arms in self-defence, in all cases ever ready to yield submission to the strongest. Of all Christian peoples the Armenians harmonise best with their Turkish rulers; they habitually speak Turkish like a second mother-tongue, and come nearest to the Osmanli in their quiet, earnest disposition.

Of far different temperament are their southern neighbours, the fierce, freedom-loving Kurdish highlanders. Long recognised as belonging also to the Eranian branch of the Aryan stock, in which they seem linguistically to approach nearer to the Persian than to the Armenian sub-division, the Kurds have been variously depicted, according to the sympathies of the writer, as brave, chivalrous mountaineers or else treacherous, lawless, and blood-thirsty marauders. All, however, agree in describing them as of a restless and unruly disposition, some attributing this quality to the effects of Turkish misrule, others to inherent national temperament, the latter appealing with some plausibility to the sentiment of antiquity, according to which the fierce *Carduchi* of Xenophon evidently bear a strong family likeness to their modern descendants.

What may be called the disturbing element in Asiatic Turkey is continued from Kurdistan southwards to Arabia by the Bedouins of the Syrian desert. Half savage Kurdish tribes in the uplands about the head streams of the Tigris and the Euphrates, almost equally restless nomad Arab tribes in the plains watered by those rivers will for a long time tax all the watchfulness of a strong and wise administration. Nominally subjects of the Porte, the Shamara, Beni-Lam, and other powerful Arab tribes have long maintained an ill-disguised standing feud with the authorities, often making their presence unpleasantly felt, especially along the right bank of the Euphrates from about the parallel of Aleppo all the way to the Persian Gulf. If united they might easily bring from 10,000 to 20,000 formidable mounted warriors into the field. But here as elsewhere tribal dissensions neutralise their power, enabling the Turks still to keep the upper hand in the Mesopotamian plains, and show a fair front towards the Persian frontier.

A glance at the population column in the above scheme will show at once that the peoples hitherto touched upon —Turks, Greeks, Armenians, Kurds, Arabs—can alone possess any real importance for the future administrators of these regions. The Kysyl-Bashes, Juruks, Druses, and Maronites, doubtless present many curious problems

to the ethnologist and philologist. But they are numerically too insignificant to claim further notice here.

The Island of Cyprus presents no fresh ethnical elements beyond those specified in our scheme. The bulk of the population are Greek, or, at all events, a mixed Phœnician, Carian, and Greek people that have long been Hellenised. The rest are mainly Turks, and both have hitherto been permitted to live harmoniously together. They are not likely to prove a source of trouble to their new rulers.

As to the future of this varied and interesting region, it is not for us to speak. Everywhere there are evidences that at one time it must have been thickly populated and its resources highly developed. What the country is capable of may be learned from the classical reports of Palgrave, Scherzer, and other British and foreign consuls, as well as from the various special reports on the much-talked-of trans-Asiatic railway. In this connection books worth referring to are Palgrave's "Essays on Eastern Questions," Goldsmid's "Telegraph and Travel," and Goldsmid and Blanford's "Eastern Persia." Good authorities to consult on the geography and science of the region are the various articles in the "English Cyclopædia," recent volumes of Petermann's *Mittheilungen*, Hellwald's "Die Erde und ihre Völker," the *Bulletin* of the French Geographical Society, Chihacheff's "Asie Mineure," Schliemann's and Cesnola's works, Thielmann's "Caucasus, Persia, and Turkey," Unger's "Die Insel Cypern," besides older well-known works.

TYCHO BRAHE'S CORRESPONDENCE[1]

We have received the first three *fasciculi* of this work, projected by M. Friis in 1876. Its purpose is to place in the hands of the astronomer, in a collective form, the letters of Tycho and his correspondence, preserved in the Royal Library at Copenhagen, and in the libraries of Vienna, Pulkowa, and Basle, and others which may be found elsewhere, and it is expected that the work will be complete in about sixteen parts. The earliest letter is one from Tycho to Joannes Aalborg, afterwards librarian at Copenhagen, dated January 14, 1568. There are letters to or from Steno Bille, or Bilde (an uncle of Tycho's, at whose house, it may be remembered, he detected the celebrated star of 1572 which is associated with his name), Thaddæus Hagecius, physician to the Emperor Rudolph II., Paulus Haintzel, Hieronymus Wolfius, and others, whose names occur in the well-known treatise, "De Nova Stella Anni 1572." In a letter, No. 47, written in 1584, to Henricus Brucæus, Tycho enters into some discussion of the "Hypothesis Copernici," in another to Hagecius (we follow the Latin names in use at the time) he refers at length to the parallax of the comet of 1577, observed by him with much care; from his observations of this body, as Pingré says, "on en concluait que le lieu des comètes était au-delà du ciel de la Lune."

The third part contains a finely-executed portrait of Tycho (Woodburytype) from the oil painting in the possession of Dr. Crompton, of Manchester, for information respecting which M. Friis refers his readers to NATURE, vol. xv. p. 406, and vol. xvi. p. 501; an account of it also appeared in the *Proceedings* of the Manchester Literary and Philosophical Society, October 31, 1876.

We may express the hope that the success attending the publication of the first three numbers of this work may be sufficiently encouraging to induce a more rapid issue of the remaining parts.

[1] "Tychonis Brahei, et ad eum doctorum virorum Epistolæ nunc primum collectæ et editæ," a F. R. Friis. (D. Nutt: London.)

OUR ASTRONOMICAL COLUMN

PERIODICAL COMETS IN 1879.—Of the known comets of short period, two will pass through perihelion in the spring of the ensuing year. The comet discovered by Brorsen at Kiel in February, 1846, and since observed in 1857, 1868, and 1873, according to the elements deduced at the last appearance by Dr. Schulze, will arrive at perihelion again on April 1, perturbations, which must be light in the actual revolution, being neglected. This comet still approaches very near to the orbit of the planet Jupiter, though perhaps not quite so close as in 1842, when the present form of orbit was impressed upon it by the action of the planet, the point of nearest approach being at a true anomaly of 167° 48′, or in heliocentric longitude 283° 30′ (Eq. 1870); when last passing this point of its orbit, early in October, 1875, Jupiter was distant from the comet, 5·58, whence the effect of his attraction upon the length of the present revolution will be comparatively trifling. At the ascending node the comet may approach pretty near to Venus, as was the case in October, 1873, a few days previous to the last perihelion passage. To obtain an idea of the track in the heavens in the spring of next year, we may assume that the comet will arrive at its least distance from the sun at midnight on April 1 (guided by Schulze's elements) and will have the following positions:—

12h.	R.A.	N.P.D.	Distance from Earth.	Distance from Sun.	Intensity of Light.
March 12	23·6	89·7	1·35	0·71	1·08
,, 22	32·4	80·5	1·20	0·63	1·77
April 1	41·6	69·7	1·05	0·59	2·58
,, 11	51·7	57·5	0·91	0·63	3·06
,, 21	63·3	45·9	0·81	0·71	3·00
May 1	85·0	33·3	0·74	0·83	2·66
,, 11	118·6	27·2	0·71	0·96	2·12
,, 21	154·2	30·8	0·74	1·09	1·53
,, 31	176·3	40·1	0·81	1·23	1·02
June 10	189·0	50·2	0·91	1·36	0·65

Whence it may be expected that the comet will be observed in the latter half of March, attaining its greatest brightness as it traverses the constellation Perseus, about the middle of April.

The second comet due in 1879 is that discovered by M. Tempel at Marseilles in April, 1867, and re-observed in 1873, after undergoing great perturbation from a close approach to the planet Jupiter, early in 1870. The best elements for 1873 are those of Sandberg, according to which the next perihelion passage would fall on April 26, without taking into account the effect of planetary action, which, as in the case of Brorsen's comet, is not likely to be material in the present revolution; indeed, when the comet was last in aphelion, and nearest to the orbit of Jupiter, the planet was on the opposite side of the sun.

Assuming, then, that the next perihelion passage will take place at midnight on April 26, the following positions and distances will result:—

	R.A.	N.P.D.	Distance from Earth.	Distance from Sun.	Intensity of Light.
April 26	261·8	106·5	0·916	1·770	0·38
May 16	262·9	109·3	0·819	1·778	0·47
,, 26	262·1	111·0	0·796	1·789	0·49
June 5	260·7	112·6	0·791	1·804	0·49
June 15	259·1	114·5	0·808	1·823	0·46

The comet under the above condition, will therefore be situate during the whole period in the southern part of the constellation Ophiuchus, and it may be hoped that it will be well observed, as, during the ensuing revolution, material perturbations of the elements may be again occasioned by the action of Jupiter, from which body the comet at the beginning of October, 1881, may not be distant more than 0·55, a degree of approximation that, although not sufficient to lead to such heavy disturbance of the comet's motion as in 1870, will yet render a precise determination of the orbit in 1879 very essential for an accurate prediction of the apparent track in 1885.

In September, 1879, another return of Biela's comet will be due with the elements of 1866, but we reserve a few remarks upon this subject for another note.

A New Comet.—By telegram to the Vienna Academy of Sciences, with which body rests the award of the medal for cometary discoveries, it is announced that a telescopic comet was detected on July 7 by Mr. Swift, of Rochester, N.Y. The place at 14h. was in R.A. 265° and N.P.D. 72°, if the telegram is to be read according to the suggestion of the late Prof. Littröw; the comet was a faint diffused object, and had a slow motion towards the south-west.

It would be an advantage if arrangements could be made for the communication of telegraphic notices of these discoveries to the Royal Astronomical Society, which is the proper centre for such information in this country.

Minor Planets.—On June 26 Prof. Peters detected No. 188 in R.A. 15h. 37m., N.P.D. 106° 18′, shining as a star of the twelfth magnitude. No. 173, discovered by Borrelly on August 2, 1877, has been named *Ino*, and No. 180, which was found by Cottenot on February 2, 1878, it is proposed to call *Eucharis*.

Saturn's Satellites.—Mr. Marth has again prepared, evidently at great trouble, ephemerides of the five inner satellites of Saturn, which will be found in Nos. 2,205-6 of the *Astronomische Nachrichten*, as far as October 23, the conclusion to follow. With such elaborate prediction, the regular observation of these faint objects should be assured; indeed Mr. Marth's exertions in this direction have already led to excellent results.

BIOLOGICAL NOTES

The Male of Salpa.—The development of the spermatozoa in the Salpæ has hitherto not been satisfactorily studied. Only two years ago Mr. Brooks, of Boston, stated that the testis developed from the elæoblast, and moreover maintained that of the two generations which alternate in these pelagic Tunicates—the one set, the "chain" Salpæ, are exclusively males, whilst the other set, the "solitary" Salpæ, are exclusively females. This view involved the theoretical assumption that the single egg which is found in every individual of a chain of Salpæ does not *really* belong to that individual which is only a male, and has the egg laid into it by the solitary Salpa from which the chain is derived by budding. Accordingly, the elæoblast in the solitary Salpa which, according to Brooks, is female, represents the testis and points to a primitive hermaphroditism; whilst in the chain Salpæ (actual males, according to Brooks), the elæoblast becomes testis. The more usual view is that the solitary Salpæ are not sexually differentiated at all, and that the chain Salpæ are hermaphrodites. Prof. Salensky, of Kasan, has recently published (*Zeitsch. wiss. Zoologie*, 1878, Supplement 2) some observations on this matter, having previously given a very careful account of the development of these organisms, and at the same time he enters into a discussion of the relationships of various Tunicata which has much interest. He shows that the eggs found in the chain Salpæ cannot be regarded as given off from the solitary mother into the budded chain because there is no specialised ovarian cord or rudiment in the proliferous mother. She differs in this respect from the adult proliferous persons in Pyrosoma—which really, as shown by Huxley and by Kowalewsky, give to their buds a part of their own ovarian rudiment. The solitary Salpa has nothing of the kind to give. Further, Salensky shows that the elæoblast has nothing to do with the testis. It exists in the solitary Salpa, and in the chain Salpa appears only for a brief period, and then disappears; but is certainly *not* developed into a testis. Accordingly the solitary Salpa is devoid of all trace of either ovary or testis. The elæoblast appears very probably, according to Salensky, to represent the notochord. The tailed larvæ of the true Ascidians possess a well-developed notochord, and present to us the ancestral form of the Tunicata, which only persists to the adult condition in the Appendiculariæ. In the Ascidiæ the tail and notochord atrophy as development advances. In Doliolum the young form which develops from the egg has a short tail with an axis apparently intermediate in character between the notochord of the Ascidian tadpole and the elæoblast of the Salpæ. The sexual form of Doliolum developed by budding from a second sexless generation, is devoid of tail. Salensky holds that we must distinguish in the Tunicata such simple budding from the adult as is presented in the Pyrosoma colonies and others, and that kind of budding which definitely characterises the alternation of generations morphologically distinguishable from one another. The sexless nurse, constituting the one generation, appears to retain the characteristics of the ancestral Tunicate form with tail and notochord. It corresponds to the Ascidian tadpole, and is represented in more or less completely modified condition by the tailed sexless nurses of Doliolum, by the solitary Salpæ, and by the Cyathozooid or primary person of the Pyrosoma colony, which gives rise to the colony by a process of budding which it is necessary to distinguish very widely from that which the persons of the colony exhibit at a later stage themselves. Just as the Ascidian tadpole becomes itself atrophied and metamorphosed so as to form the sexually mature Ascidian, so do the "nurses" above mentioned give rise by budding to a generation not possessing their own archaic characters, but bearing sexual organs and corresponding to the adult Ascidian, and thus we have an alternation of form in the successive gamic and agamic generations. Should multiplication by budding or fission be confined to the later sexual phase, then there is no morphological alternation of generations. Salensky thinks that a hopeful way of gaining a deeper insight into the phenomenon of true metagenesis lies in the further study of the cases presented by Tunicata. E. R. L.

The Structure and Development of Sponges.—The sponges are at present attracting a very large amount of attention from zoologists and are undergoing investigation in the fresh condition, so that their living soft tissues are subjected to the refined methods of modern histology. Prof. Franz Eilhard Schulze, of Gratz, is foremost in this study, the way in which was led by Ernst Haeckel in his monograph of the Calcispongiæ. Dr. Keller, of Zürich, who has previously published on the development of certain calcareous sponges, has now (*Zeitsch. wiss. Zoologie*, 1878, part 4) given attention to *Reniera semitubulosa*, O. Schm., a representative of the commoner marine fibrous sponges. Schulze, by the use of silver nitrate, discovered a differentiated epithelial covering to the body surface, which was previously denied by Keller, who now admits Schulze's observation to be correct, and adds a similar observation of his own on Reniera. Keller describes the syncytium of Reniera, denies the existence of muscular cells, and recognises certain "nutritive wander-cells" in the body-wall of the sponge. His observations on "starch-containing cells" are of special importance. He was led to attach a high functional importance to the nutritive wander-cells which pass inwards from the flagellate endoderm-cells, carrying with them assimilated matter necessary for the nutrition of the syncytium, which forms a thick wall beyond. His conception of their importance was confirmed by the discovery that many of them contain starch. Keller has made an extensive search for starch in the cell-elements of sponges, and has found it, or rather we should say has obtained the blue reaction with iodine, in cells from the following sponges:—(1) *Spongilla lacustris*, (2) *Reniera litoralis*, nov. spec., (3) *Myxilla fasciculata*, (4) *Geodia gigas*, (5) *Tethya lyncurium*, (6) *Suberites massa*, (7) *Suberites flavus*. The substance, whatever it may be, which gives the blue reaction, is not in a granular condition, but fluid, and in those cells in which it occurs occupies a large vacuole comparable to a fat vacuole. Neither ordinary nor absolute alcohol, nor

cold water, dissolve the contents of this vacuole. Keller could not find this starch-like substance in Halisarca nor Chondrosia, nor in any Calcispongiæ. It seems desirable in this connection to refer to the strictly granular condition in which chlorophyll appears in the case of Spongilla, the granules having the form of concavo-convex discs. In colourless (etiolated) specimens of Spongilla, the same granules are present of a little different form, and as in Neottia and other similar plants, these granules turn green (develop into chlorophyll?) on the addition of strong sulphuric acid (see *Quarterly Journal of Micros. Science*, 1874, vol. xiv. page 400, where I have recorded these facts, and also that of the occurrence of starch in Spongilla, though I have not yet been able to find the authority for the latter observation, which was made many years previously to Keller's investigation). With regard to the question of the formation of a gastrula in Sponges, and as to the development of the endoderm of that gastrula into the endoderm of the adult sponge, and therefore the continuity of the archenteric cavity of the gastrula with the digestive cavity and canals of the sponge, Keller has some remarks to offer which do not, in point of fact, amount to very much. Like Franz Eilhard Schulze, Keller fell into a complete error in his earlier publication on the development of Calcareous Sponges. Haeckel, in his monograph, stated that the sponge embryo was at first a hollow one-cell-layered sac, on the inner wall of which a second cell layer formed, by delamination, whilst subsequently a mouth broke through. This was vehemently denied and ridiculed by Metchnikoff; it was also denied by Oscar Schmidt, and by F. E. Schulze, who published a beautiful set of drawings showing that after the embryo sponge had acquired some thirty or forty cells, one hemisphere of cells became granular and enlarged, and then invaginated—sunk into the other hemisphere—thus forming a gastrula with endoderm and archenteron by invagination. This account was at first accepted as the true one, but it was strongly insisted upon by Keller in his former memoir, that the orifice of invagination closes up, as in fact the blastopore so usually does throughout the animal kingdom, and that the young sponge is then a mouthless closed sac with two layers of cells. It was in this condition that Haeckel saw it and described the further stage in which the true mouth breaks through. There is, however, still a great difficulty about the development of the gastrula of sponges; for no one can doubt, who will examine a common calcareous sponge, or who looks at Barrois' valuable memoir on the subject, that F. E. Schulze was—as he himself has admitted—so far misled in his account of the development of *Sycandra ræphanus* as to transpose two very important stages of the development. In fact, the concavo-convex stage of the embryo sponge, with one set of cells (endodermic) tucked into the narrower, clearer, longer, ciliate cells, actually *precedes* that in which the same cells form respectively a hemisphere of clear ciliate cells and a hemisphere of large swollen cells, not tucked into the former at all, but so arranged that a small central cavity is closed in by the two groups. How we pass from this stage to the young sponge, or even to the two-cell-layered sac, is still a complete mystery. One thing, however, is obvious. Haeckel could hardly have been led to the generalisation known as the gastræa theory, which, on the whole, is a truthful and productive generalisation, by erroneous observation. We must, therefore, respect his positive statements of fact.

E. R. L.

GEOGRAPHICAL NOTES

THE Swedish North-East Passage Expedition, under Prof. Nordenskjöld, was arranged to start from Tromsoe about the 15th inst. in the *Vega*, which sailed from Gothenburg about the beginning of the month. The *Vega* is commanded by Lieut. Palander, who was second in command of the *Sofia* in the polar expedition of 1868. Nordenskjöld's scientific staff consists of F. Kjellman, docent in the University of Upsala, who took part in the expeditions of 1872-3 and 1875; Dr. A. Stuxberg, who took part in the expeditions of 1875 and 1876; Dr. E. Almquist, medical officer; Andreas Hovgaard, lieutenant in the Danish navy, physicist; and Giacomo Bove, lieutenant in the Italian navy, hydrographer, the last-named officially sent by his Government. The *Vega* is provisioned for two years, but if the state of the ice be favourable, Nordenskjöld hopes to reach Behring's Straits by the end of September. The *Vega* will be accompanied as far as the mouth of the Lena by a new steamer, the *Lena*, which will ascend the river of the same name, on which it is intended to ply, as far as the town of Yakoutsk.

AFTER an absence of several months Dr. Georg Schweinfurth returned to Cairo on June 13 from his exploring journey through the Arabian desert. He reports having crossed some fifty valleys in the desert mountains, which he entered near Atfieh; eventually he reached Mount Gharib, and later on the Nile near Siut. Dr. Schweinfurth intends returning to Europe for some time to recruit his health, which has considerably suffered.

PROF. BASTIAN, of Berlin, who has explored more or less in nearly every region of the globe, is setting out again for four years' work in Asia, and especially in Further India, from whence he hopes to bring home many additions to his already large ethnological collection.

THE long-talked-of voyage round the world, under the auspices of the Société des Voyages d'Etude, is at last coming off under the leadership of Lieut. Biard. The *Junon*, the vessel in which the expedition sails, leaves Marseilles this week. We have on several occasions alluded to this and other similar projects, very tempting but much too expensive to attract many passengers. In the present case only twenty-five student-passengers of various nationalities have been obtained. The *Junon* has on board three professors who will lecture on natural history, geography, physics, and meteorology. The expedition has been well planned, and if the programme is carried out to even a moderate extent, the young voyagers ought not only to enjoy themselves, but return with much more knowledge, and perhaps wisdom, than when they set out. The *Junon* proceeds westwards to North and South America, the archipelagos of the Pacific, Australia, and New Zealand, China and Japan, India and Egypt, returning to the Mediterranean by the Suez Canal. A considerable part of the eleven months or so the expedition is expected to be away will be passed in the various countries at which the expedition will touch.

IN No. 3 of the *Deutsche geographische Blätter* of the Bremen Geographical Society Herr Camill Russ has a long and valuable paper on the present position of Abyssinia; the author has an intimate personal acquaintance with the country. Dr. Sandeberg gives a graphic and instructive account of a pilgrimage in Russia, in the summer of 1876, to Solowjetsk, lying between Lake Onega and the White Sea, and for centuries one of the most celebrated places of pilgrimage in Russia. Dr. Bretschneider, physician to the Russian Embassy in Pekin, sends a long letter, giving detailed instructions as to the best methods and seasons for travelling through Siberia and Mongolia to China, which will be found of great service to any enterprising tourist who may have time and money enough to spare for such an out-of-the-way journey. The letter conveys indirectly a good deal of information concerning Siberia and Mongolia.

IT is stated that the Lisbon Geographical Society reports favourably on a project for an exploring expedition in Portuguese Guinea.

THE GENESIS OF LIMBS [1]

II.

In those huge marine reptiles of the secondary period, the *Ichthyosaurus* and the *Plesiosaurus*, we meet with the most complete serial symmetry between the fore and the hind limbs. In the *Plesiosaurus*, not only are the bones of the hand and foot completely alike, but the same is the case with those of the upper arm and thigh and those of the fore-arm and leg.

In the *Ichthyosaurus* there is a similar resemblance between the fore and hind limbs, with the further curious similarity that in both hand and foot the very numerous digital bones are so disposed as to indicate that the number of the digits exceeded five.

When we descend from reptiles to batrachians we again find in the tailed forms a remarkable and exceptional serial homology in the bones and cartilages of the limbs. Thus, through the great descending series of forms from man down to reptiles and efts, we find that there is one fundamental type of limb. In all such creatures the limbs are never more than four, they are divisible into two pairs, which always possess complete *bilateral* and more or less clearly marked *serial* symmetry. This serial symmetry is generally more or less disguised owing to the different uses to which the two pairs of limbs are respectively put. Yet whatever such uses and needs may be, whether the limbs be formed simply for locomotion, as in the horse; for grasping, as in the ape; for flying, as in the bat and bird; or for swimming, as in the seal and the large extinct reptiles before referred to, we find the same limb segmentation running through all in both limbs.

1. An upper limb segment (upper arm and thigh).
2. A lower limb segment (fore-arm and leg).
3. A root part of the extremity (wrist and ankle).
4. A middle part of the extremity (mid-hand and mid-foot).
5. A terminal part of the extremity (the fingers and toes, *i.e.*, the digits).

In the great class of fishes an altogether different set of conditions obtains. The parts in fishes which answer to the fore limbs of the creatures hitherto considered, are the pectoral and the ventral fins. In them we find a number of delicate structures, "fin-rays," supported by bones or cartilages which have no obvious, even remote resemblance to the bones or cartilages of the limbs of batrachians, reptiles, birds, or beasts. The pectoral limbs are indeed attached to a shoulder girdle, but the ventral fins are appended only to bones or cartilages which lie amongst the muscles of the ventral surface of

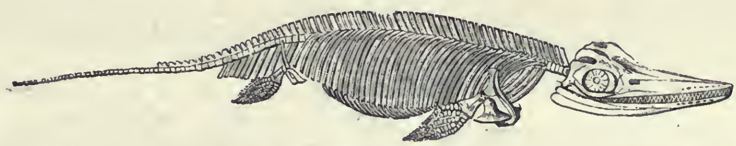


FIG. 12.—Skeleton of an Ichthyosaurus.

the body and hardly ever even approach the superincumbent vertebral column. In *Lophius*, indeed, amongst bony fishes, these supporting structures do ascend somewhat, and the same is the case in *Callorhynchus* and *Chimæra* amongst the cartilaginous fishes. Yet even here these pelvic structures are far from reaching the vertebral column.

In the fishes, which are in many respects most like the

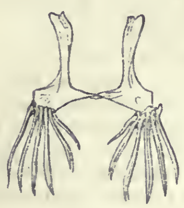


FIG. 14.

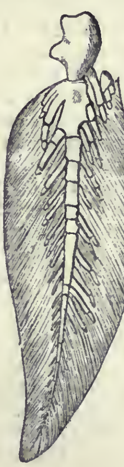


FIG. 15.

FIG. 14.—The two ossa innominata of the Angler-fish (*Lophius*), showing the ascending processes which simulate ilia. The fin-rays are attached to the outer-ventral margin of each os innominatum.

FIG. 15.—Cartilaginous skeleton of a limb of *Ceratodus*. (After Günther.) The large upper piece articulates with the limb root.

higher animals, *i.e.*, in the sharks and rays, the pectoral and ventral fins are not both formed in the same manner, but their structure differs considerably in different species.

The pectoral fin is supported by a number of cartilaginous rays which rest upon three basal cartilages lying nearest to the slender girdle and called respectively the *Propterygium*, the *Mesopterygium*, and the *Metapterygium* (see Fig. 17). The ventral fin is smaller, and consists of cartilaginous rays appended to a basal cartilage, which is suspended from the pelvic cartilage, to which latter some rays may also be directly attached.

In one very exceptional form, the ancient triassic fish still surviving in Queensland, *Ceratodus*, the limb-skeleton is *sui generis*. It consists of a median longitudinal series of cartilages, whence other smaller cartilages diverge on each side, the whole structure tapering to its distal end.

We see then, now, the answer to one of our initial questions, "What do our limbs stand for as compared with the bodies of other animals," and may proceed to another of those questions, namely, "Why are our limbs so much alike and yet so different?" We form one of a series of creatures with digit-bearing limbs (all vertebrates above fishes), all presenting similar resemblances and differences in varying degrees. The differences between our limbs are manifestly due to difference of function, but the resemblance may be due to one of two causes:—(1) The conservation of a complete serial symmetry present in the earliest vertebrate types, or (2) The action of internal polar force, tending to educe serial symmetry when such symmetry does not interfere with function. In other words, it may be due either (1) to genetic homogeny, or (2) to intra-organic homoplasy.

It appears to me impossible that it can be due to the former cause, for even if the first pairs of limbs ever formed were completely similar, which I by no means regard as certain, still the detailed resemblances found in animals high up in the scale cannot have continued uninterruptedly from such primitive forms. Even if we consider that the complete serial homology in the limbs of the tortoise, *Chelydra*, might be primitive, the limb of *Hyrax*, *Perodicticus*, and *Nycticebus* cannot owe their peculiar serial resemblances [1] to the survival of primeval conditions.

[1] Continued from p. 284.

[1] See "Genesis of Species," second edition, p. 201.

But the action of an internal polar force is evidenced by other phenomena of pathology and teratology, which come to the aid of comparative anatomy in demonstrating its existence. Thus we very often see that parts which are serially or bilaterally symmetrical, are abnormally affected in a similar manner,[1] as recorded by Sir James Paget, Dr. William Budd, Prof. Burt Wilder, and others, and the same is the case with congenital deformities.[2]

Perhaps, however, the most curious and instructive are those presented by some of our domestic birds. In birds we have found the serial symmetry of the limbs reduced almost to its minimum—the leg and foot being so widely different from the wing. In trumpeter pigeons and some bantams, however, the feet, which usually are naked, become abnormally furnished with feathers (technically called "boots"), which may be even longer than the wing feathers, and are developed from that side of the foot which corresponds with the feather-bearing side of the hand. Moreover, in ordinary pigeons, though the digits of the hand are completely united together, while the toes of the foot are free, but in "booted" pigeons the outer toes become more or less united together by skin like the fingers.

With facts such as these it seems to me unreasonable to deny the existence in each animal (which as a whole is a visible unity) of an innate polar force tending to carry out development in definite directions, but liable to have its action and effects modified by the environment.

We may now turn to those other initial questions, Whence have limbs such as ours arisen? What is a limb?

As to the first question: The digit-bearing limbs of man, beasts, birds, reptiles, and batrachians are usually supposed to have been derived from structures having more or less resemblance to the paired fins of fishes, but the path which the genetic process has followed is differently represented by different evolutionists.

As to the second question, *i.e.*, as to the essential nature of vertebrates' limbs, of whatsoever kind, different views have also been maintained. By some anatomists they have been regarded as parts which have in one way or another been derived from the axial skeleton, by others they have been represented as skeletal structures appended to, but not derived from the axial skeleton.

In 1843 Oken taught the extremely fanciful doctrine that arms and legs are so many liberated ribs, and Carus followed him to a certain extent, teaching that they are essentially elements radiating from the exterior of a rib-like arch.

In 1848 Owen propounded the view that they are diverging appendages attached to ribs like the uncinate processes of birds, and he compared them to the branchiostegal rays of fishes. He also taught that the shoulder and pelvic girdles are modified rib arches.

In 1852 Maclise represented the limbs as modified ribs, the parts beyond the elbow and the knee, however, corresponding with the interspinus bones and fin rays of fishes' azygos fins.

In 1857 Goodsir described them as radiating actinopophyses.[3]

In 1871 Humphrey represented the limbs as modified portions of a primitively continuous inferior azygos fins.

In 1872 Gegenbaur threw out the suggestions which he has since (1874 and 1876) more definitely adopted, that the shoulder girdle is a modified arch of similar nature to the branchial arches, the limbs having been formed from rays diverging outwards from such an arch. He also considers the skeleton of the azygos fins as the separated ends of the neural and hœmal spines of the vertebral column.[4]

Owen and Gegenbaur consider the paired and azygos limbs to be two fundamentally different structures, and Huxley[1] calls their sustaining bones or cartilages "elements of the exoskeleton."

The fundamental distinctness between the paired limbs and the axial system appears to have been held by Cuvier and by Huxley. I advocated the same view in 1870,[2] and I have since expressed[3] my conviction "that the appendicular skeleton is no mere portion of the axial skeleton, but a distinct system of parts appended to, and more or less closely and variously connected with, the axial system." To this conviction I now adhere more firmly than ever. I have also been long convinced that the shoulder-girdle could not be (as Gegenbaur thinks) a branchial arch, or be formed of coalesced branchial arches, as also that the branchial arches could not be (as some have supposed) serially homologous with rib-arches. For the branchial arches are within the aortic vessels, which vessels I took[4] to

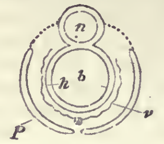


Fig. 16.—Diagram of the condition of the skeleton in the branchial region of some sharks (transverse vertical section).—*n*, neural canal; *b*, alimentary canal, surrounded by *h*, the branchial arches (splanchnapophyses); *v*, the aortic vessels, extending up outside the branchial arches and inside the paraxial system (*p*), here represented by certain external branchial cartilages.

"indicate the line of the pleuro-peritoneal division of the ventral laminæ" of the embryo. This conviction has been remarkably justified by Mr. Balfour's recent discovery of the continuation (in embryo elasmobranchs) of the pleuro-peritoneal cavity into the head, and externally to their aortic vessels.[5]

Prof. Parker has recently suggested[6] that the "extrabranchials" of the dogfish may be homologous with the scapulo-coracoid, but I am persuaded this is not the case.

In 1876 Mr. Balfour described[7] the development of the limbs of elasmobranchs as "special developments of a continuous ridge on each side, precisely like the ridges of epiblast which form the rudiments of the unpaired fins." Since then the paired fins arise in the same way as the azygos fins; they all probably belong to the same category of peripheral, non-axial structures. Moreover, since this is the mode of origin in the individual, there of course arises an *à priori* probability that it was the mode of origin in the race, and that the primeval vertebrate limbs were a pair of continuous lateral folds, serving to balance the body in swimming.

Now I conceive it will hardly be disputed that when supporting hard structures were first developed in the azygos fins they had the form they so generally still present, of a longitudinal series of numerous similar, separate, rod-like structures. But the skeleton of the paired, especially of the pectoral fins, is very different from this. The interesting questions, therefore, arise: (1) Whether any azygos fins present characters approximating them to the normally developed paired fins? and (2) Whether any paired fins present characters approximating them to the normally developed azygos fins?

A detailed account of some recent dissections of fish-fins made with a view to answer these questions—dissections effected through the kindness and liberality of Dr. Günther, at the British Museum—have been lately[8] communicated to the Zoological Society. The result of those dissections

[1] See "Genesis of Species," second edition, p. 205.
[2] *Loc. cit.*, p. 202.
[3] *Edinburgh New Phil. Journal*, vol. v. (new series), 1857, p. 178.
[4] "Grundriss d. Vergl. Anat.," 1874, p. 488.

[1] "Anat. of Vertebrates," p. 43.
[2] *Linn. Trans.*, vol. xxvii. p. 388.
[3] "Lessons in Elementary Anatomy," 1873, p. 230.
[4] *Linn. Trans.*, *l.c.*
[5] *Cambridge Journal*, vol. xi. part 3, April, 1877, p. 474. The author's words are:—"It occupies a position on the outer side of the aortic trunk of its arch."
[6] "Morphology of the Skull," p. 343.
[7] *Cambridge Journal*, vol. xl. part 1, p. 132.
[8] Vol. v., 1878.

is that I have found such varying degrees of coalescence between the cartilaginous rays of the dorsal fins as may go far to bridge over the differences between the two orders of fins, while the close resemblance sometimes presented by paired fins to azygos fins (some of the ventrals being so nearly like certain dorsal and anal fins), is such that I think a conclusion in favour of their essential similarity of nature cannot be successfully contested.

As to the dorsal fin, I have found incipient coalescence between the rays, in *Scyllian canicula*, *Ginglymostoma cirratum*, and others, but in *Notidanus cinereus* I have found this process carried to such a degree, that there comes to be one continuous basal cartilage to the dorsal margin of which the cartilaginous rays are appended.

In *Pristis* and *Pristiophorus* I found a very interesting condition which I am not aware has been described. The rays in these genera repose upon solid cartilages which are absolutely continuous with the subjacent axial skeleton. I would suggest that the lateral pressure of the saw-like rostrum must be aided in these fishes by such a firm attachment of the dorsal fin cartilages to the vertebral column. ST. GEORGE MIVART

(*To be continued*)

A HUNTING WASP

THE following interesting account of a chase between a wasp and a spider has been forwarded to us by Mr. Henry Cecil, who, it may be remembered, wrote to NATURE on the subject (vol. xvii. p. 381):—

The Piræus, Athens, June 19

DEAR SIR,—Your letter of April 5, and the two numbers of NATURE, reached this during my absence in Thessaly, which must be my apology for not having sooner replied to your letter.

Though more than thirty years have elapsed since the circumstance alluded to, I perfectly remember the curious chase I witnessed of a very large and powerful hunting-spider by a species of wasp.

I was sitting one summer's afternoon at an open window (my bed-room) looking into a garden, when I was surprised to observe a large and rare species of spider run across the window-sill in a crouching attitude. It struck me the spider was evidently alarmed or it would not have so fearlessly approached me. It hastened to conceal itself under the projecting edge of the window-sill inside the room, and had hardly done so when a very fine large hunting-wasp buzzed in at the open window and flew about the room evidently in search of something. Finding nothing the wasp returned to the open window and settled on the window-sill, running backwards and forwards as a dog does when looking or searching for a lost scent. It soon alighted on the track of the poor spider, and in a moment it discovered its hiding-place, darted down on it, and no doubt inflicted a wound with its sting. The spider rushed off again and this time took refuge under the bed, trying to conceal itself under the framework or planks which supported the mattress. The same scene occurred here, the wasp never appeared to follow the spider by sight, but ran backwards and forward in large circles like a hound. The moment the trail of the spider was found the wasp followed all the turns it had made till it came on it again. The poor spider was chased from hiding-place to hiding-place—out of the bedroom across a passage and into the middle of another large room, where it finally succumbed to the repeated stings inflicted by the wasp. Rolling itself up into a ball the wasp then took possession of its prey, and after ascertaining it could make no resistance, tucked it up under its *very long hind legs* just as a hawk or eagle carry off their quarry, and was flying off to its nest, when I interposed and secured both for my collection.

Both insects were rare ones, and during the ten years I collected as a field naturalist in Greece, I don't remember ever seeing more than three or four specimens of either that species of wasp or spider.

The wasp was a hunting one (a female) about an inch and-a-half long, a very finely formed insect, which for gracefulness of form and beauty of colouring is entitled to be placed at the head of its species. The legs of this kind of wasp are very long and of a dark chocolate brown. It runs very quickly. The wings are a light-brown with dark-brown tips and long and powerful, and the body beautifully mottled with pale yellow and brown. It has very long fine antennæ. It is not an English species, but probably exists in Spain, the south of France, and Italy.

The spider, too, was a rare one. One of the largest Greek hunting-spiders, nearly as large in the spread of its legs as the flesh-coloured tarantula though without his powerful crab-like pincers. The one I allude to must have covered at least three inches in circumference when its legs were fully extended. It was of a dull mottled brown colour on the upper surface of the body. Very difficult to distinguish from the ground. The lower part of its body was, however, brilliantly coloured, the long legs, or arms, being marked underneath with velvet-like looking black and white rings. The head, thorax, and abdomen were of a velvety black, the lower portion of the latter surrounded with a bright orange ring.

There is only one error in the account given by you in NATURE, that is, that you were under the impression I told you, that kind of spider was the common prey of that species of wasp. You must have misunderstood me.

1. I do not think that particular kind of spider is sufficiently common for this to be the case.

2. I never saw a similar conflict of the kind before or after, which as it was in a room, and not in the grass, where I presume such encounters usually take place, I observed under exceptionally favourable circumstances.

I am certain the spider left no web or thread behind it. I cannot be sure, however, that, as it had evidently been attacked by the wasp before entering my room, a small quantity of liquid may not have exuded from its wounds, which may have helped the wasp in tracking it. I have no doubt myself that insects have the sense of smell, and probably much more developed than our own. No one, as you remark, who has sugared for moths, or seen the large sphingidæ hovering over the strongest scented flower at night, or employed a caged female moth as a lure to her male admirers, can, I think, doubt this. If so, let them put a saucerful of honey in a corner of a room opening into a garden, throw open the window, and see how soon the bees, wasps, &c., will be attracted to the honey.

There is a tradition in the east that one of the tests by which the Queen of Sheba tried to prove the wisdom of Solomon, was placing on a table before him two bouquets, one of artificial, and the other of natural flowers, and requiring that he should say which were the real and which the artificial, without moving from his throne. Solomon ordered the windows to be thrown open, and in flew the bees, &c., which went at once to the real flowers.

Whether the senses of insects, birds, and what we call the lower creation, are similar to ours in every respect, it is very difficult to say. No doubt a dog, if he could speak, would say a man had not the sense of smell, and would prove that his nose was worse than useless to him. An eagle or hawk would say that men and moles, &c., have only the rudiments of eyes, and so on.

Man, with five very imperfectly developed senses (who can say that there are not twenty senses), is the only animal that is dogmatical, and denies all he cannot understand. The oracle of Delphi said "Socrates was the wisest man in Greece, *because he was the only man who knew he knew nothing*." Yours faithfully,

C. L. W. MERLIN

To Henry Cecil, Esq., Bournemouth.

A NEW CAMERA LUCIDA

THE various kinds of camera lucida hitherto used have always possessed many inconveniences, none of them allowing to be seen upon the paper with sufficient precision, and simultaneously, the image of the object and the point of the pencil. For the purpose

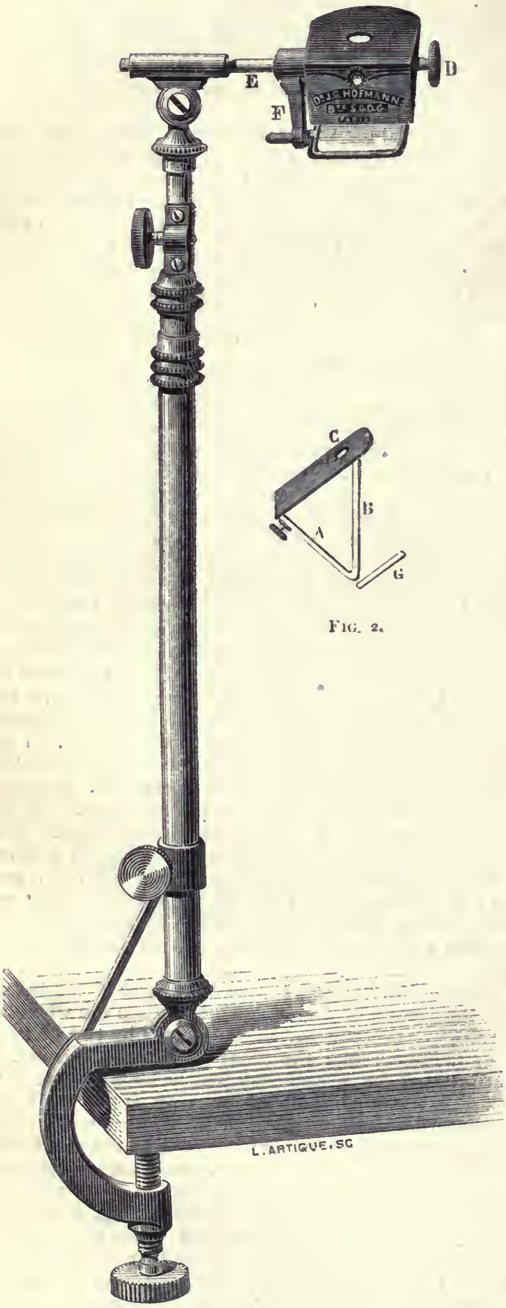


FIG. 2.

FIG. 1.

of remedying this inconvenience, Dr. J. G. Hofmann, of the Rue Bertrand, Paris, has had recourse to an arrangement by which he believes he has obtained the most satisfactory results. The illustration will give some idea of this arrangement.

Fig. 1 represents the general elevation, in half size, of Hofmann's camera lucida. Fig. 2 is a transverse section of the optical part, composed, at A, of a metallised mirror, or other metallic surface, polished and rigorously plane; at B, of a small plane mirror of parallel glass, forming, with the metallised mirror, a fixed angle. The function of the latter is to let pass a part of the luminous rays coming from the object to be drawn, and to show at the same time the point of the pencil alongside the image upon the paper. At G may be placed, in a movable frame, either a plate with parallel surfaces, or lenses of neutral glass of various foci, the principal object of which is to enable a satisfactory drawing to be made of the objects placed inside, when using white paper; for the outside, this glass serves to temper the brightness of the sun.

At C is the eye-hole or opening before which the eye is placed. The knob D serves to place the chamber in a convenient position, which sometimes depends on that of the artist with respect to the object; but generally it is convenient to place the mirror D vertically. With the same pieces of the optical part, with the addition of a concentrating lens, Dr. Hofmann has been able to construct a second model applicable to microscopes, for which, as well as for telescopes, all previous forms of camera have given only very mediocre results.

ANATOMICAL PREPARATIONS FOR MUSEUM AND CLASS PURPOSES

IN a former number of NATURE (vol. xvi. p. 360) I offered some suggestions on museum preparations and arrangement. These I can now supplement by a new method which I have tried with encouraging success.

No museum-curator needs to be reminded of the many defects of the ordinary fluid-preservatives. Evaporation, blanching, spilling, optical distortion, the cost and inconvenient shape of glass vessels—these are among the serious and apparently inevitable advantages of dilute spirit. I have found it possible to get rid of all these difficulties together by mounting dissections and entire animals in glycerine jelly.

The following directions may be followed until experience shall suggest better. Soak gelatine (best quality) in water until it has absorbed as much as it can, melt and add an equal bulk of best German glycerine. Clarify with white of egg, one egg to a pint of mixture, taking care to boil very steadily, without burning. Filter hot through flannel. The jelly should be transparent, and of a pale straw-colour. It should melt at 39° C., and have a specific gravity of 1·186 at 8° C., compared with water at the same temperature.

The jelly may be diluted with water, with glycerine, or with a mixture of the two. I find one part of jelly to one of glycerine and one of water a convenient proportion. The dilute jelly is apt to run fluid on exposure to the air, owing to the growth of moulds. This may be prevented by using a solution of salicylic acid or thymol in water for dilution. These substances cause opalescence in the medium, but a very minute quantity of acetic acid clears it again.

Lay out the dissection on wax, as recommended in my previous letter, but without pins, and fill up with jelly rendered fluid by gentle heat. When the vessel is full, allow the jelly to cool and set, then pour a little more on the top. After this also has set, lay the glass cover (warmed by immersion in hot water) in its place. As the superficial layer of jelly melts, press the cover down. When cold, cement the edges with strips of cloth smeared with coaguline.

The vessel for mounting may be of almost any size and shape. I have tried glass jars, built-up glass cells, ebonite, gutta percha, earthenware, and wood soaked in paraffin. The vessel should be strong and quite air-tight.

It is early as yet to speak of the final result. Some preparations have lasted five months without alteration.

The inert character of glycerine jelly, so well-known to microscopic mounters, justifies confidence in its stability. A large proportion of glycerine may render certain objects too transparent. This tendency may be corrected by changing the proportions as required, or by adding alum. I have found even delicate colours, such as those of squids, readily preserved by the jelly. No effusion of mucus or colouring matter takes place, and an animal may be mounted fresh if care be taken that the jelly penetrates sufficiently into cavities of the body. Previous immersion in alcohol, or other preservatives, does not prevent re-mounting in glycerine jelly. Many of the ordinary reagents used by the histologist may be added to obtain special results. I have not as yet succeeded with large objects, but mountings with as much as a pint of jelly have done well.

The cost of the jelly is not prohibitive, and when the freedom from loss by evaporation, or spoiling by turbidity and discoloration is considered, this mode of preparation will be found cheapest in the end. Harvey and Reynolds, of Leeds, undertake to supply the undilute jelly at a moderate price.

Until experience suggests improvements I have nothing to add. The preparations ought to be kept for years before the new process can be recommended in unqualified terms. I think, nevertheless, that I have already seen enough to warrant the anticipation that mounting in jelly will for certain purposes displace all the fluid methods in use. L. C. MIALL

Leeds Museum

BEES

AN American correspondent writes asking Mr. A. R. Wallace, through NATURE, his opinion as to the genus *Apis*. Are *dorsata, zonata, indica, adansoni, nigro-cincta,* and *florea*, each or all distinct species? or, our correspondent asks, are some of these like *ligustica* and *fasciata*, simply varieties of *mellifica?* Also as to structure and habits of *A. dorsata* and others, which Mr. Wallace has personally seen and handled.

The following reply has been sent us to these queries:—

Mr. Alfred R. Wallace having suggested that I should answer the queries of your American correspondent, I do so at once, having in the year 1865 published in the *Annals and Magazine of Natural History*, a somewhat elaborate paper on the subject on which information is sought for. The species that in my opinion are distinct are *Apis mellifica, A. adansoni, A. dorsata, A. zonata, A. unicolor, A. indica*, and *A. florea*. I do not consider the examination of worker bees only sufficient material to enable any one to form a decisive opinion as to species; the examination of drones, also, I consider indispensable; it is advantageous to see queens, but those which I have seen do not present any very marked peculiarities indicative of specific distinction. I possess males and workers of *A. dorsata, A. indica*, and *A. florea*. That *A. ligustica* and *A. fasciata* are climatal varieties of *A. mellifica* has been apparently proved by the fact of their having in England reverted to the original stock, *A. mellifica;* there is, however, a remarkable fact to be noticed that, notwithstanding the change referred to, they still possess a much greater degree of irascibility than *A. mellifica; A. fasciata* undoubtedly in the greatest degree. I consider *A. zonata* distinct from *A. dorsata*, its nearest ally; it is a larger bee, jet-black, with snow-white bands on the abdomen; I have not seen *A. dorsata* from Celebes, where *A. zonata* was discovered by Mr. Wallace, but he found that species in Sumatra, Flores, Timor, and Gilolo. *A. adansoni*, and *A. nigro-cincta*, will probably prove to be climatal varieties of one species, the latter being a pale form with dark bands. There is no doubt of *A. indica* being a distinct species, all the sexes are known, and there is no other species found in India with which it could be assimilated. Of the specific distinction of *A. florea*, the remarkable structural formation found in the drone, that of a lobe on the metatarsus, is conclusive; it is also much the smallest species known of the genus Apis. *A. unicolor* inhabits Madagascar, Mauritius, and the Island of Rodriguez; a considerable portion of a swarm was obtained from the latter island, an examination of which inclines me to consider the insect much more than a climatal variety of any other species; it remains that the drones and queens should be obtained in order to decide the question; until this can be effected I shall consider *A. unicolor* a good species.

Of the habits of the species of the genus Apis, Mr. Wallace, Sir John Hearsey, Dr. Jerdon, and Mr. Chas. Horne have given some interesting particulars. *A. dorsata* suspends its mass of combs on the branches of trees, quite exposed, having no covering whatever; Sir John Hearsey succeeded in obtaining a swarm which he secured in a box-hive, thus domesticating the species, and obtaining from time to time quantities of delicious honey. Dr. Jerdon gave me combs of *A. indica*, which had taken up its abode in the rafters of an outhouse. Mr. Horne gave me the comb of *A. florea;* it is attached to a twig of some bushy plant. Dr. Welwitsch brought combs of *A. adansoni* from Angola; they were found inside a hollow tree; the cells are considerably smaller than those of any of the honey-bees of Europe. FREDERICK SMITH

British Museum

THE ORION NEBULA

A SHORT time ago we gave an abstract of d'Arrest's "spectroscopical researches." The Danish paper contains also the conclusions at which he arrived after many years contemplation of the nebula in the sword-handle of Orion. The spectrum is now easily visible, with open slit, even without a telescope. Then we see three images of the nebula corresponding to the three lines, whose relative intensity d'Arrest found to be 100, 24, and 71. To see the fourth line is of course very difficult. If the spectrum of the stars is looked at together with that of the nebula, we find the nebular lines continue absolutely unimpaired through the inner trapeze. Consequently it cannot be considered as proved that the stars are in connection with the nebula. It has not, of course, yet been possible to ascertain spectroscopically whether the stars are nearer to us than the nebula, or farther away in space. The question of resolvability has lost a good deal in interest since Huggins showed its gaseous nature. However, d'Arrest would not believe that it had ever been resolved into stars in any of the large telescopes of his day. All the more startling was the Rev. Dr. Robinson's letter (NATURE, vol. xv. p. 292), that he as early as 1848 had resolved this nebula with the Earl of Rosse's telescopes. It would be worth while for Mr. Ellery, who, according to our astronomical column, is investigating the southern nebulæ, to ascertain whether actual resolvability is referred to here, or the circumstance that, as might be expected in so enormous a reflector, a good many small stars become visible by glimpses. Liapounov describes the appearance of Regio Hugeniana as follows: "Ces masses m'avaient présenté à plusieurs occasions des ressemblances frappantes avec des amas d'étoiles. Le caractère stellaire s'est prononcé d'abord dans la masse la plus lumineuse, dont l'apparence me conduisait depuis constamment à l'idée d'une agglomération de petites étoiles condensées." We are hardly right in concluding that the nebula could be resolved in the nine-inch refractor of the Cazan observatory.

The Orion nebula was first pictured together with the four stars of the trapeze by Huyghens, who discovered it in 1656, though Cysat referred to it already in 1618. It was afterwards examined by Derham, Godin, Fouchy, Mairan

and Picard. Legentil compared its outline to that of the open mouth of an animal. Messier was the first who gave a catalogue of the stars seen in the nebula. Schröter, in Lilienthal observed it, 1794-1799. It was this eminent astronomer who discovered that this chaotic mass is not in perfect equilibrium, and several of the changes he pointed out have been verified by modern observers. Sir W. Herschel watched the nebula during thirty-seven years. He believed that changes were taking place in Nebula Mairani. The most important fact connected with the discovery of changes was, that the three conspicuous stars ϵ, ζ, and η (J. Herschel, 1825), in most of these old maps, are represented as inside the bright nebulosity, while they are now seen far from it. D'Arrest showed that no changes have occurred here by aid of a drawing, which Lefebvre published, 1783, in Roziers, "Observations de la Physique." He also remarked the characteristic circumstance that in this figure Sinus Magnus is represented as running right across the trapeze, which, in consequence, is lying altogether outside the nebula. Lefebvre's drawing is, however, executed in the style of those that preceded Messier. It is of uniform brightness and sharp outlines.

Sir J. Herschel appears to have been the first who understood that in order to ascertain changes, it was required to give faithful drawings of all the minute parts of the nebula. His first drawing was executed in 1824, and that was, in 1847, followed by the beautiful figure founded on micrometric measurements made at the Cape, 1834-1837. He attributes hardly any weight to the first drawing, which had been made with freehand, compared with the last one.

Lamont published, 1837, an image of the brightest part of the nebula which Herschel criticised. He found, for instance, Regio Hugeniana more uniform, and marked with certain channels, while Lamont represented it as consisting of rounded masses running into each other. Later authorities agree with Herschel, but it deserves to be remarked that he had not himself, 1824, remarked these channels, nor are they laid down on Cooper's map. It so happens that the refractor in Copenhagen is exactly similar to that in Munich, and in consequence a comparison of the respective drawings made at an interval of thirty-five years could not but be of importance. There is no trace of the sharp outline in the north-west corner, which the Danish drawing shows, and it is so much more likely that here great alterations in brightness have taken place, as all the old drawings, for instance Cooper's, support Lamont, while the later ones in this respect agree among themselves. Amongst the most remarkable differences d'Arrest classed Pons Schröteri in Sinus Magnus. Lamont has of this bridge only the small piece, which, like a promontory, is attached to the north side, while d'Arrest saw the brightest patches about midway. On the above-mentioned old drawing by Cooper, Pons Schröteri is only represented as three small pieces emanating from the north side, while the same is now in the large refractor of the Markree Observatory only noticed as a little spot in the middle of the bay. Such changes were already alluded to by Schröter, and modern diagrams support this hypothesis.

Liapounov's diagram, drawn after most careful micrometric measures,[1] represents the object as seen about 1850. He agrees with Lamont about Regio Hugeniana, and also about the east point, which he found well defined against the far fainter Proboscis Major. He observed Sinus Lamontii, which he surrounded by the bright nebulosity, since called Hemicyclium Liapounovii. The darkness of this Sinus varied considerably, and thus it was explained why it was not noted by Herschel, though indicated on Cooper's map. Liapounov represented Pons Schröteri as emanating from the north side of Sinus Magnus, but he made it end with a bright spot, and his representation is, therefore, the midway between older and later drawings. Few astronomers have conducted similar researches so earnestly and faithfully as the Russian professor, and his merits have not been so highly appreciated as they deserve. It appears to me that this is even the case from the side of his Danish colleague.

Lassell published in 1854 a steel engraving, which was badly executed, the regions round Sinus Magnus in particular. Nebula Mairani was made brightest of all the nebulæ, while it only holds the third or fourth place. All these drawbacks have, however, been removed from the second drawing made in Malta, 1862 and 1863, which is one of the best extant.

The drawing Secchi published in 1868 is not to be trusted, and even the central region is wrongly drawn. D'Arrest had made a similar remark about an earlier figure by Secchi, to which the Papal astronomer answered: "Che la figura litografica pubblicata, benchè esatta in generale, ha alcune inesattezze non trascurabili." The possibility of a similar explanation in the present case was excluded by the remark: "Cosi siamo sicuri che l'incisione rappresenta la nebulosa come vedesi da noi nel nostro strumento."

George Bond's drawing, of about 1860, is in d'Arrest's opinion, more like the nebula than any that has been drawn from a refractor, and the characteristic calmness over the whole has been successfully imitated. He only saw the northern boundary, and the parts about Palus Bondii somewhat different from Bond. The divisions in the south-east corner of the nebula, so prominent in the drawings made of late with gigantic telescopes, do not appear so distinct in d'Arrest's refractor as in Bond's. In Markree it is not possible to trace them at all. On this point Rosse's drawing contains more particulars than any that I have seen.

The most complicated drawing of the nebula was published in 1868, by the present Earl of Rosse. D'Arrest found this drawing to be very accurate. The dark channels in Regio Hugeniana are, however, rather broad, and two large spots north of the bright mass too prominent, the boundaries are generally considered too sharp, and the contrast between the stronger and feebler parties rather strong by those accustomed to other telescopes; but it does not appear that the limits to which nebulosity was traced are much farther than in the refractors of Cambridge, United States, and Copenhagen. The feeble streams of nebulosity which connect the θ with the southern ι nebula have been well studied at Birr Castle, while the faint northern branches were more attended to in Cambridge, United States, where Bond first traced the connection with the c nebula. The connection between θ and ι was known to d'Arrest since autumn, 1865.

On Rosse's drawing the east point of the main part is bent somewhat and does not go smoothly over in Proboscis Major. Thus far this agrees with d'Arrest, but the image at present seen in the Markree refractor is more like Bond's figure. D'Arrest evidently gives the almost straight south-eastern outline of Regio Hugeniana too great concavity. Rosse, d'Arrest, and Holden agree well about the part west of Sinus Gentilii. Hereabout the Roman drawing does not correspond to nature at all. Nor are the diagrams of Liapounov and Cooper in accordance with d'Arrest. Now this might arise from the different quality of their telescopes, but it is not unlikely that some change has taken place here, though d'Arrest does not offer this explanation. But he declares Sinus Lamontii and Hemicyclium Liapounovii to be very changeable. The agreement of the different diagrams of Lacus Lassellii is striking; it was remarked already in 1795, by Schröter, and notwithstanding possible fluctuations in brightness, no alteration in the form has taken

[1] From a discussion of his own and W. Struve's observations, Liapounov concluded that three stars of the trapeze were moving with respect to the fourth, the most southern star. An investigation, on the whole confirmative of this, was read by Prof. Nobile, last year, before the Reale Accademia of Naples.

place during the last eighty years. On the whole this constancy in the form d'Arrest considers the principal result of all our studies of this object. The changes, which have been remarked, seem all reduced to mere variations in intensity, but such small alterations may greatly change the impression we get on looking at certain parts of the nebula.

The nebula has of late been well watched at the United States Naval Observatory. Prof. Holden has been hitherto engaged in making micrometric measurements of prominent parts of the nebula and noting the order of brightness of the various masses. He will even attempt a little photometry with the 26-in. refractor. The stars suspected to be variable by O. Struve, are nightly observed. From a provisory discussion of the observations, Holden alludes to changes of short period, and a preliminary sketch of the central part shows that his discoveries in nebular astronomy are likely to rank with those of Newcomb and Hall in other parts of the science. W. D.

AMERICAN GEOLOGICAL SURVEYS

NORTH-WESTERN WYOMING AND YELLOWSTONE NATIONAL PARK [1]

IN a former number of NATURE (vol. xii. p. 265) some account was given of the various independent surveys in progress among the western territories of the United States. Allusion was then made to the unfortunate want of concert among them which had led to a reduplication of the work, and consequently to a struggle at Washington between the different surveying staffs, one fighting for a continuance of power, another for very existence. By the decision adopted by Congress the Engineer Department retained control only of those surveys which might be required for military purposes, while the geographical, geological, and other surveys, carried on for the purpose of exploring new ground and making its features and productions known were to be taken charge of by the Department of the Interior. Such a limitation ought to be sufficient to prevent any future risk of the same tract of country being surveyed twice by different and independent officers. That it was needed became abundantly evident during the time of the contest which was finally settled by Congress. And the present volume furnishes fresh proof of its necessity.

Early in the year 1873 the Engineer Department organised a surveying party to make a military reconnaissance of the north-west of Wyoming territory lying between the Union Pacific Railroad and the line of the Northern Pacific Railroad in Montana. As this department had all along been in the habit of employing civilian geologists, naturalists, botanists, and other scientific observers, Captain Jones, who took command of the expedition, collected a party of nineteen persons, exclusive of a military escort under four officers. This military character which the engineers have given to their reconnaissances, though, perhaps, hardly avoidable, seems with good reason to have been regarded as irritating to the Indians. During the investigation into the question of reduplication of surveys, it was stated by the geologists of the Department of the Interior that they did not wish any escort of soldiers as they were never molested by the Indians, who would have been suspicious of their movements had soldiers accompanied them. Captain Jones, indeed, refers to a large war-party of Sioux Indians which came into Big Horn Valley shortly after he and his expedition had passed out of it, and he seems to think that he made a lucky escape. But the appearance of so large a body of armed men as he commanded within the lands reserved by treaty to the Indians could hardly fail to awaken their distrust and set them in motion.

[1] Report upon the Reconnaissance of North-Western Wyoming, including Yellowstone National Park, made in the summer of 1873, by W. A. Jones, Capt. U.S. Engineers, with Geological Report by Prof. T. B. Comstock. (Washington: Government Printing Office.)

The country passed over in the route lay across the formidable range of rugged snow-capped mountains which rise round the head-waters of the Yellowstone. By some travellers this lofty barrier had been pronounced to be inaccessible, one picturesque observer declaring that "a bird cannot fly over that without taking a supply of grub along." Once across the watershed the expedition descended upon the basin of the Yellowstone, which had already become famous for its wonderful hot springs, and had been pretty fully described and carefully mapped. Indeed when one remembers how much had already been done in the scientific exploration of North-western Wyoming, one is tempted to ask whether the elaborate preparations made by Capt. Jones were really needed. Nearly a half of the geological part of the Report is occupied with a description and discussion of the geyser phenomena of the National Park—a very interesting and important subject, but one which had already been largely treated of, and which does not appear to be quite in its proper place in the midst of a military reconnaissance. Dr. Hayden, who had done so much to make known the structure and the wonders of that region, is cited in the report, but not in such a way as to suggest any adequate notion of the relative importance of his labours and those of Capt. Jones's expedition. The most important geographical point established by the latter traveller was the existence of an easily traversible pass through the mountains between the head of Wind River and the sources of the Yellowstone. He named it Togwotee Pass, and found that though it reaches an elevation of 9,621 feet above the sea, the slopes leading to it are so gentle that a railway might be led through it at a reasonable cost.

Prof. Comstock, who was attached as geologist to the expedition, contributes a series of geological chapters to the Report. They are well written, and show him to be not only a good observer, but one who endeavours to group what he sees round some leading principles in science. In particular he adopts a systematic method of treatment in preference to the order of observation usually followed in such reports. This plan saves his readers at a distance much time and trouble, besides enabling them to grasp the main outlines of his work far more clearly than would be otherwise possible. He begins by giving a general outline of the physical geography of the region, connecting the area examined by the party with the rest of the Rocky Mountain tracts as far as explored by other observers. Availing himself of the previous labours of Hayden, Clarence King, Whitney, and others, he arranges his narrative of the geological history of the region in stratigraphical order, beginning with the most ancient metamorphic or archæan rocks, and leading his readers through the Silurian, Carboniferous, Triassic, Jurassic, Cretaceous, Tertiary, and Post-tertiary systems. In seven interesting chapters Prof. Comstock discusses the questions in dynamical geology suggested by the work of the expedition. In pointing out the evidences for glacial action in North-western Wyoming, he admits that even the hardest rocks fail to show traces of glacier-striation; that in all his journey he had only seen two or three faint scratches approaching the nature of a glacial mark, but which might have been made quite recently. He found, however, on the Wind River plateau long and high ridges composed of huge granite boulders and immense blocks of Silurian and other rocks, with intervening lakes or ponds, and he no doubt correctly regards these features as glacier-moraines. He finds evidence of enormous erosion in recent geological times, and points out the causes now at work in producing rapid disintegration and removal of rock. Among these he mentions the great altitude of the region allowing of the accumulation of large masses of snow, and of the alternate freezing and thawing of the snow by night and day; the steepness of the slopes favouring rapid erosion, and the character of the rocks powerfully influencing alike the amount of

waste and the nature of the resultant forms of surface. The wind plays a not unimportant part in modifying the scenery partly by transporting vast clouds of sand away from the mountains and forming sand-hills in the plains, partly by felling large quantities of timber which obstruct the flow of surface-water, dam up streams, and render the country for wide distances all but impenetrable. The action of vegetation in preserving the surface of loose soil from disintegration, and in giving rise to mould, turf, and other accumulations is illustrated by examples met with on the journey. The author enters with considerable minuteness into the dynamics of the Yellowstone geyser region. He carefully describes eighteen groups of thermal springs, and distinguishes these somewhat arbitrarily, as he admits, from the geysers or eruptive springs, of which he enumerates twenty-five. In two concluding chapters he gives some account of the archæology of the region and of the manners and customs of the Eastern Shoshone Indians, from jottings made by him in intervals of leisure during the march. The Report is illustrated by forty-nine small sketch-maps of each day's march, two large general maps of the region traversed (one coloured geologically), and numerous sketches and sections. As a record of three months of daily toil in a wild little-known region the volume is creditable to its authors, and as a source of information regarding one of the most interesting regions of North America it will be useful to geological and general readers.

ARCH. GEIKIE

NOTES

THE programme of the fifty-first meeting of the German Naturalists and Physicians (the German equivalent of the British Association) will be held this year at Cassel, from September 18 to 24. This, probably the most thoroughly scientific and efficient of all the Associations, consists of twenty-five sections, ranging from Mathematics and Astronomy to Veterinary Surgery. This year a number of addresses on leading topics by eminent men of science are promised. Among these are the following:—"On the Relation of Darwinism to Social Democracy," by Prof. Oscar Schmidt, of Strassburg; "On Symbiosis, Parasitism, and Allied Phenomena of Life," by Prof. De Bary, of Strassburg; "On the Education of the Physician," by Prof. Fick, of Würzburg; "On the Physician in his Relation to Research and Natural Science," by Prof. Hüter, of Greifswald; "On Harvey's Life and Work," by Dr. Baas, of Worms; "On the Colour-sense and Colour-blindness," by Dr. J. Stilling, of Cassel. Many other attractions are promised, including excursions, social gatherings, and the inevitable winding-up "Abschieds-Commers." The various German railways will afford great privileges to those attending the meeting.

WE have received a circular issued by the local committee of the American Association, which meets at St. Louis on August 21, giving detailed directions as to how to reach the place of meeting from different points. From this circular we learn that the railway companies, proprietors of Pullman and other luxurious cars, various express companies, and the local hotel-keepers, afford unusual facilities to members at greatly reduced rates. The concluding excursion of the meeting is to be to the Rocky Mountain region of Colorado, the details of which have not yet, however, been arranged.

THE thirty-fifth annual congress of the British Archæological Association will be held at Wisbeach, from August 19 to 27, under the presidency of Lord Hardwicke.

WE are sure that all our readers will be pleased to hear that a Civil List Pension of 200*l*. per annum has been granted to Dr. Prescott Joule.

THE Royal Society of Sciences at Upsala have shown their appreciation of Mr. Alex. Buchan's work as a meteorologist by electing him a foreign member of their body.

MR. P. S. ABRAHAM, M.A., B.Sc., of St. Bartholomew's Hospital, who recently catalogued the Nudibranchiate Mollusca at the British Museum, has been engaged to arrange scientifically, and to write a descriptive catalogue of the natural history collections at the Winchester Town Museum.

LAST week we spoke of the generosity of the United States Government in the distribution of the publication of their admirable surveys. We regret to see, from a speech in the House of Representatives by the Hon. O. R. Singleton, that the usefulness of Dr. Hayden's surveys threatens to be seriously crippled from want of funds. The appropriation for this survey in 1867 was only 5,000 dollars, which in 1873 had been raised to 95,000 dollars. In 1876, however, this was reduced by 30,000 dollars, and again, in 1877, by 20,000, leaving the appropriation at only 45,000 dollars. The largest sum is what is actually needed that the survey may be carried on with efficiency, and to reduce it is quite unworthy of a nation so advanced and liberal as the United States, and is really the worst possible economy. The additions which have been made to science by Dr. Hayden's survey have been immense and of the highest importance, and its economic value to the country can be no less great. The mere list of the many admirable publications of the survey is sufficient to prove that the money has been well spent; and we trust the United States Government and Congress will be able to rise above all party feeling, and prove to the world that they have the best interests of the country and the interests of scientific knowledge at heart by restoring the appropriation to at least its old amount. Mr. Singleton truly says not a small item in favour of these surveys is the check they place on mining and land swindles.

WE have received the first number of the *American Journal of Mathematics*, to which we have already referred on more than one occasion. It is a large quarto of 104 pages, the chief editor being Prof. Sylvester. Its contents will bear comparison with those of any similar publication on this side of the water. We can only give a list of the papers in this number: "Note on a Class of Transformations which Surfaces may undergo in Space of more than Three Dimensions," by Prof. Simon Newcomb; "Researches in the Lunar Theory," by G. W. Hill; "The Theorem of Three Moments," by Dr. H. T. Eddy; "Solution of the Irreducible Case," by Guido Weichold, of Zittau, Saxony; "Desiderata and Suggestions," by Prof. Cayley—"No. 1. The Theory of Groups;" "Note on the Theory of Electric Absorption," by H. A. Rowland; a review, by Mr. C. S. Peirce, of Lieut.-Col. Ferrero's "Espozione del Metodo dei Minimi Quadrati;" "On an Application of the New Atomic Theory to the Graphical Representation of the Invariants and Covariants of Binary Quantics," by Prof. Sylvester. The first announcement of Prof. Sylvester's remarkable application of the chemical theory was made in NATURE (vol. xvii. p. 284). The London publishers of the journal are Trübner and Co.

ON the 21st will be opened the new magnetic observatory at Pavlovsk in connection with the Central Physical Observatory of St. Petersburg. The new observatory covers about eight hectares of surface, and the situation is in all respects favourable. The establishment comprises three principal scientific buildings, the main building of stone and surmounted by a tower for meteorological observations; a double-arched structure in stone covered with earth for observations in magnetic variation; and a wooden pavilion, without a particle of iron, for absolute magnetic measurements and for determinations of time. Besides these three buildings devoted to the purely scientific work of the

observatory, there are four wooden houses for lodging the staff, servants, and for other purposes. All these structures are roofed with bituminized paper, and we need scarcely say that the scientific buildings are at a sufficient distance from the other buildings to prevent the scientific observations being affected by their neighbourhood. The Pavlovsk Observatory is furnished with the most improved scientific instruments, and like the Central Physical Observatory of St. Petersburg, is a model establishment of its kind. Every precaution has been taken, both during the building and after its completion, to prevent a trace of iron getting near it. The instruments themselves have been put in their places under the personal superintendence of Dr. Wild, the head of the Central Observatory at St. Petersburg. Provisionally the work of the establishment at Pavlovsk will be specially directed to the observation of the meteorological elements and of terrestrial magnetism. As soon as trustworthy methods have been found for the constant measurements of other elements, such as atmospheric electricity, terrestrial currents, radiation of heat, the optics and chemistry of the sun and sky, these elements will also form the objects of normal observations.

MR. MURRAY has the following books in the press:—"Researches and Adventures among the Lakes and Mountains of Eastern Africa," from the journals of the late Capt. F. Elton, H.B.M.'s Consul in Mozambique; this work will include notes on the suppression of the slave trade, and will be edited, with additions, by the author's companion, H. B. Cotterill; "Sketches of the Natives of Burmah," an account of their manners, customs, and religion, by Capt. C. J. F. S. Forbes, the officiating Deputy Commissioner of British Burmah; the fifth division of Dr. Percy's "Metallurgy," which will treat of silver; and a third revised edition of Mr. E. B. Tylor's "Researches into the Early History of Mankind." Mr. Murray will also publish in autumn the life of another Scottish naturalist, by Dr. Smiles. This newly discovered prodigy, a baker, whose name was Dick, has been dead ten years, and is said to have been an even more remarkable man than Thomas Edward. The principal sphere of his geological and botanical labours was in the region of the Pentland Firth and Dunnet Bay, on the north-west of Scotland.

THE problem of technical education, of which so much has been said of late, has long occupied the attention of thoughtful men in France and Germany. In the former of these countries the question has received much more attention than in the years preceding the war of 1870. In a recent conference at the Trocadéro Palace, M. Corbon, who has laboured in this direction for forty years, urged the introduction of manual employments and of the practical teaching of the skilled industries into the higher schools. He spoke of the good results which had been found to follow the establishment of the municipal School of Apprenticeship in Paris. Although founded only a few years ago, their system of teaching has attained a high degree of development; the mechanical trades being particularly well taught and the pupils of the school being in great demand by masters. Examples of the work of pupils of this excellent institution are shown at the Paris Exhibition in the building known as the "Ville de Paris." Visitors to Paris interested in scientific and technical education should not fail to note the collection of objects there shown. They would also find the school well worthy of a visit. It is situated at No. 60, Boulevard de la Villette.

IT is stated that the Jablochkoff electric lights now so brilliantly employed in the Avenue de l'Opera of Paris are costing 20*l*. per night, but the four-and-thirty lamps that illuminate that street give much more light than is necessary for the purposes of street-lighting. The problem of electric lighting is evidently one in which the scientific workers across the channel are deeply interested. It is stated that no less than eighteen different kinds of regulators for the electric light are exhibited at the Paris exposition; not including the "candle" of M. Paul Jablochkoff.

M. MAUMENÉ recently communicated to the Société de Physique, of Paris, a discovery of some importance in thermochemistry. Concentrated sulphuric acid which has been left for some months undergoes a change of condition of a singular nature. On mixing a liquid such as olive oil, with, say, one-tenth of its weight of fresh concentrated acid, a certain constant rise of temperature is observed, but if acid three months old is used, the rise of temperature so obtained has a value of about 8° C. less. The same results occur even if the acid has been hermetically sealed in glass tubes. With water and other liquids analogous results are found. It is evident that some of the most important data of the thermal effects of chemical action may require revising in the light of this discovery.

THOUGH it is very difficult to obtain any details about their jealously-guarded country, a Japan contemporary gathers that the condition of the Coreans is just now miserable in the extreme. The spring crops of this year, it is said, will utterly fail, and the stock of food in the country is reported to be a mere nothing for the four or five millions of people, who must, if they can, struggle on in the hope of a possible autumnal harvest. The cause of the Corean famines is not known, but it is probable that the primary cause there, as in China, is disafforestation, for, although the forests of pines, oaks, &c., on the sea-board are carefully preserved, a great drain must have thinned the woods by the river-sides. Much or most of the wood used in Peking for building houses, temples, and palaces is said to come from Corea, and from the same source are obtained the vast supplies needed for Tientsin and the cities of the province of Chihli, which lie on the Pei-ho, Peitang-ho, and the Grand Canal. Corea produces various woods of the finest quality, and the cart shafts, dray poles, and axle-trees in Northern China are made out of the tough and strong Corean ash, elm, hornbeam, and other hard timber. We think, however, that our contemporary is in error in stating, without qualification, that "the great wooden masts which support the noble temples and gatehouses of the Imperial city of Peking (all enormous, beautiful, and enduring spars) come from Corea," for there is no doubt that most of the magnificent wooden pillars to be found in the halls of the Ming tombs and the Peking palaces and temples came from the Chaotung department and other parts of the Yünnan province. The timber in question is called by the Chinese *nan-mu*, and is to be seen in the places mentioned at the present day in perfect condition after the lapse of nearly three hundred years. It may not be uninteresting to add that it is not teak, as is often supposed by foreigners, and that the tree is tall, thin, straight-growing, having no bough or twigs on the stem, but suddenly shooting out branches at the top somewhat like a canopy over a maypole, and its bark is of a peculiar ashy grey colour. This is the account given of it by Mr. Consul Davenport in his Report on the trade capabilities of the country traversed by the Yünnan Mission in 1875-76, who also observed in the Manwyne valley, in the Kakhyen hills, and again in Lower Burmah, in places comparatively accessible, many trees bearing so striking a resemblance to the valuable *nan-mu* that the Indian Government have been recently instituting inquiries into the subject with a view to the development of the timber trade in British Burmah.

WE have received the first part of a new "Anatomisch-physiologischen Atlas der Botanik," by Dr. Arnold Dodel-Port, of Zurich, and his wife, published by Schreiber, of Esslingen. The atlas will be published in two forms containing forty-two and sixty plates respectively, to suit different classes of schools. It

is the finest publication of the kind we have seen, and the plates are of such a size that they may be hung up on the wall. The plants and various parts of plants in the part sent us are magnified from 15 to 8,000 times, and are most beautifully and successfully coloured according to nature. Explanatory text accompanies each plate, and as an aid to botanical teaching it would be difficult to imagine anything more useful and attractive; it would be a boon to teachers and students of botany to have the Atlas published in this country.

THE Harvard Library *Bulletin* No. 8, the *Nation* states, announces that a sufficient subscription to Scudder's "Catalogue of Scientific Serials" has been secured, and that the work will be immediately put to press.

THE eleventh annual report of the Peabody Institute of Baltimore shows that the institution is efficiently serving the various scientific, literary, and artistic purposes for which it was established.

LAST Friday, at half-past 8 P.M., a magnificent meteor was seen at Privat, in the Ardèche Department. The meteor broke into several pieces and emitted a magnificent blue light.

THE state of the weather in the principal Algerian towns is posted regularly at the Meteorological Pavilion in the Trocadero, Paris. A special column is devoted to describing the state of the sea, but the writers having the care of translating the telegrams are so ignorant that they have posted a notice for several days telling the Parisians that "the sea was very smooth at Laghouat and Biskra," two Saharan cities!

THE French *Journal Officiel* has published a notice intimating that a school for telegraphy has been established, and that the course of instruction will be opened in October next. Pupils will be admitted after a competition. Preliminary examinations will take place in several cities of France, and the final examination will take place in Paris. A certain number of places is reserved to the pupils of the Polytechnic School, without competition, though it is expected that this privilege will be cancelled by the Chamber of Deputies when deliberating upon the matter next session.

THE inflation of the great Giffard balloon was completed on Sunday evening. Aeronauts are now busy arranging the manœuvres, and it is expected that the preliminary ascents will be made at the end of this week. Next week M. Tissandier will make a communication to the Academy of Sciences on behalf of M. Giffard, who has appointed him general manager. MM. Eugene and Jules Godard and Camille Dartois have been appointed aeronauts. Free ascents will be made twice a week from the Cour des Tuileries. The reappearance of a monster captive balloon will very likely revive an interest in aeronautics. We have heard of many contemplated experiments on a smaller scale. Some Americans have constructed, with light oiled silk, a cylinder six feet in height and twenty feet long, which has been filled with pure hydrogen. This elongated balloon supports an immense sheet in silk, on which advertisements are to be painted and exhibited at fixed rates per hour. The effect is said to be very graceful indeed.

IT is stated, on the authority of the *Agricultural Gazette* of Hanover, that a discovery has recently been made of a new remedy for the prevention of ravages to cabbages by the common caterpillar. A steward of an estate in Hanover having observed that one bed of cabbages was left untouched by caterpillars, whilst others were infested with them, found that the healthy bed had a quantity of dill growing on it, the smell of which, apparently, was obnoxious to the caterpillars. As dill will grow in almost any soil, it is suggested that the experiment might be tried by agriculturists. As indicative of the possibility of there being some truth in this, *The Colonies and India* says:—"We have heard of the common green ("gooseberry") caterpillar being kept off by planting broad beans close to the bushes—and the pyrethrum, a strong smelling weed which is cultivated as a garden border flower—is said to protect vines from the ravages of the *Phylloxera*.

THE phenomenon of supersaturated solutions of salts forms the subject of an elaborate study by M. D. Gernez (*Ann. de l'École normale*, 1878). He finds that besides water a number of other liquids, such as carbon-disulphide, the hydro-carbons, the phenols, and notably the alcohols, afford instances of this peculiarity. A substance which does not yield supersaturated solutions with one solvent never yields them with another, nor can the phenomenon be produced by the addition of substances such as dextrin, tending to increase the viscosity of the solvent. The salts yielding these solutions most easily are sodium carbonate, calcium nitrate, magnesium sulphate, plumbic acetate, and alum. In the case of all five crystallisation ensues only on the introduction of crystals of an isomorphous substance, and the latter lose this property if once heated above a certain temperature, for example, 98° for alum. The author gives a list of 120 substances which possess the property of yielding these solutions.

THE Ethnographic Congress in connection with the Paris Exhibition was opened on Monday. The President, M. Leon de Rosny, delivered a somewhat vague and apparently not over scientific address, in which he defined ethnography as the study of conscious humanity, the discovery of the law of the evolution of humanity in its relation with the general laws of the universe. While anthropology studied individuals or grouped them only according to physical affinities, ethnography recognised groups formed by collective consent and based on compatibilities of temperament and intelligence. It was the fashion, indeed, to decry half-breeds; but the majority, if not the whole, of nations prominent in history had been mixed races, and this mixture was the law of nature, though, under unfavourable conditions, it sometimes proved a failure.

THE report of the Miners' Association of Cornwall and Devon for the year 1877 is, we are glad to say, as satisfactory as usual.

MR. E. SCHÖNE, of Moscow, who is making extended researches on the presence of peroxide of hydrogen in the air, communicates recently the results of his investigations on its presence in the solid and liquid depositions from the atmosphere. He finds that in general the percentage of peroxide of hydrogen increases with the height above the earth's surface at which the condensation of the aqueous vapour takes place. Thus rain always contains more than snow—the rain-clouds moving, as is well known, at a higher elevation than those yielding snow—mists which take their origin near the earth's surface contain comparatively little, and dew and frost show no traces.

THE additions to the Zoological Society's Gardens during the past week include a Beatrix Antelope (*Oryx beatrix*) from Tyef Hedgar, presented by Commander Burke, s.s. *Arcot*, two Crested Porcupines (*Hystrix cristata*), a Banded Ichneumon (*Herpestes fasciatus*) from East Africa, presented by Dr. G. P. Badger; four Paradise Whydah Birds (*Vidua paradisea*), a pin-tailed Whydah Bird (*Vidua principalis*), three Grenadier Weaver Birds (*Euplectes oryx*) from East Africa, presented by Mr. Archibald Brown; a Barn Owl (*Strix flammea*) from Mesopotamia, presented by Commander Wyatt, s.s. *Deccan*; a Hawk's-billed Turtle (*Chelone imbricata*) from the East Indies, presented by Capt. Henderson; a Water Chevrotain (*Hyomoschus aquaticus*), an Electric Silurus (*Malapterurus beninensis*) from West Africa, a Plantain Squirrel (*Sciurus plantani*) from Java, purchased; a Chimpanzee (*Troglodytes niger*) from West Africa, four Vulturine Guinea Fowls (*Numida vulturina*) from East Africa, deposited; a Hairy Tree Porcupine (*Sphingurus villosus*) born in the Gardens.

THE EXPLANATION OF CERTAIN ACOUSTICAL PHENOMENA [1]

MUSICAL sounds have their origin in the vibrations of material systems. In many cases, *e.g.* the pianoforte, the vibrations are free, and are then necessarily of short duration. In other cases, *e.g.* organ pipes and instruments of the violin class, the vibrations are maintained, which can only happen when the vibrating body is in connection with a source of energy capable of compensating the loss caused by friction and generation of aërial waves. The theory of free vibrations is tolerably complete, but the explanations hitherto given of maintained vibrations are generally inadequate, and in most cases altogether illusory.

In consequence of its connection with a source of energy, a vibrating body is subject to certain forces, whose nature and effects are to be estimated. These forces are divisible into two groups. The first group operate upon the periodic time of the vibration, *i.e.* upon the pitch of the resulting note, and their effect may be in either direction. The second group of forces do not alter the pitch, but either encourage or discourage the vibration. In the first case only can the vibration be maintained; so that for the explanation of any maintained vibration, it is necessary to examine the character of the second group of forces sufficiently to discover whether their effect is favourable or unfavourable. In illustration of these remarks, the simple case of a common pendulum was considered. The effect of a small periodic horizontal impulse is in general both to alter the periodic time and the amplitude of vibration. If the impulse (supposed to be always in the same direction) acts when the pendulum passes through its lowest position, the force belongs to the second group. It leaves the periodic time unaltered, and encourages or discourages the vibration according as the direction of the pendulum's motion is the same or the opposite of that of the impulse. If, on the other hand, the impulse acts when the pendulum is at one or other of the limits of its swing, the effect is solely on the periodic time, and the vibration is neither encouraged nor discouraged. In order to encourage, *i.e.* practically in order to maintain a vibration, it is necessary that the forces should not depend solely upon the position of the vibrating body. Thus, in the case of the pendulum, if a small impulse in a given direction acts upon it every time that it passes through its lowest position, the vibration is not maintained, the advantage gained as the pendulum makes a passage in the same direction as that in which the impulse acts being exactly neutralised on the return passage, when the motion is in the opposite direction.

As an example of the application of these principles, the maintenance of an electric tuning-fork was discussed. If the magnetic forces depended only upon the position of the fork, the vibration could not be maintained. It appears, therefore, that the explanations usually given do not touch the real point at all. The fact that the vibrations are maintained is a proof that the forces do not depend solely upon the position of the fork. The causes of deviation are two: the self-induction of the electric currents, and the adhesion of the mercury to the wire whose motion makes and breaks the contact. On both accounts the magnetic forces are more powerful in the latter than in the earlier part of the contact, although the position of the fork is the same; and it is on this *difference* that the possibility of maintenance depends. Of course the arrangement must be such that the retardation of force *encourages* the vibration, and the arrangement which in fact encourages the vibration would have had the opposite effect, if the nature of electric currents had been such that they were more powerful during the earlier than during the later stages of a contact.

In order to bring the subject within the limits of a lecture, one class of maintained vibrations was selected for discussion, that, namely, of which *heat* is the motive power. The best understood example of this kind of maintenance is that afforded by Trevelyan's bars, or rockers. A heated brass or copper bar, so shaped as to rock readily from one point of support to another, is laid upon a cold block of lead. The communication of heat through the point of support expands the lead lying immediately below in such a manner that the rocker receives a small impulse. During the interruption of the contact the communicated heat has time to disperse itself in some degree into the mass of lead, and it is not difficult to see that the impulse is of a kind to encourage the motion. But the most interesting vibrations of this class are those in which the vibrating body consists of a mass of air more or less completely confined.

If heat be periodically communicated to, and abstracted from, a mass of air vibrating (for example) in a cylinder bounded by a piston, the effect produced will depend upon the phase of the vibration at which the transfer of heat takes place. If heat be given to the air at the moment of greatest condensation, or taken from it at the moment of greatest rarefaction, the vibration is encouraged. On the other hand, if heat be given at the moment of greatest rarefaction, or abstracted at the moment of greatest condensation, the vibration is discouraged. The latter effect takes place of itself, when the rapidity of alternation is neither very great nor very small, in consequence of radiation; for when air is condensed it becomes hotter, and communicates heat to surrounding bodies. The two extreme cases are exceptional, though for different reasons. In the first, which corresponds to the suppositions of Laplace's theory of the propagation of sound, there is not sufficient time for a sensible transfer to be effected. In the second the temperature remains nearly constant, and the loss of heat occurs during the *process* of condensation, and not when the condensation is effected. This case corresponds to Newton's theory of the velocity of sound. When the transfer of heat takes place at the moments of greatest condensation or of greatest rarefaction, the pitch is not affected.

If the air be at its normal density at the moment when the transfer of heat takes place, the vibration is neither encouraged nor discouraged, but the pitch is altered. Thus the pitch is *raised*, if heat be communicated a quarter period *before* the phase of greatest condensation, and the pitch is *lowered* if the heat be communicated a quarter period *after* the phase of greatest condensation.

In general both kinds of effects are produced by a periodic transfer of heat. The pitch is altered, and the vibrations are either encouraged or discouraged. But there is no effect of the second kind if the air concerned be at a loop, *i.e.*, a place where the density does not vary, nor if the communication of heat be the same at any stage of rarefaction, as in the corresponding stage of condensation.

The first example of aërial vibrations maintained by heat was found in a phenomenon which has often been observed by glass-blowers, and was made the subject of a systematic investigation by Dr. Sondhauss. When a bulb about three quarters of an inch in diameter is blown at the end of a somewhat narrow tube, 5 or 6 inches in length, a sound is sometimes heard proceeding from the heated glass. It was proved by Sondhauss that a vibration of the glass itself is no essential part of the phenomenon, and the same observer was very successful in discovering the connection between the *pitch* of the note and the dimensions of the apparatus. But no explanation (worthy of the name) of the production of sound has been given.

For the sake of simplicity, a simple tube, hot at the closed end and getting gradually cooler towards the open end, was first considered. At a quarter of a period *before* the phase of greatest condensation (which occurs almost simultaneously at all parts of the column) the air is moving inwards, *i.e.* towards the closed end, and therefore is passing from colder to hotter parts of the tube; but the heat received at this moment (of normal density) has no effect either in encouraging or discouraging the vibration. The same would be true of the entire operation of the heat, if the adjustment of temperature were instantaneous, so that there was never any sensible difference between the temperatures of the air and of the neighbouring parts of the tube. But in fact the adjustment of temperature takes *time*, and thus the temperature of the air deviates from that of the neighbouring parts of the tube, inclining towards the temperature of that part of the tube *from* which the air has just come. From this it follows that at the phase of greatest condensation heat is received by the air, and at the phase of greatest rarefaction is given up from it, and thus there is a tendency to maintain the vibrations. It must not be forgotten, however, that apart from transfer of heat altogether, the condensed air is hotter than the rarefied air, and that in order that the whole effect of heat may be on the side of encouragement, it is necessary that, previous to condensation, the air should pass not merely towards a hotter part of the tube, but towards a part of the tube which is hotter than the air will be when it arrives there. On this account a great range of temperature is necessary for the maintenance of vibration, and even with a great range the influence of the transfer of heat is necessarily unfavourable at the closed end, where the motion is very small. This is probably the reason

[1] Friday Evening Lecture, by Lord Rayleigh, M.A., F.R.S., March 15, at the Royal Institution of Great Britain.

of the advantage of a bulb. It is obvious that if the *open* end of the tube were heated, the effect of the transfer of heat would be even more unfavourable than in the case of a temperature uniform throughout.

The sounds emitted by a jet of hydrogen, burning in an open tube, were noticed soon after the discovery of the gas, and have been the subject of several elaborate inquiries. The fact that the notes are substantially the same as those which may be elicited from the tube in other ways, *e.g.*, by blowing, was announced by Chladni. Faraday proved that other gases were competent to take the place of hydrogen, though not without disadvantage. But it is to Sondhauss that we owe the most detailed examination of the circumstances under which the sound is produced. His experiments prove the importance of the part taken by the column of gas in the tube which supplies the jet. For example, sound cannot be obtained with a supply tube which is plugged with cotton in the neighbourhood of the jet, although no difference can be detected by the eye between the flame thus obtained and others which are competent to excite sound. When the supply tube is unobstructed, the sounds obtainable are limited as to pitch, often dividing themselves into detached groups. In the intervals between the groups no coaxing will induce a maintained sound, and it may be added that, for a part of the interval at any rate, the influence of the flame is inimical, so that a vibration started by a blow is damped more rapidly than if the jet were not ignited.

Partly in consequence of the peculiar behaviour of flames, and partly for other reasons, the thorough explanation of these phenomena is a matter of some difficulty; but there can be no doubt that they fall under the head of vibrations maintained by heat, the heat being communicated periodically to the mass of air confined in the sounding tube at a place where, in the course of a vibration, the pressure varies. Although some authors have shown an inclination to lay stress upon the effects of the current of air passing through the tube, the sounds can readily be produced, not only when there is no through draught, but even when the flame is so situated that there is no sensible periodic motion of the air in its neighbourhood. In the course of the lecture a globe intended for burning phosphorus in oxygen gas was used as a resonator, and, when excited by a hydrogen flame well removed from the neck, gave a pure tone of about ninety-five vibrations per second.

In consequence of the variable pressure within the resonator, the issue of gas, and therefore the equivalent of heat, varies during the vibration. The question is under what circumstances the variation is of the kind necessary for the maintenance of the vibration. If we were to suppose, as we might at first be inclined to do, that the issue of gas is greatest when the pressure in the resonator is least, and that the phase of greatest development of heat coincides with that of the greatest issue of gas, we should have the condition of things the most unfavourable of all to the persistence of the vibration. It is not difficult, however, to see that both suppositions are incorrect. In the supply tube (supposed to be unplugged, and of not too small bore) stationary, or approximately stationary, vibrations are excited, whose phase is either the same or the opposite of that of the vibration in the resonator. If the length of the supply tube from the burner to the open end in the gas-generating flask be less than a quarter of the wave length in hydrogen of the actual vibration, the greatest issue of gas *precedes* by a quarter period the phase of greatest condensation; so that if the development of heat is *retarded* somewhat in comparison with the issue of gas, a state of things exists *favourable* to the maintenance of the sound. Some such retardation is inevitable, because a jet of inflammable gas can burn only at the outside, but in many cases a still more potent cause may be found in the fact that during the retreat of the gas in the supply tube small quantities of air may enter from the interior of the resonator, whose expulsion must be effected before the inflammable gas can again begin to escape.

If the length of the supply tube amounts to exactly one quarter of the wave length, the stationary vibration within it will be of such a character that a node is formed at the burner, the variable part of the pressure just inside the burner being the same as in the interior of the resonator. Under these circumstances there is nothing to make the flow of gas, or the development of heat, variable, and therefore the vibration cannot be maintained. This particular case is free from some of the difficulties which attach themselves to the general problem, and the conclusion is in accordance with Sondhauss' observations.

When the supply tube is somewhat longer than a quarter of the wave, the motion of the gas is materially different from that first described. Instead of preceding, the greatest outward flow of gas *follows* at a quarter period interval the phase of greatest condensation, and therefore if the development of heat be somewhat retarded, the whole effect is unfavourable. This state of things continues to prevail, as the supply tube is lengthened, until the length of half a wave is reached, after which the motion again changes sign, so as to restore the possibility of maintenance. Although the size of the flame and its position in the tube (or neck of resonator) are not without influence, this sketch of the theory is sufficient to explain the fact, formulated by Dr. Sondhauss, that the principal element in the question is the length of the supply tube.

The next example of the production of sound by heat, shown in the lecture, was a very interesting phenomenon discovered by Rijke. When a piece of fine metallic gauze, stretching across the lower part of a tube, open at both ends and held vertically, is heated by a gas flame placed under it, a sound of considerable power, and lasting for several seconds, is observed almost immediately *after* the removal of the flame. Differing in this respect from the case of sonorous flames, the generation of sound was found by Rijke to be closely connected with the formation of a through draught, which impinges upon the heated gauze. In this form of the experiment the heat is soon abstracted, and then the sound ceases; but by keeping the gauze hot by the current from a powerful galvanic battery, Rijke was able to obtain the prolongation of the sound for an indefinite period. In any case from the point of view of the lecture the sound is to be regarded as a *maintained* sound.

In accordance with the general views already explained, we have to examine the character of the variable communication of heat from the gauze to the air. So far as the communication is affected directly by variations of pressure or density the influence is unfavourable, inasmuch as the air will receive less heat from the gauze when its own temperature is raised by condensation. The maintenance depends upon the variable transfer of heat due to the varying *motions* of the air through the gauze, this motion being compounded of a uniform motion upwards with a motion, alternately upwards and downwards, due to the vibration. In the lower half of the tube these motions conspire a quarter period *before* the phase of greatest condensation, and oppose one another a quarter period after that phase. The rate of transfer of heat will depend mainly upon the temperature of the air in contact with the gauze being greatest when that temperature is lowest. Perhaps the easiest way to trace the mode of action is to begin with the case of a simple vibration without a steady current. Under these circumstances the whole of the air which comes in contact with the metal, in the course of a complete period, becomes heated; and after this state of things is established there is comparatively little further transfer of heat. The effect of superposing a small steady upwards current is now easily recognised. At the limit of the inwards motion, *i.e.* at the phase of greatest condensation, a small quantity of air comes into contact with the metal, which has not done so before, and is accordingly cool; and the heat communicated to this quantity of air acts in the most favourable manner for the maintenance of the vibration.

A quite different result ensues if the gauze be placed in the *upper* half of the tube. In this case the fresh air will come into the field at the moment of greatest rarefaction, when the communication of heat has an unfavourable instead of a favourable effect. The principal note of the tube therefore cannot be sounded.

A complementary phenomenon discovered by Bosscha and Riess may be explained upon the same principles. If a current of *hot* air impinge upon *cold* gauze, sound is produced; but in order to obtain the principal note of the tube the gauze must be in the upper, and not as before in the lower, half of the tube. An experiment due to Riess was shown in which the sound is maintained indefinitely. The upper part of a brass tube is kept cool by water contained in a tin vessel, through the bottom of which the tube passes. In this way the gauze remains comparatively cool, although exposed to the heat of a gas flame situated an inch or two below it. The experiment sometimes succeeds better when the draught is checked by a plate of wood placed somewhat closely over the top of the tube.

Both in Rijke's and Riess' experiments the variable transfer of heat depends upon the motion of vibration, while the effect of the transfer depends upon the variation of pressure. The gauze must therefore be placed where both effects are sensible,

i.e. neither near a node nor near a loop. About a quarter of the length of the tube, from the lower or upper end, as the case may be, appears to be the most favourable position.

RAYLEIGH

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

AMONG the bequests of the late Mr. Henry Brown, J.P., formerly of Bradford, is a sum of 5,000*l*. to the Yorkshire College, Leeds, for the purpose of founding and maintaining scholarships.

THE New York *Nation* states that Dr. A. S. Packard, jun., has been appointed Professor of Natural History at Brown University. His departure from Salem, Mass., the *Nation* states, following on Prof. Morse's and Prof. Putnam's is a serious loss to that scientific centre, and implies an inadequate endowment of the Peabody Academy of Sciences.

THE first conferment of degrees by the Johns Hopkins University took place on June 13. Four candidates were admitted to the degrees of Ph.D. and M.A.

THE following figures, which have been published quite recently at Algiers, will give an idea of the state of public instruction in that colony. Superior instruction is represented only by a preparatory school of medicine in Algiers. It is contemplated to establish in that city a university of letters, science, law, and medicine; but no step has yet been taken to realise the scheme. There are colleges, or lycées at Algiers, Oran, Constantine, Bone, Philippeville, Blidah, Mostaganem, and one or two other places, and two clerical institutions, one at Blidah, and the other at Algiers. The number of pupils of these establishments is 3,142 in a population of 344,849 of European extraction. Primary instruction is given in 803 schools, frequented by 66,343. A few natives follow the course of instruction in European or secondary schools. Most of them are pupils in the Algiers lycée, which has no less than 980 pupils, and is considered one of the best under the authority of the French government, even in France. Great efforts have been made to organise French-Arab schools for natives, but with not much success. Within the last few years thirteen French-Arab schools have been opened in the Sahara and Kabylè, which have now 1,481 pupils. The aggregate number of young Arabs, receiving education from the French government, is only 1,573 boys, and 173 girls out of a population of 2,500,000. A normal school has been established at Mustapha, near Algiers, and numbers from thirty to forty pupils.

SCIENTIFIC SERIALS

Annalen der Physik und Chemie, No. 4, 1878.—This number commences with a paper by M. Schering on friction currents as exemplified in the rubber of a (cylinder) electrical machine. For production of such currents it is unimportant whether the cylinder be connected to earth or not; and the occurrence of *opposite* electricities at the two ends of the rubber is also not essential. The electricity on the hinder margin of the rubber is derived from the insulator (cylinder); for it agrees in sign with that of the latter, and nearly always disappears when the insulator is connected to earth. The friction causes a less quantity of negative electricity to exist on the hinder margin of the rubber than on the forward margin; the quantity of electricity steadily varies from the hinder to the forward margin.—M. Fröhlich investigates the intensity of diffracted light in relation to that of the incident light. His experimental results closely correspond to those of theory. With small angles the entire incident energy of motion appears again after diffraction as light-motion.—Fresnel's theory of diffraction phenomena is treated at some length by M. Voigt.—Studying certain hydrodynamic problems in relation to the theory of ocean currents, M. Zöppritz concludes, *inter alia*, that the influence of friction has, in one direction, been underrated, in another overrated; the former, because it has not been supposed to extend deep enough, the latter, because in regard to propagation of variable current-motions too much has been ascribed to it. He calculates that with a mean ocean depth of 4,000 m. the trade winds in their present extent and strength would have to blow 100,000 years ere the present state of motion of the equatorial current could be supposed approximately stationary. The damping influence of continents and islands would somewhat diminish the number.—M. Antolik communicates further observations on the gliding of electric sparks, obtaining new evidence for the fact that a greater tension is required for discharge of positive than for that of negative electricity, and that the one kind passes more rapidly and further than the other.—A formula determining the rotation of the plane of polarisation in quartz for all colours as function of the temperature, is given by M. Sohncke, who also finds that the rotation in chlorate of soda increases with rising temperature in a greater degree than in quartz.—An improved tangent galvanometer for lecture purposes (based on the principle of the Gauss-Weber mirror-magnetometer), a modification of the mercury air-pump, and a method of more accurate measurement of thickness by means of the spherometer, are among the remaining subjects here dealt with.

No. 5.—M. Kohlrausch here describes a "total reflectometer," or instrument by means of which the total reflection in solid bodies is utilised for determination of refraction. (The instrument can also be adapted for liquids.) A liquid is employed which refracts more strongly than the body examined (generally sulphide of carbon). The author gives his numerical results in a table.—A paper on the theory of double refraction, by M. Lommel, furnishes, with two previous papers, the outlines of a new theory of light (he says it might be called the "friction theory"), in which the phenomena in their connection are explained by the reciprocal action of the ether and the particles of bodies.—M. von Waha calls attention to some interesting movements obtained in badly-conducting liquids (as olive oil or petroleum), when *placed, e.g.*, on a horizontal metallic plate, connected electrically with one pole of a Holtz machine, while a point connected with the other pole is held above the liquid.—The phenomena of resonance in hollow spaces are investigated mathematically and experimentally by M. Wand, and an improved anemometer, capable of measuring the mean velocity of air-currents of constant direction between wide limits, forms the subject of a paper by M. Recknagel.

Actes de la Société Helvétique des Sciences Naturelles (C. R. 1876–77) contain an account of the sixtieth meeting of the Society, held at Bex on August 20–22, 1877, together with notes of the sectional meetings, and the following more elaborate memoirs:—On the adaptation of copepod crustaceans to parasitism, by Prof. K. Vogt.—On the fecundation and first development of the ovum, by H. Fol.—On the railway over the Simplon, by Herr Lommel.—Historical account of the mines and salt-works of Bex, by Ch. Grenier.—On the retrogradation of the shadow on the sun dial, by E. Guillemin.—Note on the study of thunderstorms accompanied by hailstorms and electric phenomena, by D. Colladon.—On the geology of the neighbourhood of Bex, by E. Renevier.—On some geological formations in the Bernese Alps, by S. Chavannes.—On the nummulites of the Western Alps, by Ph. de la Harpe.—On the origin and the repartition of the Turbellaria of the deep fauna of the Lake of Geneva, by G. du Plessis.—On the formation of feathers in the gold-hair penguin and *Megapodius*, by Th. Studer.—On the blood corpuscles of *Mermis aquatilis*, Duj., by E. Bugnion.—On a new Amphipode (*Gammarus rhipidiophorus*), by O. I. Catta.—On the doubtful species in the flora of Switzerland, by L. Leresche.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, June 19.—"On the Reversal of the Lines of Metallic Vapours," by G. D. Liveing, M.A., Professor of Chemistry, and J. Dewar, M.A., F.R.S., Jacksonian Professor, University of Cambridge. No. III.

In our last communication to the Royal Society we described certain absorption lines, which we had observed to be produced by the vapour of magnesium in the presence of hydrogen, and certain other lines which were observed when potassium, and others when sodium, was present, in addition to magnesium and hydrogen. These lines correspond to no known emission lines of those elements; but, inasmuch as they appeared to be regularly produced by the mixtures described, and not otherwise, we could only ascribe their origin to the mixtures as distinct from the separate elements. It became a question of interest, then, whether we could find the conditions under which the same mixtures would give luminous spectra, consisting of the lines which we had seen reversed. On observing sparks from an induction coil taken between magnesium points in an atmosphere of hydrogen, we soon found that a bright line regularly appeared, with a wave-

length about 5,210, in the same position as one of the most conspicuous of the dark lines we had observed to be produced by vapour of magnesium with hydrogen in our iron tubes. This line is best seen, *i.e.*, is most steady, when no Leyden jar is used, and the rheotome (the coil we used has an ordinary self-acting one) is screwed back, so that it will but just work. It may, however, be seen when the coil is in its ordinary state, and when a small Leyden jar is interposed; but it disappears (except in flashes) when a larger Leyden jar is used, if the hydrogen be at the atmospheric pressure. This line does not usually extend across the whole interval between the electrodes, and is sometimes only seen near the negative electrode. Its presence seems to depend on the temperature, as it is not seen continuously when a large Leyden jar is employed, until the pressure of the hydrogen and its resistance is very much reduced. When well-dried nitrogen or carbonic oxide is substituted for hydrogen, this line disappears entirely; but if any hydrogen or traces of moisture be present it comes out when the pressure is much reduced. In such cases the hydrogen lines C and F are always visible as well. Sometimes several fine lines appear on the more refrangible side of this line, between it and the *b* group, which give it the appearance of being a narrow band, shaded on that side. We have used various samples of magnesium as electrodes, and they all give the same results. We have also used hydrogen, prepared and purified in different ways: hydrogen prepared by the action of zinc on dilute sulphuric acid, purified by an acid solution of bichromate or permanganate, and by potash, and dried by sulphuric acid; electrolytic hydrogen; hydrogen from dry formiate of soda and soda lime; hydrogen occluded by sodium and expelled by heat; and hydrogen occluded by palladium and expelled by heat. In the last two cases the whole apparatus was connected by fusion, and a Sprengel pump, also connected by fusion, employed to remove the air. In all cases the phenomena were the same.

In addition to the above-mentioned line, we observed that there is also produced a series of fine lines, commencing close to the most refrangible line of the *b* group, and extending with gradually diminishing intensity towards the blue. These lines are so fine and close to one another, that in a small spectroscope they appear like a broad shaded band. We have little doubt that the dark absorption line, with wave-length about 5,140, shading towards the blue, which we previously observed in our iron tubes, and described in our last communication, was a reversal of part of these lines, though the latter extend much further towards the blue than we had observed the absorption to extend. In fact, the bright lines extend somewhat more than half the distance between *b* and F, from forty-five to fifty being visible, and placed at nearly equal distances from each other. They also commence close to the *b* group, *i.e.*, with a wave-length nearly 5,164, but the first two or three lines at that end are not so bright as those which immediately succeed them. The light giving these lines does not extend to more than a short distance from the electrodes, and is generally most conspicuous at the negative electrode. There is a difficulty in consequence of the flickering character of the discharge in getting any accurate measures of them, though they are bright enough, especially at the less refrangible end, to be easily seen. The comparative faintness of the light from the iron tubes appears to us almost sufficient to account for our not having seen the reversed lines so completely as the bright ones; nevertheless, it is quite in accordance with what we in other cases observed, to suppose that some of these lines may be more easily reversed at the temperature of the iron tubes than others.

Zoological Society, June 18.—Arthur Grote, vice-president, in the chair.—The Secretary read extracts from a letter addressed to him by Mr. E. L. Layard, containing remarks on two species of New Caledonian birds.—A second communication from Mr. Layard stated that there was an example of the recently-described woolly cheetah (*Felis lanea*) in the South African Museum at Cape Town.—Mr. Edward R. Alston read a paper on the squirrels of the Neotropical region, in which he recognised twelve out of fifty-nine described species, and redescribed two, *Sciurus rufo-niger*, Pucheran, and *S. pusillus*, Geoffroy, which had been recently overlooked.—Mr. Sclater exhibited and made remarks on a third collection of birds from Duke of York Island, New Britain, and New Ireland, which he had received from the Rev. George Brown, C.M.Z.S. Amongst them was an example of a new fruit-pigeon, proposed to be called *Carpophaga melanochroa*.—A communication was read from Dr. M. Watson, containing a description of the male generative organs of *Chlamydophorus truncatus* and *Dasypus sexcinctus*.—A communication was read from Prof. Garrod on certain points in the anatomy of Levaillant's darter (*Plotus levaillanti*).—A communication was read from Messrs. Garrod and Turner on the gravid uterus and placenta of *Hyomoschus aquaticus*.—A communication was read from Mr. F. Moore containing the descriptions of New Asiatic butterflies of the family Hesperiidæ.—A second communication from Mr. Moore gave a list of the lepidopterous insects collected by the late Mr. R. Swinhoe, in the Island of Hainan.—A communication was read from the Marquis of Tweeddale, F.R.S., being the tenth of his contributions to the ornithology of the Philippines. The present paper gave an account of the collection made by Mr. A. H. Everett in the Island of Bohol. The collection contained representatives of forty-seven species. Although all of these were previously known, seven of them had not been before recorded as being inhabitants of the Philippines.—Dr. O. Finsch, C.M.Z.S., read the description of a new species of starling from Lake Marka-kul, in the Chinese High Altai, which he proposed to name *Sturnus poltaratzskyi*, after Gen. Poltaratzsky, Governor of Semipalatinsk.—A communication was read from Mr. H. W. Bates containing the description of new species of coleopterous insects (*Geodephaga* and *Longicornia*) taken by the late Dr. Stoliczka during the Forsyth Expedition to Kashgar in 1873–1874.—A communication was read from Dr. G. Hartlaub, in which he gave the description of a new species of *Notauges* (*N. hildebrandti*) of Cabanis, M., discovered by Mr. Hildebrandt at Ikanga in Ukamba, Eastern Africa.—A communication was read from Lieut.-Col. R. H. Beddome, C.M.Z.S., giving the description of a new batrachian from Southern India belonging to the family *Phryniscidæ*, which he proposed to call *Melanobatrachus indicus*.—Sir V. Brooke, Bart., exhibited and made remarks on a fine head of the male *Gazella granti*, originally described from sketches made by Capt. Speke during Speke and Grant's expedition. The present specimen had been shot by Mr. Arkwright about eighty miles from Ugogo in Eastern Africa.—A communication was read from Prof. v. V. Barboza du Bocage, F.M.Z.S., containing a list of the antelopes observed in Angola.—A communication was read from Mr. Carl Bock, in which he gave the description of two new species of shells from China and Japan.—A communication was read from Mr. Edgar A. Smith, containing the description of five new shells from the Island of Formosa and the Persian Gulf, with notes upon some known species.—Messrs. Godman and Salvin read the descriptions of some apparently new species of butterflies from New Ireland and New Britain, received from the Rev. G. Brown.—Mr. O. Salvin read the twelfth of a series of reports on the collection of birds made during the voyage of H.M.S. *Challenger*. The present paper contained an account of the Procellariidæ, collected during the expedition. Eighty specimens had been obtained belonging to twenty-two species.—Mr. Sclater read some supplementary notes on the curassows now or lately living in the Society's gardens.—Mr. J. Wood-Mason read a paper on the structure and development of the trachea in the Indian painted snipe (*Rhynchæa bengalensis*).

Physical Society, June 22.—Prof. G. C. Foster, vice-president, and afterwards Prof. W. G. Adams, president, in the chair.—The following candidate was elected a Member of the Society: Mr. F. W. Grierson.—Prof. W. G. Adams exhibited a new form of polariscope suitable for projecting on to a screen the figures formed by any crystal, and for measuring the angle between the optic axes. Parallel light from the electric lamp, after traversing a Nicol of about two-inch aperture, is rendered divergent by a set of lenses. The crystal under examination is placed in a recess formed by removing a slice from the middle of a spherical lens which is capable of motion in any direction about its centre, while any movement in the vertical plane passing through the axis of the instrument can be measured by a scale and Vernier; and if, by such a motion, the point on the screen representing the position of one axis, when the two are in the vertical plane, be transferred to that indicating the position initially occupied by the other axis, we have at once a measure of the optic angle of the crystal, for the rotation of two plano-spherical lenses forming an exact sphere has no effect on the direction of the beam.—Mr. Walter Baily read a paper on the effect of starch, salicene, unannealed glass, &c., on polarised light. In his experiments light was passed through a Nicol's prism, then through a quarter undulation plate, and then through a body having an optical structure symmetrical round

an axis in the direction of the ray, such as any of the above-named substances. The axes of the quarter undulation plate being taken as axes of reference, ρ being the angle between the plane of polarisation and one of these axes, σ half the difference of retardation at a given point between the part of the light resolved in a plane through the axis of the body and the part resolved perpendicular to that plane, ϕ being the angle between an axis of the $\frac{1}{4}$-undulation plate and the perpendicular on the axis of the body from the given point (which perpendicular is taken as the initial line in the equation to the ellipse defining the light at such point) and r, θ, being the co-ordinates of this ellipse, the writer finds the equation to the ellipse to be

$$1 + A\cos\theta + B\sin 2\theta = r^{-2}\left\{1 - (A^2 + B^2)\right\}$$
$$\text{where } A = -\cos 2\rho \cos 2\phi$$
$$\text{and } B = \sin 2\rho \sin 2\sigma + \cos 2\rho \cos 2\sigma \sin 2\phi.$$

From this equation diagrams have been drawn to exhibit the condition of the light at every point for different positions of the polariser. For the simple case of salicene or starch, in which the difference of retardation is the same, or nearly so, for all distances from the axis of the body, the diagram consists of a ring of ellipses of various eccentricities and inclinations, each ellipse showing the condition of the light along the radius on which it lies. For the general case in which the difference of retardation is a function of the distance from the axis of the body the diagram consists of two series of curves, one series being "isomorphal" curves or curves along which the eccentricity of the ellipse is constant, and the other series being "isoclinal," or curves along which the inclination of the axis major of the ellipse to the perpendicular in the axis of the body is constant. The general equation to the isomorphal lines is $A^2 + B^2 = \text{constant}$; and to the isoclinal lines is $\frac{A}{B} = \text{constant}$. These two series of curves completely define the form and position of the ellipse of polarisation at every point and render it easy to determine what appearances will be presented on passing the light through an analysing prism in any given position. The results obtained were illustrated by some experiment.—Prof. W. C. Unwin made a communication on the flow from orifices at different temperatures. A paper recently appeared in *The Franklin Journal of Science*, by Mr. Isherwood, giving results of experiments on this subject, and according to him the volume discharged from a given orifice is increased by about 12 per cent. on raising the temperature from 60° to 212°. It is difficult to accept this result, because the friction is known to diminish the discharge by an amount much less than 12 per cent., and no other cause than a decrease of friction can be assigned to account for Mr. Isherwood's results. In the author's experiments the increase of discharge at 190° above that at 60° was only 4 per cent., with conoidal orifices in the form of the vena contracta; with thin edged orifices the variation of discharge was still less. He is disposed to think that the great increase of discharge in Mr. Isherwood's experiments was due to diminution of friction in a rather small pipe leading to the orifices, and would not occur with any other arrangement. —Mr. Graham then read a paper on complementary colours. He stated that the three primaries are green, red, and blue, and not yellow, red, and blue; that yellow is a binary compound of green and red; and that yellow and blue when mixed form white. He remarked that after looking at a green disc the eye evokes another colour, but this is not seen unless certain conditions are fulfilled; thus the undulations must be arrested by a gray surface: this was proved by an experiment in which a green disc carrying a concentric ring half white and half black was caused to rotate, when a medium gray was produced and this at once arrested and made visible pink the complement of the green. Several other complementaries were shown by the same means. Mr. Graham next showed how the grays can be formed by cancelling either reflected or transmitted rays of white light. The first of these cases is illustrated by white paper painted over with a wash of Indian ink, and the second by the well-known Berlin tiles, in which light and shade are obtained by giving varying thickness to the ware. He showed that this last effect may be imitated by piling strips of paper to varying heights, and he has succeeded in photographing geometrical figures so formed. Lastly the author explained a method of arresting and showing the complementaries more satisfactorily than he considers has hitherto been done. Six thicknesses of white paper are gummed together and cut into a ring, a ring of the same size and shape being also cut from a disc of coloured paper, and the white ring is let in to fill its place. On observing such a disc by white transmitted light the complement is seen through the ring.—Prof. S. P. Thompson exhibited a series of magnetic figures illustrating electro-dynamic relations. The lines of magnetic force around a wire carrying a magnetic current can be shown by passing a wire through a glass plate, strewing iron filings around, and tapping the plate gently. The filings may be fixed in their places, if the plate has previously been gummed and dried, by softening the gum with steam. Such a prepared plate may be used to project the figures of the magnetic curves in the lantern. Two parallel like currents attract, their curves forming a figure illustrative of the action; or they repel if travelling in opposite directions, the repulsion also being evident from the form of the curves. It was shown by a series of such lantern-slides that a very large number of electro-dynamic relations can be illustrated by curves produced in this manner. Figures were thrown upon the screen illustrating the law of oblique currents, the attraction of a magnet into or its repulsion out of a circuit, the deflection of a magnetic needle by a current, and the mutual tendency of a current and magnetic pole to rotate. A very curious figure was produced by a current running through a magnet longitudinally. A transverse section of the lines of force at a pole gave neither the radial lines of the magnet nor the circular lines of the current, but a series of spirals. It was argued that Faraday's conception of the lines of force tending to shorten themselves supplied the means of interpreting the physical effects indicated by the lines of force in the various figures.—The Secretary read a paper by Mr. C. H. Hinton, on the co-ordination of space. If a cubical space be divided into twenty-seven numbered cubes, and each of these be again subdivided in the same way, and so on, the position of any point within the initial cube can be expressed by a reference to the numbers of the several cubes in which it is placed, and the more this series of numbers is extended, the more accurately is its position defined; and, further, if we consider an expression of the form $\ldots\ldots rqponm.lkjih\ldots\ldots$ where each letter stands for any number from 1 to 27, and if m, before the dot, indicates the unit space, it will be evident that by such an expression the position of any point in space can be indicated with any degree of accuracy; each letter representing a space twenty-seven times as great as that which immediately succeeds it; or, in place of 27, any other number offering special facilities for any given purpose may be employed. The author then gives some account of the manner in which the system can be utilised for classifying chemical phenomena and in arranging plants, &c.—An adaptation of the telephone and microphone for communicating vibrations to the phoneidoscope, by Mr. Tisley, was then shown. The metal disc carrying the soap film is fixed just above the telephone plate, and, this being in circuit with a microphone and battery, any vibration imparted to the microphone at once sets the soap film in action, the characteristic figures being at once obtained.—Mr. A. Haddon exhibited a modified form of microphone which he has arranged with a view to make the same instrument available for receiving sounds of any given intensity. Its main peculiarity consists in having a thin strip of elastic attached to the middle of the pointed graphite. By varying the tension of this elastic, the sensitiveness of the instrument can be accurately regulated.—The meeting of the Society was then adjourned to November.

Chemical Society, June 20.—Dr. Gladstone, president, in the chair.—The following papers were read:—Contributions to the history of the naphthalene series, No. 2, β naphthaquinone, by Dr. Stenhouse and Mr. Groves. By the action of nitric acid sp. gr. 1·2 on this substance, mononitroy-β-naphthaquinone was obtained in red crystals. By the action of dilute sulphuric acid a dark-coloured compound was obtained, which, on reduction, yielded white acicular crystals, and on oxidation orange-coloured prisms. The new quinone has the formula $C_{20}H_{10}O_4$; the authors purpose to call it dinaphthyldiquinone. It is very stable.—On pyrotritartaric and carbopyrotritaric acids, by Mr. G. Harrow. By saponifying diacetosuccinic ether with dilute sulphuric acid, the author succeeded in preparing these two acids; the author has obtained sodium and silver salts, and discusses their constitution.—Laboratory notes, by Dr. Armstrong.—On the action of alkaline hyprobromite on ammonium salts, urea, and oxamide, Prof. W. Foster. The author gives a *résumé* of the present state of our knowledge as to the action of hypobromite on ammonium salts and urea, with some results of his

own; he then investigates the action of hypobromite on oxamide—74·87 per cent. of its total nitrogen is given off—and endeavours to ascertain the precise condition of the suppressed nitrogen.—Action of the halogens at high temperatures on metallic oxides, by Messrs. C. F. Cross and S. Suguira. With lead oxides oxyiodides are formed, and with the oxides and carbonates of the alkaline earth metals in the presence of oxygen periodates are produced.—On manganese tetrachloride, by Mr. W. W. Fisher. The author has studied the action of strong hydrochloric acid on the black and red oxides of manganese; brown liquids are formed containing a highly chlorinated manganese compound, probably the tetrachloride, which is readily resolved into manganous chloride and free chlorine.—On salts of nitrous oxide, by Mr. A. E. Menke. The sodium salt was obtained by fusing nitrate of soda with iron filings; its properties and reactions were studied. Diver's silver salt was prepared, and its composition confirmed.—Notes on madder colouring matters, by Messrs. E. Schunck and H. Roemer. The authors have prepared some quantity of munjistin and examined its properties, also its reactions with acetic anhydride, bromine, potash, and nitric acid. In all respects munjistin resembles purpuroxanthic acid.—On the occlusion of hydrogen by copper, by Mr. G. S. Johnson. The discrepancy between the results obtained by previous experimenters is explained (1) by the fact that hydrogenised copper retains nearly all its hydrogen in vacuo at a red heat. (2) That the same metal occludes varying quantities of hydrogen. The amount occluded is in most cases sufficient to introduce a serious error in organic analysis. At a red heat copper oxide occludes carbonic acid.—On the *rôle* played by carbon in reducing the sulphates of the alkalies, by Mr. J. Maclear. At a high temperature with excess of carbon, sodium sulphide and carbonic oxide are formed. At a dull red heat sodium carbonate and carbonic acid are produced in addition.—On the action of ethylchlorcarbonate on some oxygenated haloid compounds of the fatty series, by Mr. O'Neil F. Kelly. The compounds employed were allyl alcoholdibromide glycerindichlorhydrin and epichlorhydrin.—The Society adjourned over the recess.

PARIS

Academy of Sciences, July 8.—M. Fizeau in the chair.—The following among other papers were read:—Action of heat on aldol, by M. Wurtz. He obtains, beside crotonic aldehyde, a little ordinary aldehyde, and, in certain circumstances, a new polymer of the aldehyde, which he describes.—On malignant pustule in fowls, by MM. Pasteur, Joubert, and Chamberland. Fowls when cooled contract it easily, and they may then be completely cured by reheating.—Influence of atmospheric electricity on the nutrition of plants, by M. Grandeau. His mode of experiment was to place two plants of the same species (tobacco, maize, wheat) under the same conditions as to soil, aëration, isolation, &c., but the one withdrawn from the action of atmospheric electricity by means of a Faraday's cage. The plants thus withdrawn elaborated, in equal times, 50 to 60 per cent. less of living matters than the others. Plants of small elevation above the ground are also affected by atmospheric electricity. The centesimal amount of proteic matter formed appears not to depend sensibly on this action; it is proportional to the yield. The proportion of ash is higher in plants removed from the electricity; and the proportion of water is less.—On the curves of solubility of salicylic and benzoic acids, by M. Bourgoin. Taking the temperatures for abscissæ, and the quantities dissolved for ordinates, the solubility of salicylic acid in water is represented by a parabolic curve, whose convexity is towards the axis of temperatures.—On the diffusion of fire-damp in mines, by M. Coquillion. The experiments show that it diffuses very slowly from above, but rapidly upwards.—On a disease of malignant pustule form, caused by a new aerobic vibrion, by M. Toussaint. He found this vibrion in a rabbit inoculated from the blood of a horse which had died rapidly with symptoms of malignant pustule.—On *Avenardia Priei*, a giant Nemertian of the west coast of France, by M. Giard. In the state of rest it measures 1 m. to 1·20 m. in length (in extension twice or thrice as long), the width being 2 to 3 ctm. It is found in hundreds in an old canal from salt marshes at Pouliguen, now transformed into a reservoir, where the sea-water is renewed each tide.—Observations and experiments on the migrations of *Filaria rytipleurites*, a parasite of cockroaches and rats, by M. Galet. The eggs produced by the parasite in the alimentary canal of the rat are thrown out with fecal matters, and swallowed by the cockroach. The embryos, when hatched, penetrate the walls of the alimentary canal of the latter, and are encysted in fatty matter, where they await the cockroach being devoured by the rat. In the rat they now complete their cycle.—Experimental researches on the variations of volume of the cranium, and on the applications of the graphic method to solution of various anthropological problems, by M. Le Bon. A superior race contains more of voluminous crania than an inferior. Among 100 modern Parisian heads there are about eleven with a cranium of 1,700 to 1,900 cubic centimetres; in the same number of negro heads not one will be found of such size. The weight of 100 masculine Parisian brains of the present varies between 1,000 and 1,700 grammes, the volume between 1,300 and 1,900 cubic centimetres. The difference between the largest and smallest brains among modern Parisians is three times that observed in the negro, and it is greater than in the Parisians' ancestors of 600 years ago. Stature has only a very slight influence on the volume of the brain. With equal stature woman has a much less heavy brain than man. Rising in the scale of civilisation, the difference in weight of brain (and so volume of cranium) between man and woman is found constantly increasing; thus the average difference of crania of the present Parisian men and women is nearly double that between the crania of the ancient Egyptian men and women. Persons having the same circumference of crania may have differences in volume of over 200 cubic centimetres; but operating on series, 1 centimetre increase of circumference corresponds to an increase of about 100 cubic centimetres in volume. Certain relations are found to exist between circumference of cranium and head, and volume and weight of brain. The cranium is always unequally developed on the two sides, without apparent relation to race or intelligence.—Automatic imitation of mountain chains on a globe according to the theory of upheaval, by M. Chancourtois. This is by the method of a caoutchouc balloon covered with wax, then allowed to contract.—Determination of the orbit of the planet 103, Hera, by M. Leveau.—On the development of the cephalo-thoracic portion of the embryo of vertebrates, by M. Cadiat.

CONTENTS

ERRATA.—Vol. xviii. p. 294, 2nd column, line 22 from top, read for "insolvents" "resolvents;" line 31, for "2m" read "2^m." In last week's "Paris," parenthesis near beginning, for "distillation (which discoloured the fatty acids)" read "distillation (which had been resorted to to purify the fatty acids)."

THURSDAY, JULY 25, 1878

BOTANY IN AMERICA

Synoptical Flora of North America. By Asa Gray, LL.D., Fisher Professor of Natural History (Botany) in Harvard University. Vol. ii. Part 1. (New York, 1878.)

Bibliographical Index to North American Botany, with a Chronological Arrangement of the Synonymy. By Sereno Watson. Part 1, Polypetalæ. (Washington, 1878.)

As with the British nation, so with the American, the rapid extension of its territorial boundaries, and absorption of outlying regions abounding in new and interesting forms of life, has determined to a great extent the direction and progress of the natural history sciences which it cultivates.

The first requisite of the explorer who brings back with him collections illustrative of the new or little-known countries he has visited is to know what these are; and the emigrant and colonist who follows the explorer makes the further demand upon the naturalist of the means of ascertaining the names and relationships of the useful, ornamental, noxious, or otherwise remarkable plants of his new home.

Hence the more rapid strides of classificatory and descriptive botany in England and America than in any other countries, the multiplicity of botanical appendices to narratives of voyages, travels, surveys, and explorations; and those libraries of local and general floras which have contributed so largely to our knowledge of the vegetable world. In this respect, namely, the accession of new domains, America and England have no rival but Russia, and in her case the newly-acquired or explored territories have in many cases, from the scantiness of their floras, yielded comparatively little of botanical interest. France, indeed, during the last decade of the past and first of the present centuries, displayed remarkable activity in this direction, stimulated thereto by the genius and activity of Richard, De Candolle, St. Hilaire, and many other botanists, and by the accumulation of collections brought by her naval expeditions and geographical explorations; but latterly she has left the field to the Anglo-Saxon; and her publications relating to the botany of her later voyages are too often ambitious failures, *ouvrages de luxe;* abandoned and left incomplete after the first few parts have dazzled the scientific world by their size and beautifully executed plates; whilst of her latest and most splendid territorial acquisition, Algeria, we must go to the work of an Englishman (Munby) for even a catalogue of its botanical riches.

America, on the other hand has gone about her work of this kind in an eminently practical fashion. Most of her local floras, and they are very numerous, are exceedingly well done, and complete up to their date of publication, whilst the botanical collections made during her almost innumerable topographical, geological, boundary, and railroad surveys have been published with a completeness and accuracy that leave little to be desired; and these hence form most valuable contributions not only to systematic but to geographical botany.

Of the contributors to this branch of botanical literature in America none is entitled to take rank with Prof. Asa Gray, of Harvard, Massachusetts, whether in respect of the amount of labour undertaken, or the treatment of it; and there is certainly no living botanist whose work has better stood the test of time and subsequent scrutiny, or who has turned his materials to better account in dealing with problems of classification and geographical distribution.

One circumstance alone has stood in the way of the recognition of the merits of this class of work, which is the fragmentary character of most of it, and the fact that so much of it has appeared as appendices and supplements to ponderous volumes descriptive of territorial and other surveys, which have necessarily a limited circulation, and whose titles are unfamiliar to scientific inquirers.

It has long been felt that there was but one way of satisfactorily dealing with the mass of materials of this nature that had been accumulating in the United States for now thirty-five years, and this was the resumption by the surviving author of Torrey and Gray's "Flora of North America," which was begun in 1838, and of which the last part, embracing the Compositæ, was published in 1843. Happily the surviving author has not been allowed to lose sight of the fact, that to him alone would botanists look with confidence for its satisfactory completion, and he has consequently been in the habit of so working up the materials confided to him for publication from the numerous expeditions alluded to and from private sources, that these should be directly available for the final effort to complete the American flora.

In commencing this, two courses were open to Dr. Gray: to re-edit, with additions, the antiquated volumes already published and continue onwards, or to begin where the old work ended, and re-edit afterwards the previously published ones. Dr. Gray has chosen the latter course, and, we doubt not, wisely; it leaves the lighter task for the future. It also more quickly meets the necessary wants of botanists, for the first volumes are, to a very important extent, supplemented by Sereno Watson's "Bibliographical Index to North American Botany," which gives references to all the genera and species added to the American flora since 1843, as well to those published up to that time.

In various other respects, also, the model of the old flora of North America has been advantageously departed from; a new title, of "Synoptical Flora of North America," has been adopted; the pages are larger and the type smaller (though still beautifully clear); synoptical tables of the genera are introduced under each order; and the synonomy and references have been greatly reduced, these being supplied by Watson's above-mentioned bibliography.

The work embraces all the regions of America belonging to the United States and Great Britain, thus excluding Greenland, the flora of which is more European than American. The part now published contains 400 pages, and embraces thirty-two Gamopetalous orders (from Goodeniaceæ to the end of that division of Dicotyledons) according to the sequence of Bentham and Hooker's "Genera Plantarum," the limitation of orders in which work is closely followed. The orders include 321 genera and

1,663 species, according to a statement in the Bulletin of the Torrey Botanical Club of New York (May, 1878), which contains a justly appreciative notice of the work. The preface states that the whole will be completed in two volumes of 1,200 pages each, which, if the forthcoming parts contain as many species as that now before us, would imply that the known phænogamic floras of the area embraced consists of about 10,000 species, a rather larger number than is included in Nyman's "Sylloge Floræ Europeæ," published in 1855, which contains 9,738.

If this latter number represents even approximately the extent of the European Flora, it shows that Europe contains, in proportion to its area and latitude, more plants than temperate North America: for its area (3,600,000 square miles) but little exceeds that of North America, exclusive of the vast British possessions, whilst the United States extends almost into the tropics and contains many subtropical plants. This superior botanical richness of Europe is no doubt due to the great diversity of the floras of its three southern peninsulas, Spain, Italy, and Greece, and to the number of species in its central mountains; as also to the prevalence of annuals, in which Europe far outnumbers any other continent. On the other hand, Dr. Gray's views of the limitation of species are no doubt large, compared with that adopted by Dr. Nyman.

Before concluding this notice of Dr. Gray's work, we may notice the identification of the North American *Solanum Fendleri*, Gray, with the potato (*S. tuberosum*) as var. *boreale*, thus giving this esculent a range from Chili and Buenos Ayres to the United States.

Mr. Watson's "Bibliographical Index to North American Botany" is not a mere compilation, as its title would make it appear, but a first-rate contribution to botanical science; the authorities cited being in all possible cases verified by a reference to the works themselves, and often by a critical study of the specimens by a botanist of scrupulous exactness, who is also well acquainted with the North American Flora, as collaborator with Asa Gray in the herbarium at Harvard. The first part, now published, includes the Polypetalous Dicotyledons, thus covering the ground of the first volume of Torrey and Gray's "Flora of North America," published in 1838–40. Next to Gray's Synoptical Flora this is the greatest boon to systematists that has appeared in North America for many a year, and it is further a necessary adjunct to the Synoptical Flora, containing as it does a mass of citations of descriptions and plates, of which that work is thus relieved.

It is further very interesting as showing the additions made to North American Botany since the publication of Torrey and Gray's above-mentioned first volume. Taking ten of the most considerable American orders of Polypetalæ, we find that in 1840 these included 262 genera and 1,267 species, to which have been added in the thirty-seven years that have elapsed since that period, only 12 genera, but 756 species. In so far as this affords the means of forming a rough estimate, it shows that the discoveries during the period in question, and which are almost confined to the southern and western States, have added about one-third to the previously known North American flora. These additions affect the different orders very variously, as might be expected; thus, whilst the increments to some, as *Ranunculaceæ* (20) and *Caryophylleæ* (16) are small, and of others moderate, as *Onagrariæ* (24), *Rosaceæ* (28), and *Saxifrageæ* (10), those of *Umbelliferæ* (45) are large; the orders *Cruciferæ* (95) and *Onagrariæ* (47) have been nearly doubled, *Leguminosæ* (360) more than doubled; and to *Cacti*, of which but nine species were known at the first period, 111 have since been added!

In two comparatively unimportant matters of convenience the author has departed from usage, and, we think, without good cause; the genera and species are not numbered either in Gray's or Watson's work, on what grounds we cannot imagine. To have to run up the numbers of the large genera (as Astragalus), for the purpose of comparing or contrasting the items of the American flora with those of others, involves a grievous loss of time to the botanist, whilst for the arrangement of herbaria, and ready reference in it to these standard books, the numbering would have been very useful. The other departure is the alphabetical arrangement of the genera under the orders in the "Bibliography," which, in the case of genera absorbed, necessitates a reference to the index.

If a few copies, with the matter printed on one side of the page, could have been obtained by botanists, it would have been a great boon. In such a case the pages of the "Bibliography" might have been intercalated with those of Gray's "Synoptical Flora;" and each species, with its references, might have been cut out and attached to the herbarium specimens of the species.

It remains to wish these able and industrious authors successfully through the works they have so well begun.

J. D. HOOKER

THE HYDROIDS OF THE GULF STREAM

Report on the Hydroida collected during the Exploration of the Gulf Stream by L. F. de Pourtalès. By G. J. Allman, M.D., F.R.S., President of the Linnean Society. (Cambridge, U.S., 1877.)

THIS report forms No. 2, vol. ii. of the quarto memoirs of the Museum of Comparative Zoology at Harvard College, and has been published by permission of the Superintendent of the United States Coast Survey, and is illustrated by thirty-four plates. It forms one of the most important contributions to the natural history of the hydroids that have appeared of late years, and it describes a very large number of most interesting and new forms. One of the sub-orders of the hydroida is well characterised by having the hydroids quite unprotected—not covered by any external protective receptacle. In this sub-order (Gymnoblastea) but nine species were found in Pourtalès' collection. Although they are all referable to known genera, yet all of them are new. The great bulk of the collection belonged, however, to another sub-order, in which the hydroids are provided with external protective receptacles (hydrotheca), and which is more or less familiar to us as containing the Campanularian and Sertularian forms. Of this sub-order (Calyptoblastea) some fifty-six species are described, and among the Sertularinæ no less than seven new genera are described, with forty-two new species out of a total of forty-three. Among the Campanularinæ two

new genera are described, with twelve new species out of a total of thirteen. In addition to these new species there were but seven others which, so far as their identification was possible, were already known as European forms. The collection had been preserved in spirits and was for the most part in excellent preservation. It would seem obvious that the region from which this collection had been obtained, and which includes an area between the Florida Reef on the north and west and Cuba the Salt Key and Bahama Banks on the south and east, is characterised by a very distinct hydroid fauna, and must form part of a special province in the geographical distribution of the Hydroida, though of course further researches may greatly extend the area of the new forms. The greatest depths at which any species had been dredged was 470 fathoms. The collection was rich in the plume-like hydroids (Plumulariadæ). In some genera of this beautiful family of hydroids the ultimate generative zooids which give origin to either the germ or the sperm cells (gonophores) are developed within a horny covering (gonangium), groups of which are often to be found inclosed in most curious basket-like receptacles (corbula). Such basket-like receptacles are well seen in Aglaophenia, and in a new species described as *A. bispinosa*, they attain a very large size and form most beautiful objects. In this genus the twigs of the cradle are much altered pinnæ which are pressed into a protective service. In one of Dr. Allman's new genera nearly allied to Aglaophenia (Cladocarpus) the groups of gonangia are not inclosed in corbulæ, but are borne on the sides or at the base of special protective branches which are not altered pinnæ but appendages to them. They certainly seem to act the part of corbulæ, and to form very effective organs of defence to the gonangia—though they do not present so effectual a covering as in Aglaophenia. In one magnificent species (*Cladocarpus paradisea*) the stem of which attains a height of fourteen inches, these phylactogonia, as Dr. Allman calls them, are in the form of pinnately-branched offshoots. In another species (*Cladocarpus dolichotheca*) the stem for nearly the whole of its course carries a longitudinal series of tubular receptacles which contain sarcode in which thread-cells are found. These nematophores, which are situated at short and equal intervals from one another, give to this part of the hydroid colony a very close resemblance to certain forms of graptolites. Elsewhere Dr. Allman has called attention to the close affinity that appears to exist between the so-called denticles of the graptolites and the nematophores in these Plumularian hydroids; and, as we know, that these bodies in the hydroids are not only filled with protoplasm, and that this is capable of developing pseudopodia after the manner of some rhizopods, this might seem to point to a relationship between the graptolites and the rhizopods through some ancestral form in which the affinities looked on the one side to the hydroids and on the other to the rhizopods, the graptolites having stopped short of the progress which the hydroids were enabled to make.

The beautiful plates illustrating this memoir have been executed by the faithful pencil of Mr. Hollick, though the details of structure have been drawn by the skilled and practised hand of the author. E. P. W.

OUR BOOK SHELF

Automatic Arithmetic: a New System for Multiplication and Division without Mental Labour and without the Use of Logarithms. By John Sawyer, Public Accountant. (London: George Bell and Sons, 1878.)

THIS is an ingenious work, and would have suited the "fantastical" Armado ("I am ill at reckoning; it fitteth the spirit of a tapster"). By the aid of 1759 figures only the user of it is able to multiply any two numbers each not exceeding 999,999,999, and also to divide one number by another, neither exceeding the same limits.

Its advantage over logarithms, in the cases to which it applies, is that the results are accurate instead of approximate.

The principle of construction is very simple: take ten pages; at the top of each write the nine digits in succession (for facility in working, the numbers are printed red and black alternately); cut each page into nine horizontal sections, and at the end of each strip write the same number (viz., for the fifth page the constant number will be 4), then on each strip will be printed

04 08 12 16 20 . . . 36

the printing being so arranged that each strip shall have the numbers of the previous strip printed one place further to the right. From this description it will, we think, be readily seen how to apply the tables to multiplication. To take a very simple example (any other example coming within the limits we have specified can be worked with almost equal facility), multiply 374 by 7.

Take the larger of the two numbers; turn back all the top row slips above 3, then all the second row slips above 7, then all the third row slips above 4, then carry the eye down the vertical column corresponding to the number, and we see (in this case in black figures)—

```
21
 49
  28
```

whence, adding, the result is seen to be 2618.

Each additional figure in the multiplier gives a result similarly obtained, and the answer is got by no mental operation more difficult than simple addition. A mere child who can add numbers together can thus perform mechanically (or automatically) difficult exercises in multiplication. Division is similarly reduced to subtraction. Whether much saving of time and labour is effected is a question we leave to practical men. We showed the work to a class of boys, and they were much interested in watching the process, the principle of which was readily understood by them.

Numbers can thus be easily raised to powers, but the method does not serve for the extraction of roots. We need hardly state that the tables are protected by letters patent.

A Handbook of the Plants of Tasmania. By the Rev. W. W. Spicer, M A., &c. (Hobart Town: J. Walch and Sons, 1878.)

THIS little book of 160 pages contains a great deal more valuable matter than would at first sight appear. The author justifies the production of his book from the fact that previously existing works treating of Tasmanian plants are though "works of extraordinary merit, costly and ponderous," in proof of which he points to the labours of Hooker, Mueller, and Bentham, all of whose works are undeniably of very great value, but if for no other reason than their bulk, quite unfitted to be the companion of a botanical ramble. The book before us is intended for this purpose, being convenient in size, and as the author tells us in his preface, "moderate in cost." With regard to the plan of the book, the author's own description will make it more clear than any words of our own. He says,

"the descriptions are arranged on the branched or binary system, first established by the French naturalist Lamarc. Under this system, a series of salient characteristics is laid before the reader in pairs, the numbers of each pair being as nearly as possible opposed in their terms, and each giving rise to a new pair in like manner contradictory. The choice of these contradictions being left to the reader, he selects the number which applies most nearly to his specimen, and then passes on to the next pair. It is evident that, sooner or later, the several series of characters must be exhausted, and the name of the plant arrived at." The method of using this system is so fully explained further on, that, by following the author, no one can possibly fail to understand it and to be able to identify any plant by its aid. It is, in short, an exhaustive system by which the plant we may be examining is, so to speak, run into a corner and so fixed in its proper place. Thus we have a pocket flora of the colony in which not only the scientific, but, in most cases, also the common or colonial names are given. A short glossary of botanical terms, illustrated by figures, is placed at the beginning of the book, but this includes only such words as it was found absolutely necessary to use in the book. The aim of the author in assisting to popularise a knowledge of Tasmanian plants amongst the colonists will, no doubt, be furthered by the appearance of this little volume. A more careful revision of the proof-sheets, however, would have repaid for the extra time so spent. We think, also, that the adoption of some recognised system of spelling the natural orders would have had its advantages. Thus, we find Ranunculeæ instead of Ranunculaceæ, Lobeleæ instead of Lobeliaceæ, while, on the other hand, Papaveraceæ, Scrophulariaceæ, Lauraceæ, and others, occur as we have written them.

The coloured frontispiece of the Waratah (*Telopea truncata*) is, to say the least, a poor attempt at plant-figuring, both the drawing and colouring being equally bad.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

The Natural History Collections

I SEE by *Galignani*, the only newspaper that reaches this high elevation, that a bill has just been introduced into Parliament by Mr. Walpole, and read a second time, to enable the Trustees of the British Museum to move the natural history collections into the new building at South Kensington. Not having before me a copy of the Bill, I cannot say whether it contains any clauses to alter the present mode of government of the natural history collections, but if such be not the case, and it is proposed to leave the new institution at South Kensington under the domination of the Trustees of the British Museum, I can assure Mr. Walpole and his friends that they will cause bitter disappointment to the naturalists of the country by such a course of action. We have always looked forward to the epoch of the removal of the national collections of natural history to the new site as the only opportunity ever likely to arise of making a reform in their government. That a Board of Trustees consisting of the principal Officers of State and great nobles of this country could be abolished was, of course, impossible, but it was hoped that the great men of Bloomsbury would not care to extend their authority to South Kensington. It will not be forgotten that the Royal Commissioners on Science, who went into this question in full detail, came exactly to this conclusion, and recommended that the new museum, when constituted, should be placed under a director immediately responsible to one of the ministers. And there can be no doubt that this should be done, and that the Act which authorises the removal of the collections should give them their new constitution. Our two best scientific institutions in this country—Greenwich Observatory and Kew Gardens—are governed after this fashion, and could never have attained their present standard of excellence under the rule of fifty irresponsible trustees.

Let me briefly point out the evils of the present system of government of the British Museum as regards the natural history collections. The "principal librarian" is secretary to the trustees and sole executive officer. His policy is, naturally enough, to get all he can for his books, and to keep the expenditure on the natural history branches as low as possible. One glance at the estimates annually proposed for the various departments in the British Museum will be sufficient to show how well this policy is carried out. It may be that the trustees will ask to appoint a new secretary and executive officer for the new building at South Kensington, and that the estimates for the two institutions will be kept separate. I trust that such may be the case, as it will partially mitigate the evils of which I complain. But I much fear that the principal librarian will resist giving up any part of his present authority, and that the tendency to starve the natural history and pamper the library will remain as heretofore. I earnestly hope, therefore, that Mr. Walpole's bill will not pass into law unless it contain clauses to ensure a cessation of the disadvantages of the present system of government. It would be better far that it should not pass at all this session. Even the fabric of the new building will not be complete until next spring, and there is much to be done in the way of internal arrangements and fittings before the time comes to remove the collections. Why, therefore, should a bill of this importance be introduced and hurried through Parliament at the fag-end of the present session? It must be naturally supposed that the only object is to elude criticism and to keep the rights and privileges of the fifty trustees as far as possible inviolate.

A NATURALIST

Hotel de la Furca, Canton Uri, Switzerland,
July 15

The Genesis of Cyclones

IT is to be gathered from Mr. Barham's communication on cyclones and anti-cyclones in NATURE, vol. xviii. p. 249, that he is probably unacquainted with either Prof. Dove's partial explanation of the effect of the earth's rotation on the winds, published nearly fifty years ago, or Prof. Ferrel's more recent and comprehensive memoirs on the same subject; not to speak of Mohn and Guldberg's elaborate discussion of the mechanics of cyclonic and anticyclonic movements in their "Études sur les Mouvements de l'Atmosphère," published within the last two years.

It is not, however, to point out the fact that Mr. Barham's idea has been anticipated by these, and indeed many other writers, that I address you; but rather to show that the theory of parallel currents, which Mr. Meldrum, among others, has appealed to to account for the formation of cyclonic storms, is, taken by itself, inadequate; since any circular movements generated, in the manner supposed, by opposite currents, cannot receive from them a greater velocity than the mean velocity of the generating currents; and the theory leaves unexplained the spiral indraught of the air, which both Mr. Meldrum's charts of cyclones in the South Indian Ocean, and those which I myself, the late Mr. Willson, and Mr. Eliot, have prepared of storms in the Bay of Bengal, show to be an invariable as it is probably an essential feature of all such storms.[1]

[1] Mr. Piddington is usually regarded as an upholder of the hypothesis of the truly circular or tangential movement of the winds in cyclones, and not without reason; since, although he admitted, as a possibility, an occasional spiral indraught, he regarded this as non-essential, and equally admitted that the winds may occasionally blow outwards from the tangential direction. In most of his charts he does not give all the observed wind directions, but in that accompanying his first memoir, on the storm of June 3–5, 1839, he has done so. If any one will refer to this chart, and, leaving out of consideration Mr. Piddington's hypothetical circles, will draw for himself the most probable course of the currents in accordance with the charted wind arrows, he will soon convince himself that this course is sharply spiral, and also that Mr. Piddington, probably influenced by the erroneous hypothesis of circular rotation, has misapprehended the several positions of the storm-centre, and has assigned to the storm a course quite at variance with its real course, and equally so with that of any storm since observed in the same sea. The case is an interesting one, because Prof. Dove, misled by Mr. Piddington's error, has made this storm the basis of an erroneous explanation of the origin of the storm in his well-known work on "The Law of Storms," which has been translated by Gen. Sabine. Of course, in pointing out an error, I would in no way seek to disparage the admirable and laborious work of Mr. Piddington, whose great merits I most willingly recognise.

The storms generated in the Bay of Bengal afford unusual facilities for studying the genesis of cyclones. We have observatories round three sides of the bay, and the sea is, at all times of the year, traversed in all directions by numerous steamers and sailing vessels, which have furnished abundant logs. Did parallel currents play any important part in the production of the vortices, they could not possibly escape our notice. But we find that the antecedent conditions of a cyclone are light, variable winds and calms, with a nearly uniform barometric pressure all round the coasts; and only to the south, in the neighbourhood of the equator, is there any considerable movement of the air, viz., from the west. Under these circumstances, the pressure falls over some part of the bay; most frequently in the middle, and especially to the west of the Andamans. This region of falling pressure is characterised by torrential rains, with, at first, but little wind; but after a day or two (sometimes several days) of this weather, a cyclonic circulation is set up, with a marked indraught in the neighbourhood of the cyclone cradle, and thus the storm is generated.

Having regard to these facts of observation, it appears to me that it is in the condensation of the heavy rain (constantly noted as "torrential" in ships' logs) over the cyclone cradle, that we have the real source of the energy of the incipient storm. The hypothesis of parallel currents fails to provide this energy; for it is obvious that the deviation of the winds under the influence of the earth's rotation can furnish no energy, and can produce only a moderate barometric depression, the amount of which depends on the velocity of the original winds, and can be calculated by Ferrel's law. When this is reached, the system of pressures and wind-movements will be in equilibrium. If (and this I am not prepared to deny) a cyclone is sometimes generated between parallel currents, it must be that the energy of the storm is supplied from some other source, and what this is, is, I think, clearly indicated by the case of the Bay of Bengal storms.

It was first noticed by Mr. Eliot as a general fact, that, during the formation of a cyclone in the Bay of Bengal, little or no rain falls on the coasts; while, as already remarked, it is exceedingly heavy over the place of the storm's origin. The vapour generated over the bay, which, under other circumstances would be carried away by the winds and condensed over the land, is then condensed over the bay itself. The quantity of latent heat thus set free is enormous; and as Reye has shown, is ample for the production of the most violent cyclone. It would be erroneous to say that the air is thereby warmed and expanded, because, of course, the very fact of its vapour being condensed proves that it must be cooling; but Welsh's and Glaisher's balloon observations show that in a cloud-laden atmosphere, the vertical decrement of temperature is slow, as compared with that in a clear atmosphere; and the same fact is further illustrated by the temperature of hill-stations in the wetter parts of the Himalaya as compared with that of the plains at their foot. At Darjiling, for instance, the temperature from June to August (the season of greatest cloudiness and heaviest rainfall) is only 17° or 18° below that of Goalpara. In February and March (the driest months) it is between 23° and 24°. The explanation of these facts is afforded by the different rates of cooling experienced by saturated and unsaturated air, respectively, in an ascending mass of air which is expanding under a constantly diminishing pressure. Saturated, *i.e.*, cloud-laden and rain-condensing air at 80° cools only 20° by the work done during its ascent from the plane of 30 inches pressure to that of 20 inches pressure, say through 10,000 feet; whereas unsaturated air cools about 54° in the same ascent, the exact amount varying slightly according to the quantity of vapour it contains. The latent heat set free in the condensation of cloud and rain is then entirely used up in the work of expanding the cloud-laden air under a constantly diminishing pressure, and economises more than half (indeed, in the case adduced, nearly two-thirds) of the sensible heat which furnishes the energy to unsaturated air. Hence, an ascensional current, however small, once set in action in a nearly saturated atmosphere, such as exists over the Bay of Bengal during the formation of a cyclone, carries warm air to a greater height than in the clearer and drier atmosphere around the coasts, relatively raising the mean temperature of the former air-column, and of course reducing its weight. This differential effect goes on increasing, and the ascending current becomes more rapid, until the indraught below attains the conditions of a cyclonic storm.

Now, in the case of parallel currents, there must be between them a region of calm; and, if this is over a sea of high temperature, it is conceivable that, as in the Bay of Bengal, local condensation may proceed for a sufficient time unchecked to lead up to the formation of a cyclone; but, in that case, the cyclone will be generated, not immediately, as supposed by Mr. Barham, by the energy of the pre-existing winds, but by their affording conditions in which another and far more potent source of energy comes into play. H. F. Blanford

Dinard, France, July 10

The Tasimeter and Magnetisation

After perusing an account, in a recent number of the *Scientific American*, of Edison's Tasimeter, it occurred to one of us to apply it to detect, and, if possible, to measure the elongation and shortening, which, as discovered by Joule, are produced in a bar of iron by magnetisation and demagnetisation. Accordingly to test whether the effect could be observed in this way, a rough specimen of the instrument was constructed, and with it some preliminary experiments made, an account of which may interest the readers of Nature. A small cylinder, about half a centimetre in length and diameter, of the carbon used for Bunsen's cells, rested with its ends which were slightly rounded, in contact with two brass plates, one of which was fixed to a rigid upright attached to one end of the base of the instrument, while the other, resting with one end on the base, formed a spring, which in its normal position just touched the end of the carbon. A coil containing four layers of insulated wire, six turns to the layer, was wound round a tube ten centimetres long and eight millimetres in diameter, and fixed with its axis in line with that of the carbon cylinder. A piece of iron wire was then placed in the axis of the tube with one end resting against the spring, and the other in contact with the end of a screw working in a nut fixed to a rigid upright at the end of the base remote from the carbon. By means of this screw the pressure of the iron bar on the spring, and consequently of the spring on the carbon, could be varied at pleasure.

A terminal of copper wire, was attached to each of the brass plates bearing on the carbon, and joined up so that the carbon, plates, and terminals formed one of the resistances of a Wheatstone's bridge, in connection with which a battery of one Daniell's cell and a very delicate Thomson's reflecting galvanometer were used. When the iron wire forming the core of the electro-magnet had been so adjusted that there was only a very slight pressure on the carbon, the resistances of the bridge were arranged to make the deflection of the galvanometer produced by the current from the battery nearly zero. The galvanometer and battery keys were then put down and the current allowed to flow through the bridge during the remainder of the experiment. The electro-magnet was then excited by the current from three of Thomson's Tray Daniells. This produced a deflection of the image on the galvanometer scale of about fifty divisions in the direction indicating a diminution of the carbon resistance, which must have been caused by change of contact produced by increased pressure on the spring. The length of the iron core of the electro-magnet had therefore been increased by magnetisation. When the magnetising force was removed the image immediately returned to its former position. As a verification that the diminution of resistance indicated by the bridge arrangement was caused by elongation of the iron core, the adjusting screw was turned forward through a very small distance, when the deflection was found to be in the same direction as before. When the screw was brought back the image on the scale returned towards its zero. Experiments with various strengths of current gave perfectly accordant results.

We hope by replacing the comparatively rough adjusting screw by a micrometer screw to be able to make some measurements of the exact amounts of elongation or shortening produced in a piece of soft iron or steel by given changes of magnetic intensity. It may be remarked that this method of measurement could be advantageously applied in cases where the amount of change of dimensions to be discovered or measured is very small, but the force which it could be arranged to produce abundant.

University of Glasgow, July 12 Andrew Gray
Thomas Gray

Physical Science for Artists

The curious phenomenon described by Prof. Brücke and Mr. Norman Lockyer, under the name of "les rayons de crépuscule," though rare and uncommon in the island of Ceylon, is

well-known to the natives under the title "Buddha's rays." It has also, I believe, been noticed in Cashmere. The phenomenon, which is very striking indeed under favourable conditions, is confined to the mountain region in the central parts of the above-named island, and is never, as far as I am aware, seen in the low country. It was therefore with considerable interest that I learnt that it is well known to the French, and had been seen by Mr. Lockyer at sea. In May, 1876, in a paper on "Remarkable Atmospheric Phenomena in Ceylon," read before the Physical Society and published in the *Proceedings*, I offered an explanation of this phenomenon, in accordance with the conditions under which it appeared. As this explanation is very brief, may I ask your permission to reproduce it *verbatim?*

"Not unfrequently in the mountain districts broad beams apparently of bluish light may be seen extending from the zenith downwards, converging and narrowing as they approach the horizon. This ray-like appearance is very similar to that seen before sunrise; only the point from which the rays proceed is *opposite* the sun: the rays themselves are very broad and blue in colour; and the spaces between them have the ordinary illumination of the rest of the sky. If we suppose in this instance that the lower strata of air are colder than the upper (a condition of the atmosphere which not unfrequently occurs in a tropical mountain district like that of Ceylon, where large currents of heated air sweeping up a valley cross another valley nearly at right angles and at a considerable elevation above it), the refraction spoken of in the case of Adam's Peak will be downwards instead of upwards. If, too, the observer be *below*, the veil of darkness will appear to him like a very elongated triangle, apex downwards, or broad ray, through which the blue sky beyond may be seen free from the palish illumination of the atmosphere, whilst on either side the ordinary illuminated sky will be seen. If now we suppose several isolated masses of cloud to partially obscure the sun, as was the case when I witnessed the phenomenon, we may have several corresponding inverted veils of darkness, like blue rays in the sky, all apparently converging towards the same point below the horizon. This apparent convergence of the beams is merely an effect of distance, as in the case of parallel rays of light from the rising or setting sun, the blue rays being practically parallel bands in the atmosphere devoid of illumination. It will be evident that conical-shaped clouds are not necessary to produce this effect. Isolated clouds of any massive form would be sufficient to throw the bands of shadow through the illuminated atmosphere, and refraction and perspective would do the rest. The above phenomenon is called by the Singhalese 'Buddha's rays;' and though, according to Sir Emerson Tennent, it is very varied in character and appears in different parts of the sky, yet I have only seen it when the sun was low at evening and when the rays converged to a point, apparently directly opposite the sun; and I do not think it possible for the phenomenon to be seen in any other position."

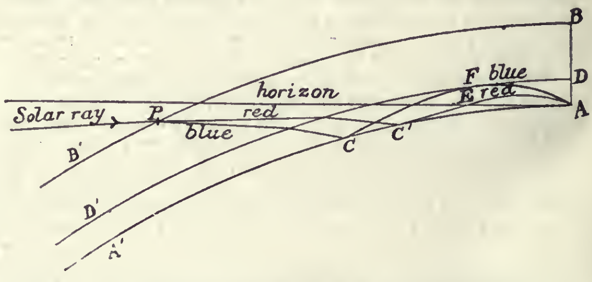
It will be seen that I thought it necessary to suppose a peculiar state of the atmosphere, in order that the shadows themselves might be sharply refracted downwards; indeed, I think, if the phenomenon can be produced under ordinary conditions, it would be much less rare in the island than it is. It may be mentioned that the sharpness and definition of the rays was most decided—far more so than the sunbeams and shadows are at sunrise or sunset—and that the position from which I observed the phenomenon was a ridge 4,500 feet high, overlooking a deep valley lying north and south, into which another valley running east and west opened at a high elevation, so that the warm air coming up the latter from the country below would overlie the cooler air of the first valley. I am not certain whether mountains a few miles to the west had any part in the production of the phenomenon, but the sun was certainly setting behind dense but isolated masses of cloud. With the above exception, it appears that my explanation is identical with that of Mr. Norman Lockyer and Prof. Brücke.

The reference in the above quotation to Adam's Peak is to the shadow which this isolated and cone-shaped mountain throws up into the sky at sunrise. The summit of this peak is 7,200 feet high, and overlooks the low country for fifty miles or more to the west. The shadow thrown by it at the moment of sunrise appears to lie horizontally over the land and sea for a distance of seventy or eighty miles. As the sun rises, the shadow rises and takes the form of a gigantic pyramid rising in the air and approaching the mountain, until at last it appears to fall over on the summit. The explanation which I offered of this was in principle identical with that of the mirage, the rays grazing the summit and sides of the mountain being refracted upwards, and thus leaving a portion of the atmosphere, similar in shape to the upper part of the Peak, unilluminated. This refraction is extremely likely to occur, as the difference in temperature between the air in the low country and on the top of the Peak is at least from 30° to 50° F. during the night and morning. The sudden vanishing of the shadow is, no doubt, due to the rays reaching the critical angle when the sun has risen to a certain height, and total internal reflection ceasing to take place. It may be mentioned that in both cases the shadows appear to be blue, but this, I imagine, is only the effect of contrast. This theory will, I believe, also explain the phenomenon of the spectre of the Brocken, which appears, I understand, under similar but less pronounced conditions.

The above appear to be very good instances of the illumination which the particles of air are capable of receiving, for it is evident that neither phenomenon would be visible if the atmosphere were perfectly pure.

Another phenomenon I shall be glad to refer to, as bearing on the subject of a perfectly clear sky at sunset. During the dry season on the West Coast of Ceylon, when the air is so free from moisture that even in houses close by the sea-shore the backs of books and sheets of paper curl up as if they had been placed before a fire, I have noticed, in looking westward over the sea immediately after sunset, that the sky has presented the appearance of an almost perfect spectrum extending from the horizon upwards for a very considerable distance towards the zenith (as far as I remember to within 20° of the zenith), a fiery red being lowest, succeeded by orange, yellow, green, and blue. With the exception of the red the colours were of the most transparent character possible; such as it seemed to me at the time it would be quite impossible to reproduce except with the prism or or diffraction lines.

The explanation which I would offer of this seems to bea directly on the question of the colour of the sky, and as it has not



I believe, been suggested, I shall be glad if you will allow it to appear. It may, however, be an old explanation, but I am not aware that it is. When a ray of sunlight passes obliquely through a thick plate of glass the issuing ray is found to have been dispersed during its passage, although on account of the overlapping of the spectra produced by the pencil it appears to the eye as white light, one edge where there is no overlapping being slightly red, and the other slightly blue. Now the atmosphere of the earth is such a parallel plate as this, and the solar rays falling on it very obliquely are dispersed into their component colours, the air being the dispersive medium. These colours cannot be observed (1) unless the obliquity is very great, for the colours overlap and white light is the result; (2) unless there is very little moisture in the air to absorb the more delicate colours and so prevent the dispersion effect being distinguished. In the case I refer to both these conditions were present in a remarkable degree. A reference to the figure will show at once the explanation offered for the spectral appearance of the sky. Let A A′ be the surface of the earth, B B′ the upper limit of the atmosphere, D D′ the upper limit of the more refracting layers of the atmosphere, A the point of observation, A X the horizon.

A solar ray striking the atmosphere at P will be refracted and dispersed, the blue ray striking the earth at C, and the red at C′. When these rays reach the surface of a sea that is nearly calm they will be reflected in various directions according to the angle at which they strike the wave-surfaces. Some will be reflected nearly vertically and will be lost in the clear sky, others will be reflected less vertically and will be subjected to the refracting influence of the lower strata of the air, the blue being refracted most and the red least. Thus the figure C F A may

represent the blue ray that suffers most refraction of all the blue rays, and C E A the red ray which suffers most refraction of all the red rays.

It thus appears that of blue and red rays reflected from the sea at the same angle, the former may reach the eye of the observer and the latter not, because, though the refraction is sufficient for the blue it is not so for the red ray, and it will be lost in the upper air. Consequently the blue rays will appear highest, and the red lowest, the other colours occupying intermediate positions according to their refrangibility. It is evident that any of these rays may be reflected too vertically from the sea, and so not be refracted to the earth again, but a considerable proportion will be thus refracted, and as has been said, more vertically inclined rays of the blue than of any other colour.

When we consider the effect of rays falling to the left of C, the phenomenon becomes more complicated. The same refraction, dispersion, and reflection take place, bnt the rays after reflection will mostly fall short of A, and strike the sea at various angles, producing a great variety of colour. It is not necessary for the effect that both the blue and the red from the same pencil of light should proceed to A, althongh this is shown for the sake of simplicity in the figure. It is sufficient if we know that blue rays, on account of their greater refrangibility, must of necessity be the highest, and the red, on account of their least refrangibility, the lowest.

If the above suggestion as to the dispersive power of the atmosphere be admitted, it is probable that the question of the colour and scintillation of stars will be directly affected by it.

Little Bromley, Manningtree, July 12 R. ABBAY

Zoological Geography—Didus and Didunculus

MR. SEARLES V. WOOD will, I trust, pardon me if I again take exception to the terms in which (*suprà*, p. 301) he still writes of *Didus* and *Didunculus*. These two birds do not belong to the same gronp of *Columbæ*. The fact that certain anthors may have included them under the designation of "ground-doves" is no proof whatever of their relationship, any more than it is of the relationship of either to any other birds so called—for instance those of the Neotropical genus *Chamæpelia*. I have studied pretty carefully the osteology of many forms of *Columbæ* with especial reference to their affinities. *Pezophaps* and *Didus* are of course nearly allied, though even these are not congeners. *Didunculus* is at least as distinct from them as from all other *Columbæ* with the possible exception of *Otidiphaps*, which last I have not had an opportunity of examining. Furthermore, I may remark that if Mr. Wood will but look at what has been published of the habits of *Didunculus* he will find that it is as much an arboreal as a terrestrial bird, so that the name of "ground-dove" is as unhappily applied to it as is that of *Didunculus* or its ridiculous translation, "Dodlet."

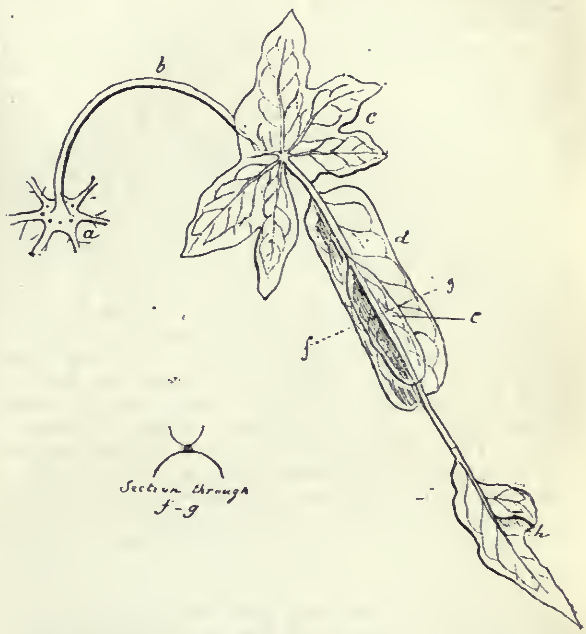
July 22 ALFRED NEWTON

Autophyllogeny

THE following case of *Autophyllogeny*, observed in a leaf of *Papaya vulgaris* (the well-known papaw-tree) appears to me of sufficient interest to be recorded in the columns of your highly interesting journal.

The letter *a* designates the central part of the primary leaf, corresponding to the apex of the petiole on the upper side of the blade. It shows some small warty protuberances, and from amidst them rises a new petiole (*b*), about six centimetres long and one and a half millimetre thick. This new petiole bears an accessory leaf of somewhat pentagonal outline (*c*), slightly crumpled and partially concave towards the upper side (the one directed downwards in the figure), as if there had been some tendency of forming a leaf pitcher. A little onwards two boat-shaped appendices are observed (*d* and *e*), the midrib or petiole forming their keel. They are real leaf pitchers, though of a rather uncommon form. The small lateral diagram represents the shape of the transversal section through *f* and *g*. The two leaves are opposed to each other by their upper sides, which are of a dark green colour; the concave parts are their under sides, as is proved by their pale green colour, which is generally the case in the leaves of the papaw-tree. The end of the petiole bears a pointed leaf (*h*), slightly contracted, and with a pitcher-like contortion on one side. The figure is about three-fourths natural size.

The case belongs to those mentioned by Masters ("Vegetable Teratology," 355, 445) under the heads of Pleiophylly and Enation from foliar organs. His explanation is certainly correct, as there cannot be any doubt that the accessory petiole *b*, but for its development in another plane, is a true homologon of the ribs of the primary leaf, and the minute warts round its base may be regarded as small or checked beginnings in this same direction.



The described anomaly does not appear to be rare [in *Papaya vulgaris*. I have observed several less-developed instances; the specimen here described was given to me by one of our students, Señor Ramon Documet. A. ERNST

Caracas, June 16

Microscopy—The Immersion Paraboloid

As I am responsible for exhibiting at the Conversazione of the Royal Society, May 1, the immersion paraboloid as being "designed by Dr. Edmunds," I should wish it to be known that, since that date, my attention has been directed to evidence establishing Mr. Wenham's priority to the invention.

Before exhibiting the paraboloid at the Royal Society, I had Dr. Edmunds' assurance that he felt justified in requesting me to describe it as designed by himself. JOHN MAYALL, Jun.

224, Regent Street, London, July 16

THE GENESIS OF LIMBS[1]

III.

I HAVE found much resemblance between the skeleton of the ventral and the dorsal fins in *Notidanus*, in *Chiloscyllium*, and in *Raia*; also between the anal and ventral fins in *Notidanus*. The ventral fins of elasmobranchs generally are so different from their pectoral fins, and so much more like the azygos fins than the pectorals are, that they serve well to bridge over the differences between the orders of fins. At the same time the value of the link is enhanced by the fact that in the very peculiar genera *Callorhynchus* and *Chimæra* the ventrals resemble the pectorals in a very remarkable and exceptional manner. But perhaps the most instructive ventral fin is that of *Polyodon*, the skeleton of which consists simply of a double series of simple parallel rays without any attachment to a pelvic cartilage which is altogether absent.

These conditions, then, appear to obliterate the distinctions which are at first apparent between the skeletons

[1] Continued from p. 311.

of the azygos and paired fins themselves. It remains to speak of the supporting structures of the paired fins, the pelvic cartilages or bones, and the shoulder-girdle. At first it appears that a formidable objection against the similar nature of the paired and azygos fins may be drawn from the existence in the former of these supporting structures (which serve in the pectoral region to fix the pectoral fins to the axial skeleton), while no such connection ordinarily exists with regard to the azygos fins.

We have seen, however, that in *Pristis* and *Pristiophorus* the dorsal fin becomes directly continuous with the axial skeleton by a mass of cartilage large enough to warrant comparison with the shoulder-girdle itself, while it is more or less firmly united with the axial skeleton in *Rhyncobates*, *Squatina*, *Acanthias*, *Spinax*, *Chimæra*, and *Callorhynchus*. It must be admitted, however, that the attachments of the dorsal fin to the axial skeleton is horizontal, direct, and continuous, while the structure supporting the pectoral fin (the shoulder-girdle) extends vertically, is arched in shape, and only abuts at one end against the axial skeleton, while ventrally it joins its fellow of the opposite side. These characters seem at first to tell against the similarity of nature of the dorsal and pectoral fins. But three things should be borne in mind—(1) the pectoral fin-support could not continuously adhere to the axial skeleton antero-posteriorily without impeding the lateral flexure of the body in swimming; (2) the pectoral fins join the body at too low a level for their support to extend in horizontally to the skeletal axis; (3) and did it so extend inwards in a straight line, even obliquely, it would intrude upon the visceral cavity. For these reasons the pectoral (and ventral fins also) must (if they are to rest on a solid support to facilitate their flapping motion) have a narrow connection with a sustaining structure, which structure must not be directly continuous, in a straight line, with the skeletal axis. Moreover, to obtain a firm basis, this limb-support, if it is attached obliquely upwards to the skeletal axis, must have some point to abut against ventrally also. Thus such support must assume the form of a limb-girdle.

I think, then, that there is sufficient evidence to warrant a belief that the skeletal structures of the paired fins of fishes (and therefore the limbs of higher vertebrates also) are the result of the centripetal growth and coalescence of a primitively distinct, parallel series of cartilaginous rays, developed in a pair of lateral fins similar to those developed, and more or less coalescing and centripetally extending in the median fins above and below.

But what about the limb-girdles themselves? Mr. James K. Thacher,[1] of New Haven, Connecticut, has thrown out the suggestion that the pelvic bones and cartilages of fishes (and therefore limb-girdles generally) are also due to the further extension inwards of such centripetal growth. I regard this as a most happy suggestion, and adopt it myself. The mystery of the limb-girdles is thus satisfactorily explicable; they are neither modified branchial arches, extra-branchials, nor ribs, but parts *sui generis*, due to the ingrowth of originally superficial structures—exoskeletal hardenings which have grown inwards and become endoskeletal.

It remains to consider the question of the development of the original digit-bearing limbs, *cheiropterygium*, from the primitive fin, or *archipterygium*.

Gegenbaur at first regarded the elasmobranch fin as derived from a limb formed like that of *Lepidosiren*, but he subsequently adopted that of *Ceratodus* as the archipterygium, in which view Huxley coincides. The former naturalist, however, considers the shark's fin and the cheiropterygium as formed by the all-but complete abortion of the rays on one side of the ceratodus limb-axis, with the simultaneous shortening and thickening of that axis into a metapterygium, while the rays of the other side of the axis coalesce to form the meso and propterygium. The latter anatomist (Huxley), on the contrary, regards the ceratodus-limb axis as forming by its progressive shortening (or drawing-in) the mesopterygium of the shark's pectoral, and the limb-axis of the cheiropterygium, the latter being perfected by the atrophy of the proximal lateral rays and the hypertrophy of the distal ones, the distal end of the axis becoming the middle digit of the hand. Of these two views the latter seems to me much to be preferred, but it demands the unity of the *centrale* carpal ossicle, which now seems most probably to have been primitively double, as it is so not only in *cryptobranchus*, but also in both limbs of three species of Siberian Urodeles.[1]

I believe, however, that the limb of *Ceratodus* is far from showing us a primitive form, but is, on the contrary, a very special and peculiar structure, which is carried to a still more abnormal development in *Lepidosiren*. This view seems warranted by the theory of evolution, according to which air-breathing vertebrates must have been amongst later developments, and therefore have post-dated creatures with limbs more or less like those of Elasmobranchs and Teleosteans. The secondary

FIG. 17.—Pectoral fin of Acanthias (from Gegenbaur). *p*, propterygium; *ms*, mesopterygium; *mt*, metapterygium. The line drawn through *mt* indicates the fundamental line of the archipterygium or Ceratodus limb-axis.

fringing rays of the central limb axis of these Dipnoi may (as Peters pointed out as long ago as 1845) have arisen like the secondary fringing rays of the dorsal of the primary rays of the dorsal fin of *Polypterus*.

As to the formation of the cheiropterygium, I think that there are some reasons which favour the acceptance of the propterygium as the part in Elasmobranchs which has most relation to its primitive axis. Such are (1) the preaxial position, in the limb, of the line of the Propterygium—which is the line of support needed for the fore-limb of a quadruped which necessarily extends preaxiad, distally; (2) the apparently complete atrophy of the mesopterygium in *Chiloscyllium* and its partial atrophy in *Polypterus*, and other forms; (3) the large size of the propterygium in *Chimæra*, *Callorhynchus*, *Cestracion*, *Scyllium*, and *Pristiurus*.

On the whole, then, I feel much persuaded that vertebrate limbs have been formed as follows:—

1. Two continuous lateral longitudinal folds were developed, similar to dorsal and ventral median longitudinal folds.

[1] See *Trans.* Connecticut Academy, vol. i.i.

[1] See "Morphol. Jahrbuch" vol. ii. 3rd Heft, p. 421, Pl. 29.

2. Separate, narrow, solid supports, in longitudinal series, with their long axis at right angles with the long axis of the body, were developed in varying extent in all these four longitudinal folds.

3. The longitudinal folds become interrupted variously, the lateral folds so as to form two prominences on each side the primitive paired limbs.

4. Each anterior paired limb increased in size more rapidly than the posterior limb.

5. The bases of the cartilaginous supports coalesced as was needed according to the respective practical needs of the different separate portions of the longitudinal folds, *i.e.*, the respective needs of the several fins.

6. Occasionally the dorsal rays coalesced proximally and sought centripetally adhesion to the skeletal axis.

7. The rays of the hinder paired limbs did so more constantly, and ultimately prolonged themselves inwards by mediad growths from their coalesced base till the piscine pelvic structures arose.

8. The pectoral rays with increasing development also coalesced proximally, and thence prolonging themselves inwards to seek a *point d'appui*, shot dorsad and ventrad to obtain a firm support, and at the same time to avoid the visceral cavity; thus they came to abut dorsally against the axial skeleton and to meet ventrally together in the middle line below.

9. The lateral fins, as they were applied to support the body on the ground, became elongated, segmented, and narrowed.

10. The distal end of the incipient cheiropterygium either preserved and enlarged pre-existing cartilages or developed fresh ones to serve fresh needs, and so grew into the developed cheiropterygium.

11. The pelvic limb acquired a solid connection with the axial skeleton—a pelvic girdle—through its need of a *point d'appui* as a locomotive organ on land.

12. The pelvic limb became also elongated, and when its function was quite similar to that of the pectoral limb its structure also became quite similar. It became segmented in a way generally parallel with the segmentation of the pectoral limb, yet in part inflected inversely owing to its different mode of use.

Vertebrate limbs then are specialised differentiations of primitively continuous lateral folds, and might, for all we see, have been more numerous than two on each side, just as there are sometimes several successive dorsal fins which are all differentiations of a primitively continuous dorsal fold. The paired limbs and azygos fins may thus be all viewed as different species of one fundamental set of parts, *pterygia*, the sum total of which may be spoken of as the *sympterygium*. The paired fins of fishes are related to the limbs of higher vertebrates as structures which have diverged from their primitive condition to a less degree, not only because the piscine body is, as a whole, a more primitive structure, but also because their fins are still used for locomotion in that medium in which their primeval form—the continuous lateral fold—was first developed.

The amount of adaptive modification supposed will perhaps appear to some persons to be excessive. But I believe that the excessive plasticity of animal organisms is in general too little appreciated—a plasticity which results in, and is evidenced by, the many instances of homoplasy—the independent origin of similar structures. The existence of these adaptive modifications points to the existence of an intra-organic activity, the laws of which have yet to be investigated. The instances of serial and bilateral homology before cited from comparative anatomy, pathology, and teratology, also concur in pointing to an intra-organic activity, the laws of which are as yet unknown. The notion of an "internal force" is very repugnant to some of my contemporaries, but it is impossible to banish the idea of innate powers and tendencies, the existence of which is manifested in the inorganic world as well as in the organic world. We cannot conceive the universe as consisting of atoms acted on indeed by external forces, but having no internal power of response to such actions; and in "physiological units" and "gemmules" we have (as Mr. Lewes has remarked) "given as an explanation that very power which was pronounced mysterious in larger organisms."

Mr. Lankester[1] speaks of each animal function, even reproduction, as being "explained by its chemical and physical constitution," and of "the possibility of development" being "solely due to the physico-chemical constitution of protoplasm;" but he does not give the explanation, nor show how such constitution by itself gives developmental power. But even if he did the puzzle would but recur—By what process of the survival of the fittest did the inorganic substances obtain their various structures and innate powers?

To my mind the presence of a special internal force is made evident by the process of development; and I am disposed to concur with Milne-Edwards when he says: "Dans l'organisme tout semble calculé en vue d'un résultat determiné, et l'harmonie des parties ne résulte pas de l'influence qu'elles peuvent exercer les unes sur les autres, mais de leur co-ordination sous l'empire d'une puissance commune, d'un plan preconçu, d'une force préexistante."

Science, as I understand it, clearly points to the existence in each animal of something more than an amalgam of physical forces, to a force or principle which is *intra-organic*, as heat is in red-hot iron or light in the glowing photosphere of the sun—one with it as the impress on stamped wax is one with the material bearing such impress, though we can ideally distinguish the two. This power or force immanent in each living body, or rather which is the force of the body living (considered in an abstract way), is of course unimaginable by us, since we cannot by imagination transcend experience; nothing can be imagined by us which has not wholly or in its parts been the subject of our sensible experience, and we can have no sensible experience of this force, save as a living body acting.

It is on this account sometimes thought reasonable to deny its existence as a "figment of the intellect," forgetting the supremacy of the intellect over sense. Though no knowledge is possible to us except as following upon sensation, yet the ground of all developed knowledge is not sensational, but intellectual; it reposes ultimately not on "feelings," but on thoughts. Even in verification by sensation it is the *intellect* which doubts, criticises, and judges the action and suggestions of the senses and imagination. If then we have *rational* grounds for the acceptance of such a purely *intellectual* conception, the poverty of our powers of *imagination* should be no bar to its acceptance. We are continually employing conceptions of the kind—such, *e.g.*, as number, being, substance, causes, &c.,—conceptions perfectly intelligible, though transcending the powers of the imagination.

If, then, we should conclude that each living animal possesses a special and peculiar intraorganic force, and if such force be the imminent cause of nutritional balancings, and thereby of the facts of serial and bilateral symmetry, is it not reasonable to refer to that same cause directly adaptive modifications which, within limits, take place in response to the actions of the environment. The presence of such innate activity has been eloquently proclaimed by Hartman, though I would repudiate the contradictory term "unconscious intelligence," and would explain it in a way which differs widely indeed from his. But if such a power is the active agent in such organic adaptations, is it not reasonable to refer to it the special variations which result in the formation of new species? This is the very activity for the existence of which I have elsewhere contended,

[1] *Quarterly Journal of Micros. Science*, October, 1877, pp. 432, 433.

and to which I have applied the term "specific genesis," and it is this which I am more and more persuaded is the determining agent in, and therefore the one true cause of, the origin of species.

St. George Mivart

VARYING EXPERIENCES

I LOVE to repeat other people's experiments, and though not in the least doubting the accuracy of recorded observations in relation to bees, clover blossoms, and fertilisation, some years ago I covered patches with wire netting, to exclude the bees, for all, every flower I believe, perfected its seeds. I hope I have earned a reputation for accuracy in my statements of facts, and that it is not necessary for me to call witnesses. I will say here, however, that about that time I was visited by Dr. Sterry Hunt, ex-President of the American Association for the Advancement of Science, and together we uncovered one patch, and examined a few mature heads, with the result as above stated.

Recently I referred to Mr. Darwin's statement that one might as well sprinkle *Linum perenne* with so much inorganic dust as its own pollen, and stated that in my own garden a plant from the Rocky Mountains perfects seeds, and can only use its own pollen. An esteemed friend takes me to task[1] for this statement, remarking that I have overlooked that Mr. Darwin's facts are confirmed by Dr. Fritz Müller, in Brazil. This, in connection with remarks made on my clover experience, leads me to suppose that some believe I have offered the facts in opposition to those of Mr. Darwin. Nothing has been further from my thoughts. My point has been to show that plants or insects do not always behave in the same manner, on all occasions, and under all circumstances. I had an interesting illustration of this in March last. Having occasion to examine a large patch of chickweed (*Stellaria media*), I was surprised to find a number of honey-bees engaged in collecting pollen from them. For the past few years I have made a point of closely watching the behaviour of insects towards flowers, and I never saw honey-bees at work on chickweed before; I never heard of any one who has. I believe the chickweed has been given up to rigid self-fertilisation. Profusely among the chickweed grew *Draba verna*. The flowers of the two are about the same size, and both white, but the bees kept with strict exclusiveness to the chickweed. Yet I know that the Draba is not obnoxious to them, for in other years I have seen them at work on these flowers. Among them also were some *Capsella Bursa-pastoris* in bloom; but they also were passed by. I have never seen bees or any insects on the shepherd's purse, but from this chickweed experience it would not be safe to say none ever do visit them. The date of this visit of the bees was March 15, the thermometer 52°, spring scarcely begun, and only these three early plants in bloom.

I had a similar instance last autumn of the honey-bee's faith in the crust of bread theory rather than have no loaf at all. We had an open mild season, and towards Christmas, long after all other flowers were gone, the *Salvia splendens*, of which I employed a large number in the decoration of my grounds, was alone in flower. On warm days they were thronged with honey-bees, and I feel almost sure they had never visited my plants in other years when other flowers were to be had. The corolla tube is too long for the bees, so they had to bore the corolla from the outside. Boring from the outside is easy work for our large humble-bees. Almost all our flowers which offer the least obstruction to mouth entrance are robbed of their sweets in this manner. Even red clover is "tapped" by them in this way. But it was very hard work for the honey-bees, and I am sure that, only for the absence of other and easier worked flowers, I should not yet be able to say that I had seen the honey-bee bore from the outside of a flower, as the humble-bee generally does. There were white-flowered varieties of this species among the scarlet ones, but all were treated alike.

It seems to me that bees are not attracted to flowers by colour or fragrance merely, but that they are influenced by labour-saving ideas. A little experience teaches them how best to work in any species to advantage, and they will of course "make time" by keeping to this one till all are done. White varieties or scarlet varieties are all one to them, they can distinguish the species by other means than colour. And then they learn where to work to the best advantage, and only glean in poor fields after the richer harvest has been gathered. These considerations will naturally lead to different behaviour in different climates, and if I note these differences it is very far from my intention to offer them as contradicting the experiences of others; on the contrary, no one has a higher appreciation of their value.

Thomas Meehan

Germantown, U.S.

[1] In *Silliman's Journal*.

OUR ASTRONOMICAL COLUMN

Double Stars.—In Gilliss's catalogue of 290 double stars formed from observations made at Santiago, Chile, during the U.S. Astronomical Expedition in the years 1850-52, the conspicuous star α Eridani (*Achernar*), is reported to have been seen double, the companion being of the seventh magnitude, faint blue, and preceding, 3" south. We look in vain for mention of this companion-star in the observations of Herschel, Jacob, and Powell, and it is especially strange that it should not have been detected by the former during his sweeps with the 20-feet reflector at the Cape. The well-known binary p Eridani is less than 2° distant, consisting of two nearly equal components of between the sixth and seventh magnitude, and at first sight it might be inferred that by a typographical error the name of the star is wrongly given by Gilliss. His position, however, is that of α Eridani, and further we happen to possess measures of p Eridani by Jacob, at the precise epoch of the Santiago observation 1850·79, giving for the angle 268°·7, and distance 4"·32; the *comes* therefore could hardly be described as preceding, 3" south, but might rather be said to precede on the parallel. This would indicate that the star intended is really *Achernar*, and it must be left for further observation to decide upon the accuracy or otherwise of the statement made by Gilliss. If the companion exists it would be of interest to know its present position; the proper motion of the principal star is very insignificant, and marked difference from Gilliss's description would be suspicious as showing a binary character. Still it is to be observed that there are considerable discordances between the angles and distances of many of the stars in the Santiago catalogue and those in Herschel's Cape volume. The former are not the results of actual micrometrical measures. It is stated that the catalogue was formed by plotting, on a large scale, the differences of right ascension and declination of the components of the double-stars observed with the transit-circle ($4\frac{1}{2}$ inches aperture), and then measuring from the drawings the angles of position and distances. In most cases the right ascensions and declinations observed are given in the preceding catalogue of 1,963 stars, and the results of the graphical process can be verified by calculation. In looking through the list of double-stars the reader will note differences from Herschel's data, which are not always easily explained by possible motion, though, as some of the stars have not been properly measured since Herschel's epoch, there will remain a doubt as to the cause of these differences. As instances in point, we may mention the following numbers of the Cape cata-

logue:—3860, 3966, 4119, 4281, 4538, 4667, and 4770. Probably Mr. Ellery at Melbourne, or Mr. Todd at Adelaide—both of whom are understood to be partially occupied with measures of the southern double-stars—may eventually clear up the uncertainties which characterise the results published by Gilliss.

While referring to the catalogue of stars observed at Santiago, it may be remarked that the majority of the large proper motions shown by comparison with Lacaille are proved to arise from errors of observation on his part, when we examine the particular cases with the aid of the valuable volumes which Mr. Stone is so regularly issuing from the Royal Observatory, Cape of Good Hope. When the volumes containing the observations made in 1876 (N.P.D. 135°–145°) and in 1877 (N.P.D. 125°–135°) are published, Mr. Stone will have placed in the hands of astronomers the means of investigating the proper motions of a large number of southern stars, which can hardly fail to lead to conclusions of much interest and importance. We are not justified in supposing that in Groombridge 1830 we have the case of largest proper motion in the northern hemisphere, and as to the proper motions of southern stars our knowledge is yet but very limited, and very conspicuous instances of rapid translation may remain to be detected amongst the telescopic stars of the southern heavens.

THE NEW COMET.—The telegram notifying the discovery of a new comet, and forwarded by the Smithsonian Institution to M. Mouchez, Director of the Observatory at Paris, is in these terms:—"Discovery, by Lewis Swift, of Rochester, of a large and faint comet, July 7, 1878, at 2h., in 17h. 40m. right ascension and 18° north declination, with slow motion towards the southwest; neither tail nor nucleus, but a central condensation. Query, is it the Tempel comet?" In communicating this telegram to the Academy of Sciences, on July 15, it was stated that the sky had been overcast at Paris, and therefore no opportunity had been afforded for verifying the discovery, and further that, notwithstanding an immediate intimation was given to the principal European observatories on receipt of the telegram at Paris on July 9, M. Mouchez had not heard of any observation elsewhere.

In this country several practised observers have failed to detect the comet, though the skies have been at times very favourable. The query in the American telegram, referring to Tempel's comet, might suggest that the declination of the comet was *south*, but, upon submitting the point to calculation, it does not appear that this change will afford an explanation of the want of success. If Tempel's comet were in perihelion about noon on August 11, its right ascension, at the time of Mr. Lewis Swift's discovery, would have been as he estimated it, but the declination would not be more than 7½° south. A large, faint, diffused nebulosity, however, is easily overlooked—the best chance of detection, when the position is not precisely known, being probably afforded with the "comet-seeker," by which we mean such an instrument as is (or was formerly) constructed by Pistor and Martins, of Berlin. Mr. Lewis Swift was already the independent discoverer of a comet, and is not likely to have been mistaken or misled by any optical illusion on this occasion.

[Since the above was written we learn that Prof. Winnecke re-observed the periodical comet of Tempel at Strasburg on July 20, the position obtained that evening indicating that the perihelion passage will not take place until September 6, or between five and six days later than the date fixed by M. Schulhof's calculations, which is, perhaps, as close an agreement as was to be expected, since the observations in 1873 did not suffice for the very accurate determination of the mean diurnal motion. The comet was from 2′—3′ in diameter, with nuclear condensation. When the mean anomaly is so corrected that the observed and computed longitudes for July 20 are made to agree, the latitudes differ only one minute, proving that M. Schulhof's other elements are very near the true ones. The following places for midnight at Greenwich may facilitate observations:—

	Right Ascension.	North Polar Distance.	Log. Distance from Earth.	Sun.
	h. m. s.	° ′		
July 26	15 24 48	97·7	9·8708	0·1518
,, 30	15 30 25	98·48	9·8741	0·1476
Aug. 3	15 36 50	100·30	9·8777	0·1438
,, 7	15 44 2	102·14	9·8818	0·1402
,, 11	15 52 0	103·57	9·8861	0·1371
,, 15	16 0 42	105·40	9·8908	0·1343

The intensity of light does not sensibly vary during the above interval.]

METEOROLOGICAL NOTES

TO the meteorologist the recent discussions in Parliament and out of it regarding the salubriousness or insalubriousness of the climate of Cyprus have been, if not instructive, at least amusing, the amusement arising from the circumstance that positive information was not forthcoming in support of the strong statements made on both sides. Thanks, however, to the Scottish Meteorological Society, we have trustworthy information on the subject, that Society having established there one of its foreign climatological stations in 1866, where, for about four years, observations were made by Mr. J. B. Sandwith, H.M. Vice-Consul, and the results regularly published in the Society's *Journal*. Summarising these results, we learn that the annual rainfall is about 14 inches, nearly the whole of which falls from November to April, notably in November and December, that no rain falls in June, July, and August, and only trifling amounts, but occurring rarely, in May and September. There is thus practically five rainless months in the year in Cyprus, the rainless summers being a feature in its climate common, as we have recently had occasion to remark, to the climates of the Mediterranean regions south of latitude 43° (NATURE, vol. xviii. p. 287). Comparing it with the coasts of Syria opposite, its winters are milder and its summers cooler; and the decidedly insular character of its climate is further apparent from the fact that the coldest month is February, with a mean temperature of 52°·8, being about equal to that of London in the middle of May, and that the mean temperature of August is nearly as high as that of July, both being about 81°·0, which is approximately the summer temperature of Algiers, Alexandria, Athens, and Constantinople. During these four years the highest recorded temperature in the shade during any of the months was 96°·0, except June, 1869, when, from the 21st to the 25th, the mean temperature at Alethriko, 3½ miles inland from Larnaka, reached 95°·5, being about the average summer temperature of the Punjab, rising on the 24th to a maximum of 105°·0. On the same day the temperature rose to 100°·0 at Larnaka, and to 103°·5 at Jerusalem, 2,500 feet above the sea, the period being characterised as one of unprecedented heat and drought over the whole of the regions bordering the Levant. It is obvious to remark that much may be done in mitigation of the effects of the summer heat, just as has been done in countries similarly circumstanced, by the establishment of sanataria among the mountains, and by carrying through agricultural improvements and engineering works, which would at the same time contribute to the material prosperity of the island.

PROF. LOOMIS, in a ninth Contribution to Meteorology, handles admirably a question of first importance in

the practical bearings of the science, viz., the relations of the barometric depressions and storms of the Pacific States to the storms east of the Rocky Mountains. As regards the twenty-seven storms whose courses he has traced, it is probable that the great majority, if not the whole of them, were first formed over the Pacific Ocean. In each of the twenty-seven cases (with perhaps one single exception) the storm crossed the Rocky Mountains, and was thence tracked across the United States to the shores of the Atlantic, subject, however, in some cases to modification in its progress. It is scarcely possible to overrate the importance of these results in the practice of weather telegraphy and on questions affecting the general movements of the atmosphere. For we see here that an unbroken mountain-range of at least 6,000 feet in height does not stop the eastward progress of these barometric depressions and storms; neither do mountain-ranges of more than 10,000 feet in height, broken as in North America, present an insuperable obstacle to the onward course of these phenomena. The mountain-ranges between the Pacific and the Mississippi present obstructions to the formation of a system of winds of any great geographical extent; and hence, probably, barometric depressions are not so great over this uneven and broken region as over the vast plains of the Mississippi and eastern States, where there are no mountain barriers to interfere with the formation of a system of circulatory winds over areas 2,000 miles in diameter.

CHEMICAL NOTES

Influence of Temperature on the Rotatory Power of Quartz.—Following up the researches of Lang and Fizeau, Sohncké has found (*Ann. d. Phys. Chem.*, N.S. III. p. 516) that the increase in rotary power in quartz, with increase of temperature, is not directly proportional to the temperature, but is less at lower than at higher temperatures. For the crystal he experimented on he determined the following formula:—

$$\phi = \phi^{\circ} (1 + 0{\cdot}0000999\, t + 0{\cdot}000000318\, t^2),$$

where ϕ° = the rotatory power of the same crystal at 0°; and he further found that the relative increase of power in the plane of polarisation was the same for all colours up to 170°. To see whether the octahedral system presented the same phenomena he also examined common salt, and obtained similar but more strongly-marked results.

Change of Indices of Refraction in Mixtures of Isomorphous Salts.—M. Dufet, in the *Comptes Rendus*, lxxxvi. 881, gives a most interesting account of some experiments he has carried out on the above subject, partly in continuation of such work as that of Senarmont, Topsoë, and Christiansen (*Ann. Chem. Pharm.*, 1874). Instead of examining simple isomorphous salts the author has taken mixtures containing varying quantities of magnesium and nickel sulphates, but of known composition. Working with such bodies he has determined that "the differences between the indices of a mixture of two isomorphous salts and those of the component salts are inversely proportional to the number of equivalents of the two salts entering into the mixture." In his calculation, M. Dufet has taken as an equivalent the number 111 or one equivalent of $SO_4.7HO$. He considers the law of variation of the index as a consequence of Gladstone's law: the refractive energy $\frac{n-1}{D}$ of a mixture of two bodies with no chemical action on one another, being the sum of the refractive energy of the component substances. According to M. Dufet, isomorphous salts crystallising together, form mixtures presenting analogies to a certain extent comparable with liquid mixtures, where the physical properties are the mean of those of the components; this, however, is only true up to a certain point.

Allotropic Modification of Copper.—By the electrolysis of a solution of about 10 per cent. of copper acetate Schützenberger has obtained an allotropic variety of copper somewhat remarkable in its physical and chemical properties. During the electrolysis the surface of the negative platinum electrode which faces the positive copper electrode becomes covered with a layer of the allotropic modification of the metal, whilst the other side of the electrode is covered with a deposit of ordinary copper. The allotropic modification forms metallic glittering scales with roughened surfaces on the side next the solution; should the electrolysis be carried on long enough, beautiful tree-like forms are deposited on the edge of the negative electrode, which gradually ramify over to the positive electrode. The allotropic copper is less red than the ordinary variety, possesses surfaces without malleability, and can be reduced to an extremely fine powder. Its density, 8 to 8·2, is higher than that of the ordinary variety, which is about 6·9. It oxidises rapidly in the air, becoming at once iridescent, and finally of an indigo blue colour; when exposed to the air as a powder it becomes black, changing finally into the oxide. According to the author it becomes reconverted into the ordinary form of copper by heat, or exposure to certain chemical agents.

Saccharose.—M. J. Motten has recently brought forward a paper, entitled a "Contribution to the History of Sugar (Saccharose)," in which the author discusses the action of light and of a temperature of 100° Cels. on solid and dissolved sugar, proving that the light alone does not invert dissolved sugar, and also that a temperature of 100° does not alter dry sugar. On the other hand solid sugar imperfectly dried, and dissolved sugar are altered under the influence of that temperature; oxygen is then absorbed, and carbonic acid evolved, but more slowly than it was often supposed.

Heat Evolved in the Formation of Isomeric Bodies.—M. Berthelot has given several communications to the Chemical Society of Paris, relating to the above subject. He finds that in general nitro compounds and isomeric nitric ethers appear to be formed with the disengagement of very unequal quantities of heat; the transformation of ethers into nitro compounds disengage approximately thirty heat units, at the same time undergoing increase of density and rise in the boiling point. In the case of metameric acids, as butyric, isobutyric, valerianic, &c., combining with the same base, his numbers show that the heat disengaged is precisely the same in the various cases which he describes. Approximately equal numbers are also obtained in the case of the chloro and bromo derivatives of these acids. There is very little difference also in the heat disengaged in the transformation of isomeric alcohols into isomeric aldehydes. The general results of his experiments, covering about thirty compounds, including alcohols, aldehydes, fatty acids, and their salts, chloro and bromo acids, &c., point to the conclusion that isomeric bodies having the same chemical function are formed with nearly identical disengagements of heat, their reciprocal metamorphosis disengaging very little heat. Finally, the same approximations exist in the formation of their isomeric derivatives.

Chemical Changes Taking Place during the Ripening of Grapes.—From experiments lately made on the transformations of the grape, and the exchanges between it and the surrounding atmosphere, MM. Saint Pierre and Magnien conclude that grapes at the time of their maturation liberate carbonic acid both in darkness and in light, the quantity produced being always superior to the quantity of oxygen consumed, if the experiment be long enough. This liberation occurs as well in an inert

gas as in air. Grapes are capable of absorbing or losing water when kept in a moist medium or in a dry medium. As maturation advances, the acids diminish and the sugar increases. The mechanism of maturation is stated to be this :—Acids and glucose are formed in the plant, and the sap conducts them to the grape ; the acids are consumed in it, while the sugar is concentrated. When the maturation is very advanced, the sugar is consumed in its turn.

RIPENING OF GRAPES AFTER REMOVAL FROM THE VINE.—In the *Gazetta chimica Italiana*, vii. 517, some experiments by M. Pollacci are described, in which he finds that the process of ripening continues for a certain time after the grape has been removed from the parent plant. The bunches of fruit removed were, as far as possible, equally divided, and the quantity of glucose and acid determined in the freshly-gathered grapes, as also in portions kept in the shade for some ten or twelve days. In all the portions which had been kept, the glucose had increased, whilst the amount of acid had diminished, showing that a certain amount of ripening action had taken place ; this action, however, ceases after a time, the ripening never attaining full maturity.

USE OF METHYL CHLORIDE FOR THE PRODUCTION OF LOW TEMPERATURES.—At a recent meeting of the French Physical Society, M. Vincent called attention to the use of chloride of methyl for production of low temperatures. It may be extracted in large quantities and cheaply from the products of beet-root molasses. It is normally gaseous, and liquefies under about four atmospheres pressure, when it may be conveniently carried about in iron or copper vessels, a store of cold at easy disposal. On opening a cock the liquid will flow out and give a bath at $-23°$, its boiling temperature under atmospheric pressure. If the vaporisation be intensified by a current of air, the temperature descends to about $-55°$. M. Vincent has arranged an apparatus for utilisation of such cold. He incloses two or three kilogrammes of liquid chloride of methyl in a double wall enveloping a bath of alcohol or chloride of calcium in solution, and protected exteriorly by an isolating layer of cork raspings. To obtain low temperatures, a cock is opened to allow communication of the double envelope (through a caoutchouc tube) with an air-pump.

FORMATION OF HYDROCARBONS BY THE ACTION OF WATER ON MANGANESE IRON ALLOYS CONTAINING CARBON.—Cloëz found that by acting on Spiegeleisen with dilute sulphuric acid bodies resembling the petroleum hydrocarbons were formed. On trying the action of pure water at 100° no results were obtained, while at 250° with super-heated steam, a certain action was perceived which increased with the temperature, being completed at a dark red. The hydrocarbons, however, were again decomposed. The same author has since tested a series of manganese alloys, and finds that the best results are obtained by means of one containing roughly Mn 85, Fe 6, C 3·5, Graphite 4, Si 1·1. Small portions of this, treated with boiling water, decomposed the latter with the evolution of hydrogen, oily drops being simultaneously formed, and the gas burning with a luminous flame showed the presence of hydrocarbons. Another alloy of nearly similar composition gave the following results: the flask contained slightly alkaline water with a mixture of iron and manganese oxides in suspension; the liquid hydrocarbons in the condenser were similar to those previously found, the gases also burning with luminous flames. He has thus shown that water alone at the proper temperature decomposes manganese iron alloys containing carbon.

ACTION OF BORON FLUORIDE ON CERTAIN CLASSES OF ORGANIC COMPOUNDS.—This body has been found by Fr. Landolph to combine in definite proportions, equivalent for equivalent, with certain classes of organic bodies such as aldehydes, acetones, and also with camphor. For his experiments the particular substances examined were ethylic, valeric, and benzylic aldehydes, ordinary acetone, euodic aldehyde (oil of rue), and ordinary camphor. In all these cases considerable disengagement of heat was manifested in the combinations of the several substances. By the action of the fluoride on acetone two products are obtained, the one boiling between 130°-140°, this being, according to the author, the most definite; another compound, however, exists which boils at a temperature of 160°-170°. The first is a fluid of a syrupy consistence and yellow-green colour ; it burns readily, giving a green flame, and is entirely decomposed by water. The compound, with ethylic aldehyde, ethylen fluoboride, $C_2H_3BFl_2$, undergoes decomposition when treated with water, into a body with a peculiar ethereal odour, the composition of which, the author thinks, may probably be C_2H_5Fl.

GEOGRAPHICAL NOTES

IN the just-published number of the Royal Geographical Society's *Proceedings* we find some useful remarks by Mr. F. Galton, on what has recently been done and what is further required for the advancement of geographical teaching. First and foremost, he says, is the publication of that excellent book by Prof. Huxley, "Physiography," which, starting from the simplest elements, led students steadily on to the higher conception of physical geography and the most recent discoveries in it. Next, Sir Walter Trevelyan, a former Secretary of the Society, had felt so much the necessity of a better form of text-book for geographical teaching that he had placed a handsome sum at the disposal of the Council to procure, if they were able to do so, the compilation of a really good county geography, to serve as an example for other similar works to be used in elementary schools. Turning to what is required in the future, Mr. Galton mentions that they have received a letter from a master of one of the great public schools, urging them to plan a system of diagrams explanatory of different physical features. His own opinion, Mr. Galton says, is that what is most urgently needed is some simple and well-methodised system of experiments, suited to illustrate lectures on the main features of physical geography. He has no doubt that an extension of the methods of illustrating the facts of physical geography—as used by Prof. Tyndall and Dr. Carpenter—on a small scale and on a lecture-room table, is perfectly feasible. Thus, as every thunder-shower shows in the streets the phenomena of erosion and deposition, he has no doubt that, on a lecture-table, with a can to supply water, and with a certain quantity of sand, gravel, and clay, all the main phenomena of river-action, such as the sifting of materials, the stratification of deposits, and the formation of deltas, might be successfully shown.

MEANS have recently been found, we learn from the *South Australian Register*, for still further increasing the usefulness of the Hon. (now Sir) T. Elder's camels on the far northern stations with which he is connected. The experiment of using them for draught purposes has been tried, and recently two teams of six camels drew loads of 5¼ tons each from Beltana to Port Augusta. The plan adopted is to yoke the animals together something after the manner in which bullocks are coupled, and one man only is required to manage each team. It has been found that the camels thrive well in the northern country ; the number originally imported several years ago was about 100, of which the greater part died, as the land, by its comparative richness, presented too great a contrast to their native soil ; there are now, however, about 400 of their descendants at Lake Hope, Umberatana, Beltana, and other stations in the far north, and the race seems to be thoroughly acclimatised. The camels have already been

found to be of great service in exploring expeditions, and they are still being used by parties engaged in opening up new pastoral country. Several of the animals have recently been lent to squatters for expeditions to the country on the Western Australian border, the MacDonnell ranges in Central Australia, and elsewhere.

FROM the new issue of Behm and Wagner's "Bevölkerung der Erde," we learn that the present population of the earth is estimated at 1,439 millions as compared with 1,424 millions given in the previous issue. This increase results mainly from the recent censuses which have been taken in several countries. This population is divided among the several continents as follows:—Europe, 312,398,480; Asia, 831,000,000; Africa, 205,219,500; Australia and Polynesia, 4,411,300; America, 86,116,000. This new issue contains the first map we have seen of New Zealand with the recent division into counties, in substitution for the old division into provinces. A census according to counties cannot, however, be taken till 1881. The North Island has thirty-three and the South Island thirty-one counties.

MR. STANFORD has issued a very useful shilling Treaty Map of South-Eastern Europe and Armenia, showing the boundaries of the New Bulgaria and Eastern Roumelia, the accessions to Austria, Russia, Montenegro, Servia, and Roumania, and all the other changes which have been made by the recent Congress. The new features are shown with unmistakable clearness. Mr. Stanford is also preparing a large scale map of Cyprus, showing not only the physical, but also the geological, agricultural, and other features of our latest acquisition.

AN expedition to the mouth of the Yenisei River left St. Petersburg last week. Principally at the instigation of a Moscow commercial firm eight steamers laden with corn, spirits, nitre, and other goods will soon start on the new sea-road to Siberia, their return cargoes consisting of wood and tea.

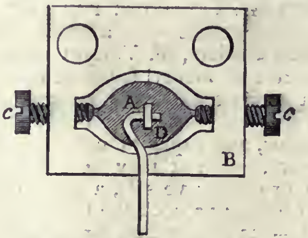
MR. GORDON BENNETT proposes to send the yacht *Dauntless* on a voyage of discovery to the Polar Seas, *viâ* Spitzbergen, in addition to the *Pandora*, which will attempt to reach the Pole by another route.

A METHOD OF RECORDING ARTICULATE VIBRATIONS BY MEANS OF PHOTOGRAPHY[1]

THE object of this paper is to describe a method of obtaining photographs of minute vibrations on a magnified scale.

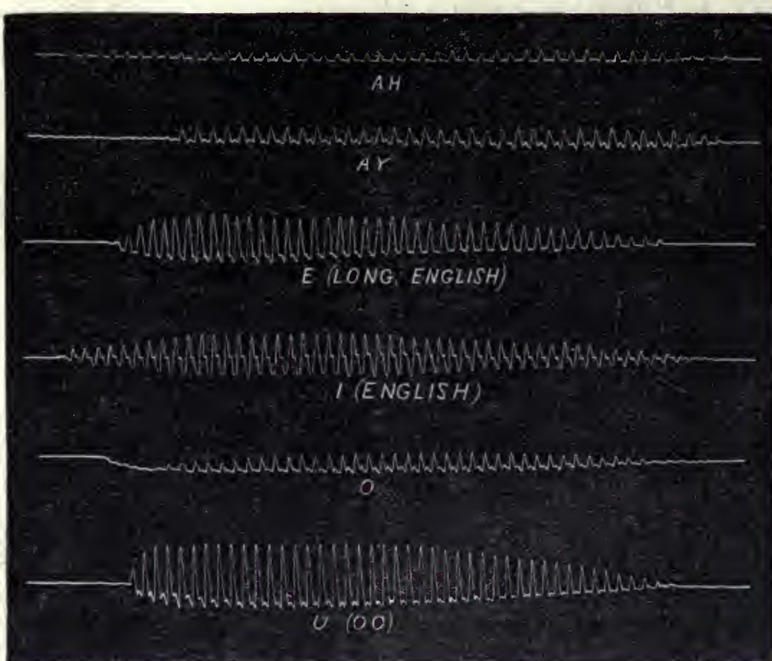
A plane mirror of steel, A, is supported by its axis in the metal frame B. The ends of the axis are conical, and carefully fitted into sockets in the ends of the screws C, C. On the back of the mirror is a slight projection, D, pierced by a small hole.

The vibrating disc, as hitherto employed, is a circular plate of ferrotype iron, 2¾ inches in diameter, screwed to the back of a telephone mouthpiece of the form invented by Prof. John Peirce, and now universally used. From the centre of the back of this disc a stiff steel wire projects, the end of which is bent at a right angle. This wire serves to connect the vibrating disc



Back view of mirror, actual size.

with the mirror by hooking into the hole in D, as represented in



the figure. The mirror frame and the vibrating disc are kept in a fixed relation to each other by a block of hard wood, to which both are firmly screwed. The mirror is set with its axis parallel, and its reflecting surface perpendicular, to the vibrating disc.

A heliostat sends a beam of sunlight horizontally through a small circular opening. This beam passes into a dark closet, and at a distance of several feet from the circular opening falls upon the mirror above described, placed with its axis inclined 45° to the horizon. The rays, reflected vertically downward, pass through a lens at whose focus they form an intensely luminous image of the circular opening.

[1] The text abridged for NATURE by Prof. E. W. Blake, of Brown University, from a paper in the *American Journal* for July. The illustrations (except mirror) from photos supplied by Prof. Blake.


Brown
U
ni


ver
si
ty


HOW
DO
YOU
DO

A carriage moving smoothly on four wheels travels beneath the lens at such a distance that the sensitised plate laid upon it comes at the focus for actinic rays. A uniform velocity is given to the carriage by a string fastened to it and passing over a pulley. To this string a lead weight, just sufficient to balance friction, is permanently attached, while a supplemental weight acts at the beginning of motion and is removed just before the sensitised plate reaches the spot of light above described.

The velocity attained by the carriage is determined by placing a sheet of smoked glass upon it and letting it run under a tuning-fork (Ut. 3—512 v. s.) provided with a pointed wire. In every case more than 200 vibrations were counted and measured, and careful comparisons made between the earlier and later ones, so as to be certain of the uniformity of the motion.

From the description it will be evident that when the carriage alone is in motion a straight line will be photographed upon the plate. On speaking into the mouthpiece the disc is set in vibration, each movement causing change of angular position of the mirror, the reflected light moves through twice this angle, and the resulting photograph gives us the combination of its motion with that of the carriage. The carriage should run from *right* to *left*. The negative (examined from the *glass* side), and prints taken from it, then give the syllables in their proper order, and show movements of the disc from the speaker by lines going from the observer. The arrangement of my dark room compelled me to make my carriage move from *left* to *right;* hence, in the figures given, forward positions of the disc are represented by the lower portions of the curves.

The general character of the curves obtained is shown in the accompanying figures, which are the actual size of the originals, except that representing the vowel-sounds, which is about one-half (0·56).

The velocity of the carriage for the vowel-sounds was 21½, for *Brown University*, 40, and for *How do you do*, 14 inches per second.

In the mathematical discussion of these curves the abscissas are measured by the known velocity of the carriage, and serve to determine the *pitch*, the ordinates represent the amplitude of vibration of the centre of the disc, magnified 200 times in the photographs. The reduction of scale makes the magnifying in the woodcuts only 112 times.

The ordinates are not strictly straight lines, but parts of the vertex of a parabola, and closely approximate to circular arcs whose radius is the focal length of the lens employed. In the figures given, the centres of curvature of these arcs is at the right hand.

With an ordinary tone of voice an amplitude of nearly an inch is obtained, implying a movement of the centre of the disc of ·005 inches as determined by actual measurement.

By varying the accelerating weight and its fall, any manageable velocity may be given to the carriage. Each syllable requires for its articulation about one-fourth of a second, hence the plates must be quite long when the velocity is great. I employ plates two feet in length, and find that velocities from 16 to 40 inches per second give good results. The action of the light is, however, inversely as the velocity. To compensate for this, the size of the circular opening admitting the light may be increased. This, of course, causes an enlargement of the luminous image, and apparently involves an injurious widening of the line traced, but, as observed by Dr. Stein in his experiments, the effect of velocity is to narrow the line photographed, since the maximum exposure is in that diameter of the circular image which lies in the line of motion. This is a great advantage, since a variation of velocity in the vibration is marked by the widening of the line, often more clearly than by the form of the curve.

I have employed the ordinary photographic process, not attempting to obtain special sensitiveness. The brightest sunlight is required, a slight haziness interfering seriously with the result. My heliostat employs two reflectors of ordinary looking-glass, and the loss of light is considerable.

Are *all* the audible elements of speech traceable in these records? in other words, is the record complete? I am not prepared as yet to answer this question definitely, but the following experiment leads me to doubt whether an affirmative answer can be given, while at the same time it illustrates in a striking manner the sensitiveness of the ear. The mirror was attached to the disk of a *receiving* telephone and a photograph taken from it while the instrument was talking audibly. The resulting record was almost a smooth line, showing but very slight indications of movement of the mirror. It would therefore appear that there are distinctly audible elements, which are too minute to be recorded by this method. It is to be noted, however, that the width of the line traced where the vibrations are extremely small, is so great as to mask the curvature, so that the experiment just cited is not entirely fair.

The clearness and beauty of the curves obtained can hardly be appreciated without inspection of the originals. Their complexity and variety open a large field for investigation, and they seem to offer the means of analysis of articulate speech.

THE PHONOGRAPH AND VOWEL SOUNDS

I.—THE VOWEL SOUND Ō.

In a recent letter to NATURE we gave a short account of what we believed to be the existing theories of vowel-sounds. In the present paper we will state the chief results as to the vowel *ō* of our investigations made by means of the phonograph.

The experiments were made as follows:—The vowel under consideration was spoken or sung at a given pitch, determined by a piano, while the barrel of the phonograph was turned at a definite speed, regulated by means of a metronome. The indentations made in the tin-foil were then mechanically transcribed, so as to give curves representing a magnified section of the impressions. The curves were magnified by a system of compound levers, and recorded by an arrangement resembling that of Sir William Thomson's siphon-recorder. The details of the apparatus are described in a paper laid by us before the Royal Society of Edinburgh. The vertical ordinates of the curves drawn in ink, as shown below, are about 400 times larger than the corresponding indentations in the tin-foil, while the longitudinal ordinates are multiplied about seven times. The slowness of the motion by which the transcript was made enabled us to avoid all error due to inertia of the working parts, and the total absence of friction between the marking siphon and the paper allowed the transcript to be made without employing such a pressure on the tin-foil as would sensibly alter the indented curves. This fact was in each case tested by making the phonograph speak the vowel after it had been copied. All transcripts were rejected if the tin-foil did not continue to give the sound clearly after being used to produce these curves.

We employed various sizes of chamber and of mouth-piece, various thicknesses of tin-foil, and various discs as receivers. The curves now given as transcripts were found to be practically independent of variations in all these conditions. We are therefore of opinion that the curves do really represent the motion of an air-particle when the vowel is spoken, and that these curves may be regarded as sufficiently unaffected by any periods of vibration proper to the disc and springs employed, or to the air in the chamber of the mouth-piece, to constitute a true record of the essential parts of the vowel-sounds. This may be inferred from the remarkable constancy in the results obtained, with great variations in the conditions of the experiment, and from the fact that the indentations, after being copied, were in each instance able to give back the vowel-sound distinctly.

Fig. 1 gives a series of curves produced by a single baritone voice singing *ō* on a series of notes ranging from G to *f′*. This series has been selected because the voice was of good quality and considerable range.

After the curves were obtained they were subjected to harmonic analysis. One period was divided into twelve equal parts, and twelve ordinates were drawn and measured at right angles to a line joining two successive maximums or minimums. The numbers so determined enabled us to calculate the amplitudes of the first *six* partial tones.

Table I. gives these amplitudes for the above series of *ō*'s, obtained by analysing one period chosen out of the hundreds of similar periods which were given by each utterance. An examination of the curves and of the table will show that the change in character from note to note is fairly gradual and consistent throughout. The figures are arranged so as to show the absolute pitch of each of the six partial tones.

Voices of very different qualities were tested in the same way throughout the same range or such parts of that range as were within their compass.

Too much space would be taken up if we were to give here all the results obtained. It may be briefly said that the several voices agreed very fairly in respect of the partials composing the vowel at each pitch, that is to say, throughout the range

TABLE I.—*Amplitudes of the Harmonic Constituents of the Vowel Sound ō Sung at Various Pitches by the same Voice.*

12·1	7·1	0·7	0·1	0·5	0·4
10·5	6·9	0·7	0·3	0·2	0·3
11·9	7·6	0·5	1·0	0·3	0·2
11·0	16·0	1·5	1·0	1·0	0·7
7·0	12·6	1·5	1·4	0·6	0·1
7·5	18·5	1·3	0·8	1·1	0·1
12·5	19·0	2·5	2·2	0·5	0·2
6·9	10·3	2·7	0·6	0·2	0·2
5·5	14·0	4·5	0·4	0·8	0·1
7·9	16·5	7·4	0·8	1·0	0·3
4·4	13·4	8·2	1·6	2·2	1·0
1·8	9·5	6·1	3·3	0·3	0
2·5	1 5	2·8	3·1	0·6	0·5
3·7	5·8	6·1	4·7	1·1	0
1·5	1·5	1·8	2·9	0·6	0·3
1·3	0	1·5	4·0	0·8	0·4

G G♯ A B♭ B c c♯ d e♭ e f f♯ g g♯ a b♭ b c' c♯' d' e♭' e' f' f♯' g' g♯' a' b♭' b' c'' c♯'' d'' e♭'' e'' f'' f♯'' g'' g♯'' a'' b♭'' b'' c''' c♯''' d''' e♭''' e''' f''' f♯''' g''' g♯''' a''' b♭''' b''' c^{iv}

from *f'* to *g* the first and second partials are the main components; from *f* to *d* the third partial is prominent, and about *c* the fourth partial becomes strong. At G, in one voice, the fifth partial was strong, whereas in the example now given it was weak.

When the several voices were compared in respect of the relative strength of the partials, it became clear that this relative strength might vary greatly without affecting the vowel quality much.

This is brought out by Table II., which contains the constituents of *ō* as sung by five different voices on five different notes. The figures are proportional to the actual amplitudes of each harmonic constituent, and therefore not only show the relative value of each constituent in a given vowel, but also indicate (although only very roughly) the strength of the several voices on each note. Those constituents which are most considerable are printed in large type. Those printed in small type may perhaps be regarded as accidental and not essential to the vowel sound.

On *e'* the vowel sound is composed of the two first partials, but with different voices these were present in proportions which varied from $\frac{100}{102}$ to $\frac{100}{34}$.

On *c'* the vowel sound was still composed almost wholly of the two first partials, and with different voices these were present in proportions which varied from $\frac{100}{145}$ to $\frac{100}{46}$. From this it is evident that the vowel sound may vary very slightly as a vowel when the proportions of its constituents vary largely. No doubt it may be said that the vowels pronounced were different vowels, but it must be remembered that each man was at least trying to sing the same vowel and was controlled by the same observers, who were satisfied that he did sing generically the same vowel.

On the other hand, the quality of the voices was extremely different. Voice No. 1 was a fine, powerful, trained baritone. No. 2 was a high-pitched voice with no notes below *f*, rather harsh, and unaccustomed to singing. No. 3 was the trained bass voice of a man of eighty. Nos. 4 and 5 were voices of average range and moderate power, with some musical training. No. 6 was a powerful bass. The differences in the proportions of the constituents appear to us therefore to indicate differences in the quality of the voices rather than differences in the vowel.

On *c'* the two first partials give a good *ō*, whether the second is one and a half times as big as the prime or the prime twice as big as the second.

The results on *a* and *b* do not differ much from those on *c'*; when, however, we come to *g*, the table shows us that while two out of the five voices formed their *ō* by producing two partials as in the case of the higher notes, three other voices introduced a considerable third partial. On descending one tone lower, to *f*, we find that all the voices agree in employing a strong third partial. This third partial is sometimes less than half the prime, but it is never so small as one-third, and in one case it is sensibly larger than the prime. It would also be observed that it was the voice No. 5, which had no sensible third on *g* which gave the largest third on *f*. It should also be noted that the smallest third is due to voice No. 4, which was at the end of its range, and spoke or sang half a tone sharp.

As we descend below *f* the third assumes more and more prominence; on *d* voices 1 and 5 gave the following series of partials:—

	I.	II.	III.	IV.	V.	VI.
1°	4·4	13·4	8·2	1·6	2·2	1·0
5°	3·3	7·2	5·6	0·5	0·7	0·2

Table II. shows that by the time B is reached the fourth partial has become very prominent.

TABLE II.—*Harmonic Constituents of ō as Sung by different Voices.*

Note on which the vowel was sung.	No. of voice.	Amplitudes of the first six partial tones. I.	II.	III.	IV.	V.	VI.
e'	1	10·5	6·9	0·7	0·2	0·3	0·2
	2	5·1	3·0	0·5	c·2	0·1	0·1
	3	5·3	1·8	0·3	0·1	0·2	0·1
	4	5·5	3·4	0·7	0·2	0·2	0
	5	5·2	5·3	0·5	0·6	0·5	0·2
d'	1	11·0	16·0	1·5	1·0	1·0	0·7
	[b] 2	[4·5	6·6	0·7	0·4	0·6	0·2]
	3	3·7	3·0	0·1	0·4	0·1	0·1
	4	5·4	2·5	0	0·3	0·2	0·1
	5	4·7	4·1	0·5	0·3	0·3	0·2
g	1	6·9	10·3	2·7	0·6	0·2	0·2
	2	2·3	5·1	1·4	0·3	0·2	0·2
	3	4·6	2·9	0·2	0·2	0·2	0·1
	4	3·3	4·4	0·7	0·2	0·1	0·2
	5	3·2	5·0	0·3	0·6	0·1	0·2
f	1	5·5	14·0	4·5	0·4	0·8	0·1
	[$f\sharp$] 2	[1·8	5·8	1·5	0·3	0·1	0]
	3	2·5	3·7	1·1	0·3	0·4	0·2
	[$f\sharp$] 4	[2·8	6·2	1·0	0·2	0·3	0·2]
	5	2·5	2·8	3·8	0·4	0·5	0·1
B	1	2·5	1·4	2·8	3·1	0·6	0·5
	2	Wanting.					
	3	2·1	4·6	2·9	2·8	1·0	0
	4	Wanting.					
	5	0·6	3·8	2·3	2·5	0·6	0·3
	[B$\flat$] 6	[1·8	2·2	3·2	7·5	0·9	0·2]

NOTE.—The analyses put within brackets are a semitone different in pitch from the others in the group, and have their pitch marked at the side. They have been taken where there did not happen to be an example on the exact pitch wanted.

On G voice 1 gave the following series :—

1° 1·3 0 1·5 4·0 0·8 0·4,

On F another voice (No. 6) gave

6° 2·2 1·0 1·5 0·8 3·4 0·1.

In this last example we see that the fifth partial was much greater than the prime.

For the sake of brevity we have only given a few cases. Our results contain the analyses of more than a hundred curves, from which we have given what we think fairly representative examples. Moreover, the curves analysed were in many instances chosen from numerous examples, so as to represent not one experiment, but several. We will now state the results arrived at in somewhat more general terms.

1. At those pitches commonly employed in conversation the sound $\bar{o}$ consists essentially of the first two partials of the note employed. The proportions between these partials may vary between 1 : 1·5 and 1 : 0·5; perhaps even more widely. When curves obtained by speaking were compared with those obtained by singing the proportions were much more nearly alike than could have been expected. The spoken $\bar{o}$ differed chiefly from the singing $\bar{o}$ by a continuous change of pitch, and also by running into an $\bar{u}$ towards the end of the utterance.

2. When $\bar{o}$ is sung from g downwards the third, fourth, and fifth partials appear in succession in such a way as to keep that which is the highest prominent partial not far from $b'\flat$, which is called by Helmholtz the characteristic tone of $\bar{o}$. Above g the second and strongly reinforced partial lies in a region varying from three semitones below $b'\flat$ to six semitones above it.

3. Until we reach the lowest notes G and A all the partials between the prime and the highest prominent partial are more or less reinforced. Thus $\bar{o}$ successively consists of the first *two* partials, the first *three* partials, and the first *four* partials. On very low notes we have insufficient data for very positive conclusions, but the second partial seems to sink into insignificance as the fifth comes in.

4. The appearance of new partials as we descend the scale is in some cases and with some voices gradual; in other cases it is singularly abrupt.

Wave-form of ō Sung by the same Voice at Various Pitches.

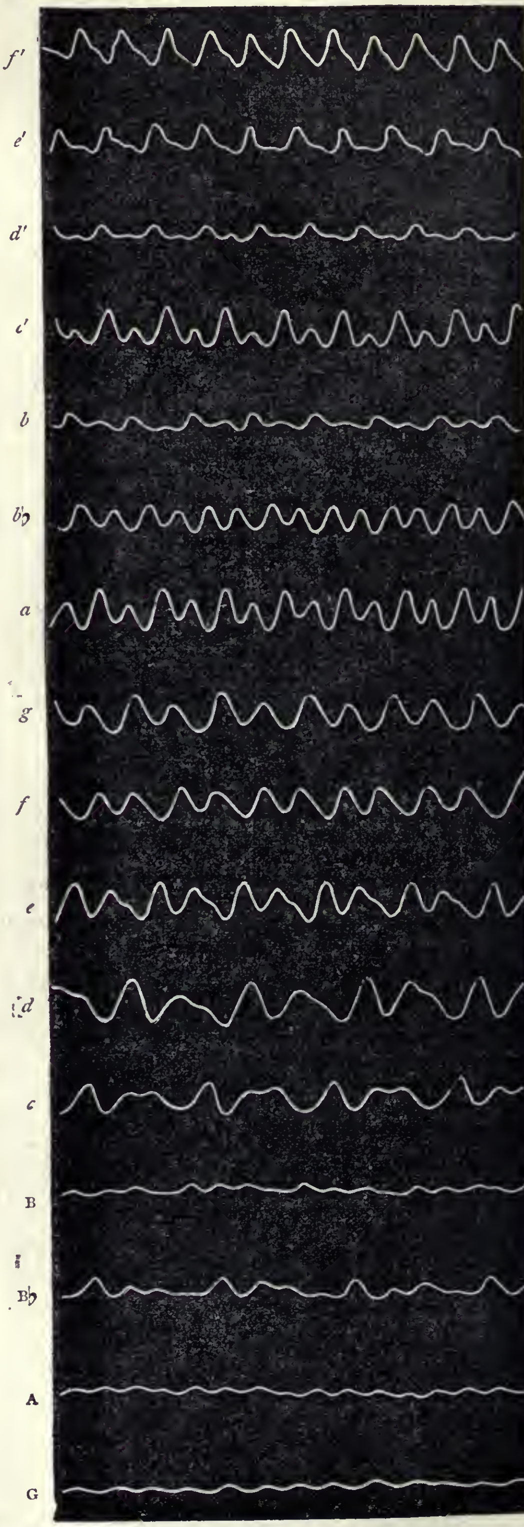


FIG. 1.

This is shown by Tables III. and IV., which give the actual

amplitudes of the third and fourth partials for voices 1, 3, and 5 at various pitches. The place where a somewhat sudden change happens is marked by a vertical bar.

TABLE III.—*Third Partials.*

Pitch of third partial.	*g''*	*f''*♯	*f''*	*e''*	*d''*	*c''*	*b'*	*a'*	*g'*	*f'*♯	*f'*	*e'*
Voice 1	1·5	1·5	1·3	2·5	2·7	4·5	7·3	8·2	6·1	2·8	6·1	1·1
„ 3	0·1	0·2	—	0·4	0·2	1·1	│3·5	—	4·6	2·9	2·5	3·5
„ 5	0·5	0·2	1·3	1·0	0·3	│5·8	6·4	5·6	—	2·3	1·5	2·2

TABLE IV.—*Fourth Partials.*

Pitch of fourth partial.	*c'''*	*b''*	*b''*♭	*a''*	*g''*	*f''*	*e''*	*d''*	*c''*	*b'*	*b'*♭	*a'*
Voice 1	1·0	1·4	0·8	2·2	0·6	0·4	0·7	1·6	3·3	3·1	4·7	2·9
„ 3	0·4	0·4	—	0·4	0·2	0·3	0·5	—	│3·5	2·8	3·6	3·9
„ 5	0·3	1·4	0·7	0·8	0·6	1·3	1·3	0·5	—	│2·5	1·5	2·1

It will be seen that the third partial springs into prominence more suddenly with voices 3 and 5 than with voice 1, but there is a rapid increase with voice 1, where the sudden change occurs with the other voices. This want of continuity suggests an adjustment or tuning of the mouth cavity, an idea which receives support from experiments on the sound *ū*, to be afterwards described.

Another very marked example of this sudden introduction of a new partial was given by voice 6, a powerful bass.

Note sung	B♭	A	G	│F
Fourth partial ...	7·5	8·0	4·5	│0·8
Fifth partial ...	0·9	1·2	0·9	│3·4

The apparent tuning of the mouth cavity required to produce the sudden introduction of a new partial might perhaps be described as the involuntary selection of a new *ō* of different quality, but better adapted to be sung on the new note. We do not reject this mode of considering the phenomenon, but at the same time point out that there is a generic property common to the *ō*'s above and below the critical notes which leads us to regard all the varieties as sensibly one and the same vowel sound. Our experiments do not so far show whether any tuning takes place in the upper part of the scale, where nothing but a prime and its octave are present.

5. Whenever a partial falls on *b'*♭ we find that it is specially prominent. This fact was ascertained by Helmholtz; and the confirmation of his experiments by a completely different method is very satisfactory, both as showing that the letter *ō*, as pronounced by the English and German singers, did not differ sensibly, and as tending to justify our confidence in the method of investigation which we have followed. In that part of the scale where the vowel consists simply of the prime and its octave, the second partial, when it falls on *b'*♭, is usually a maximum, both absolutely and relatively to the prime. This result was to be expected from the experiments of Helmholtz and Donders.

6. The upper partials are often larger in amplitude than the prime.

The second partial was occasionally a little more than double the prime; the third partial in low notes was sometimes four times as great as the prime. The fourth partial was in one case eight times as large as the prime and the fifth partial in our single example on F was one and a half times the prime.

We defer drawing any conclusions from these results until we have described the analogous phenomena observed for the vowel sound *ū*.

FLEEMING JENKIN
J. A. EWING

NOTES

PROF. F. V. HAYDEN has recently been elected Foreign Member of the Royal Academy of Sciences, Rome. This is one of the oldest scientific societies in the world, and the number of foreign members in the department of the natural sciences is only thirty-seven. The University of Rochester (U.S.) at its annual commencement on the 3rd instant, conferred on Dr. Hayden the honorary degree of Doctor of Laws. In his letter communicating its action, the president, Rev. Dr. M. B. Anderson, says:—"The Trustees and Faculty of the University desired to recognise by this action your great services to science through your personal attainments, as well as the almost unparalleled energy and success which have characterised your explorations in regions hitherto unknown to the geologist as such. They did not hope to add in the slightest degree to your well-earned reputation, but they desired to express their appreciation of the honour you have done our country and the state and city of your early residence, by those great labours which have linked your name with geological science for all coming time. The friendship of many years has made my official connection with this public act a source of the most sincere pleasure."

THE Netherlands Zoological Society held its annual summer meeting at Harlingen on July 14 last. The chairman of the Committee for the Zoological Station, Dr. Hubrecht, gave a short statement showing how the prospects of the young institution had considerably improved during the past year, how an annual income of about 150*l*. had been obtained, which had permitted an extension of the wooden building, described and figured in NATURE, as well as the application of a new heated air motor (system Rennes, Utrecht) for oxygenising the sea-water in the aquaria by a constant stream of atmospheric air. The transportable Zoological Station has been erected during the summer months of 1878, on the Island of Terschelling, between the Zuyder and North Seas, and several members of the Society are there now, for the greater part occupied in the study of the invertebrate fauna of the Dutch coast. These investigations are being considerably facilitated by the great liberality of the Marine Minister, who has put a vessel with a mate and three sailors at the entire disposal of the Committee for the next six or eight weeks. As has hitherto been done, it is expected that at the end of the season a circumstantial report will be issued by the Committee, in which the results of this year's campaigning will be duly recorded.

THE death is announced of Dr. Thomas Oldham, who was, from its origination in 1850 till 1876, at the head of the Geological Survey of India. It was under his direction that the official geological survey was commenced, first under the Hon. East India Company and afterwards as part of the Government public service; and until his retirement, from ill-health, he had the control of the issue of the official geological maps, as, portion by portion, the work was completed, and the issue of the memoirs explanatory of the maps. In 1861, eleven years after the survey was commenced, he originated the publication of the folio-sized "Palæontologia Indica," which consisted of plates, with descriptive letter-press, illustrating the fossils of the country, and the work has progressed steadily in fasciculi as an official publication printed by command of the Governor-General of India. Dr. Oldham was born in Dublin in May, 1816, and entered at Trinity College, Dublin, at the age of sixteen. After taking his B.A., he studied in 1837-38 at the engineering school of Edinburgh, and attended Jameson's lectures on geology and mineralogy. Returning to Ireland in 1839, he became chief geological assistant to Major-General Portlock, then at the head of the survey of Ireland, and he helped in the preparation of the well-known report on Londonderry, Tyrone, &c., published in 1843. After being for a while curator and assistant-secretary of the Geological Society of Dublin, he held for a year the professorship of engineering, and in 1845 succeeded the late Prof. John Phillips as Professor of Geology. He was then appointed local director of the geological survey of Ireland, and the Geological Society of Dublin elected him its president. After the various experiences thus gained, in 1850 he was appointed to organise the geological survey of India. There were many unexpected difficulties to

contend with, but during the sixteen years of his office these were more or less overcome. Besides being elected a Fellow of the Royal Society in 1848, he received the Royal medal of the Society in 1875. The Emperor of Austria conferred on him a medal in recognition of his work. The papers he wrote, apart from his official work, were not numerous. He died at Rugby July 17.

IT has been arranged that Prof. McKendrick, as President of the Physiological Section at the meeting of the British Medical Association in Bath, will give an address on the recent progress of acoustics, more especially as regards the mechanism of the ear.

AT a meeting recently held at Netley of the subscribers to the Parkes Memorial Fund, it was resolved—1. That a prize of one hundred pounds in money, and a large gold medal bearing the portrait of the late Dr. Parkes, be given triennially for the best essay on a subject connected with hygiene, to be declared at the commencement of each triennial period, the prize to be open to the medical officers of the army, navy, and Indian services of executive rank on full pay (with the exception of the officers of the Army Medical School during their term of office). 2. That the subject for the first competition for the above-named prize be as follows:—"On the Effects of Hygienic Measures in arresting the Spread of Cholera." 3. That the essays be sent in to the Committee of the Parkes Memorial Fund, care of the Director-General, Army Medical Department, 6, Whitehall Yard, London, S.W., on or before December 31, 1880. Each essay to have a motto, and to be accompanied with a sealed envelope bearing the same motto, and containing the name of the competitor. 4. That a bronze medal (also bearing the portrait of the late Dr. Parkes) be given at the close of each session of the Army Medical School to the best answerer at an examination in hygiene.

AT p. 104 of this volume we called attention to an additional exception to one of Fermat's remarkable statements regarding the forms of primes. The discoverer, M. Pervouchine, has lately succeeded in showing that

$$2^{2^{23}} + 1$$

(a number containing many more than *two millions and a half* of places of figures) is divisible by the prime number

$$167,772,161$$

or

$$5 \cdot 2^{25} + 1.$$

This result has been verified by Zolotareff of the St. Petersburg Academy of Sciences. We are not told what method he employed, but it is obviously reduced to a question of mere labour by the use of the binary scale. And even this labour may be dispensed with by the aid of very simple machinery. It is much more difficult to see how M. Pervouchine was led to choose this divisor, though it would appear that the divisor was probably first assumed and the dividend calculated from it.

THE annual meeting of German Archæologists and Historians will take place at Marburg about the middle of September.

THE American Minister for Agriculture has recently stated that in the extensive caverns of Texas enormous masses of guano are deposited. The quantity is estimated at 20,000 tons, and the quality is said to be superior to that of fish guano. Its origin must be looked for in the immense numbers of bats which inhabit these caverns. It is also reported that in the Indian Ocean several guano islands have been discovered, so that the threatened exhaustion of guano deposits need not be feared for some time to come.

IN different parts of Costa Rica grasshoppers have appeared in alarming masses, particularly near Herodia, Alajuela, and Atenas, one of the most cultivated and fertile districts of the whole country. The coffee crop for this season has been nearly all destroyed by the plague.

AT a recent meeting of the Geneva Society of Physics and Natural History, Prof. Alph. de Candolle presented a glass jar containing fruits of the coffee plant collected before maturity in Mexico, preserved in a liquid which chemical analysis proved to be salt water. It is fifty years since the jar thus filled was hermetically sealed, under the eyes of Aug.-Pyr. de Candolle, and to-day the coffee-beans which it contains are in a thoroughly satisfactory state of preservation. The water contains a solution of chloride of sodium and very small quantities of other chlorides or salts. No gas was found in solution; the water must then have been boiled, and introduced while hot into the jar. This experiment may give valuable hints as to the substitution of salt water for alcohol (of which every one knows the inconvenience) for the preservation of organic substances.

THE Japanese Government have finally authorised the immediate commencement of a line of railway between Kiôtô and Otsu, which is expected to cost nearly a quarter of a million sterling, and will probably be completed in three years. The construction of this line will have a beneficial effect upon that part of the empire, as it will afford a much needed outlet for the valuable products raised at Tsuruga, and in the rich districts in the neighbourhood of Lake Biwa.

THE Government engineering works at Shindin are a conspicuous proof of the enterprise of the Japanese, and it is satisfactory to learn from the Japan papers that the undertaking is in a highly prosperous condition. It was formed by combining the Kaga Foundry, originally started by the Daimio of Kaga in 1869, with the Vulcan Iron Works, which were bought by the government in 1872. The foreign staff at present consists of only four persons, and the works give employment to nearly a thousand skilled mechanics, exclusive of ordinary labourers. In addition to several works which have been recently executed, there are said to be sufficient orders on hand to occupy the staff for the next three years. The evidence which this establishment exhibits of the rapid development of internal trade is very satisfactory to all who watch with interest the progress of Japan.

"NOMENCLATOR STRATIGRAPHICUS: a Hand-book of the Nomenclature of the Sedimentary Rocks," by G. A. Lebour, F.G.S., is the title of a work which has been in hand for several years, consisting of a list—as complete as may be—of the subdivisions of the geological scale now or at any time in use in this country or abroad. The names are arranged in alphabetical order as the easiest for reference. The date of publication, the meaning when it seemed necessary, and the equivalence, are also given. The volume will be of at least 250 pp., and will be published as soon as the number of subscribers has reached 200. Information may be obtained from Mr. G. A. Lebour, 2, Woodhouse Terrace, Gateshead-on-Tyne.

THERE are very few botanical gardens, colonial or foreign, that can boast of such a carefully-prepared or extensive catalogue as that which Dr. Schomburgk has pronuced of the plants under cultivation in the Government Botanic Garden, Adelaide, South Australia, now before us. It comprises 285 pages; and not alone on the score of bulk, but also with regard to its contents, it is something more than a mere catalogue. The plants are arranged under their natural orders, the scientific and common names and native countries being given also. We are told whether the plant is a tree or shrub, a climber, a trailing, or a creeping plant, annual, biennial, or perennial, evergreen or deciduous, out-door or stove-plant. Besides all this are too good indices, one of English and the other of Latin names. From the preface a very good idea may be had of the climate and meteor-

ology of Adelaide, as well as of the behaviour of introduced plants from various parts of the world. The sudden changes of temperature during the Australian summer months of December, January, and February, are often very injurious to vegetation. The lengthened period of eight or ten weeks without a drop of rain, which is not uncommon, has a serious effect upon both indigenous and acclimatised plants. In the months of March, April, and May, when our own deciduous trees are putting on their fresh green foliage, the same identical European plants which have established themselves in their Australian home are assuming their autumnal tints and dropping. Alpine and tropical plants suffer in South Australia not only from the dry atmosphere, but—the tropical ones especially—from the cold of the winter months. On the other hand, the extreme heat in the month of January, coupled with the hot north wind, literally bakes the fruits upon the trees. At three o'clock in the afternoon of the 10th of January last, it is stated that the thermometer in the Botanic Garden registered 116° in the shade and 166° in the sun. The catalogue is illustrated by seventeen full-page views in the garden, engraved from photographs.

WE have before us quite a pile of *Reports* and *Proceedings* of provincial societies, all of which, we may say, appear to be in a prosperous and healthy condition. We can do little more than give the names of the societies which have issued these reports. As usual, the *Natural History Transactions* of Northumberland, Durham, and Newcastle-on-Tyne contain some papers of great importance. A paper on Eggs, by Dr. Embleton, and one on Roman remains at South Shields, by the Rev. Dr. Hooppell, deserve special mention; there are also some interesting Bewick letters. The preface to the *Report* of the Rugby School Society is rather desponding, but the contents are really creditable to the contributors, and we are glad to see the attendance is generally very good; in the Report on the Temple Observatory, a description and plan of the new buildings is given. The eighth Report of the Wellington College Society shows it to be in a state of vigorous activity, all the departments adding largely to their collections; an ethnological department has been set on foot. Besides the above we have received the *Proceedings* of the Belfast Natural History and Philosophical Society; the *Report and Proceedings* of the Manchester Field Naturalists and Archæologists; *Proceedings* of the Birmingham Philosophical Society; *Eighth Annual Report* of the Leeds Naturalists' Club; *Annual Report and Transactions* of the Plymouth Institute and Devon and Cornwall Natural History Society; *Proceedings* of the Liverpool Naturalists' Field Club; *Seventh Report* of the Croydon Microscopical Club; *Report* of the Northampton Natural History Society and Field Club; and the *Eighth Annual Report* on the Devon and Exeter Albert Memorial Museum, &c. Several of these publications contain really important papers which deserve a wider circulation than they are likely to receive in their present form. In this connection we may mention an interesting tractate published at the *Advertiser* office, Wilmston, containing an account of some Lancashire Artisan Naturalists, by Mr. A. A. Reade. From abroad we have received the *Papers and Proceedings* of the Royal Society of Tasmania; *Proceedings* of the Linnean Society of New South Wales; *Report* of the Auckland Institute; *Report* of the Dunedin Naturalists' Field Club, and the *Bulletin* of the Essex (U.S.) Institute.

THE temperature of flames has been investigated by Signor F. Rosetti (*Istituto Veneto*, ser. v. vol. iv.) in a very thorough manner by means of his ingenious calorimeter. The maximum temperature of a Bunsen flame is found to be 1,360° C., and results from a combustion of 1 volume of gas and $2\frac{1}{2}$ volumes of air. The admission of a greater or less quantity of air reduces the temperature. Changes in pressure have but slight influence on the temperature. The flame given by gas diluted with the same volume of nitrogen shows a temperature of 1,180°, and diluted with 3 volumes of nitrogen, 1,040°. The same degrees of dilution with carbonic acid show respectively 1,100° and 780°. Among other temperatures noted were the following:—

	°
Locatelli lamp	920
Stearine candle	940
Petroleum lamp with chimney	1,030
The same without chimney—	
Illuminating part	920
Sooty envelope	780
Alcohol lamp (alcohol 0·912)	1,170
Ditto (alcohol 0·822)	1,180

The slight difference in heating power resulting from widely-varying percentages of water in the alcohol is worthy of remark.

THE medical students of Paris have not forgotten that Rousseau was a botanist as well as a philosopher, and sent a delegation on July 2 to Ermenonville to celebrate the 100th anniversary of his death. Three addresses were delivered in the name of the medical body—one by Dr. Bergeron, the toxicologist, the second by M. de Lannessau, and the third by M. Baillon, himself a botanist and a professor of the School of Medicine. The speakers referred in eloquent terms to the love of Rousseau for nature, his observational genius, and his works on botany. The students had prepared a splendid crown made of *pervenches* (periwinkle, *Vinca*) the flower which Rousseau loved best, and which had been collected by them in the very forest where the philosopher spent his last years. As no boat was to be had to reach the island where the author of the "Nouvelle Héloïse" is buried, one of the students threw himself into the water and swam with the testimonial to the spot sacred to the memory of the impulsive Frenchman.

H. J. RINK has recently laid before the Dutch Academy of Sciences an elaborate paper on the alterations caused by changes of temperature in the resistance offered by mercury to the passage of the galvanic current. The coefficients found hitherto range between 0·00086 and 0·00104. The author has made experiments with seven tubes of mercury, each a metre in length, and after making all corrections for expansion of glass, &c., obtained the number 0·000989 as the coefficient for the change in the resistance corresponding to an alteration of a degree Celsius. He finds, furthermore, that the resistance increases in a more rapid ratio than the temperature.

THE great Giffard captive balloon is in the hands of a Commission appointed by the Prefect of the Seine, and composed of M. Troost, Professor of Physics at the Sorbonne, Capt. Renard, head of the balloon service of the War Office, and a few others. The Commission was appointed on July 19, and on the 20th paid its first visit to the balloon, which is attracting public notice to an unprecedented degree. Thousands of spectators look through the railings of the Cour du Carrousel at the stupendous sphere which is ready to start for its elevated station. On the 20th the wind was very violent, and no ascent was tried. The balloon will not be opened to the public before the Commission has rendered its report. A second visit took place on the 21st, when a successful trial ascent was made. A M. Carrol has designed and made wings for directing an elongated balloon. A man will be suspended under it by a rope and will try to direct it. This kind of experiment has been tried at Paris twice—by Deghen, a Viennese clockmaker, about seventy years ago, who failed; and a year ago at la Villette gasworks, by a policeman, who obtained no result. M. de Fonvielle writes that he visited the Carrol flying machine which is yet imperfect, but may eventually work. The balloon will be inflated with hydrogen gas, and the

man engaged to work the wings is an acrobat of effective muscular power. The experiment will very likely take place at Enghien, on the lake, where the balloon will be retained by a small floating buoy.

We have received a "Catalogue des Ouvrages d'Astronomie et de Météorologie," found in the principal libraries of Belgium, prepared at the Royal Observatory of Brussels. It extends to upwards of 630 pages, and will be found of great service to those interested in astronomy and meteorology. The publisher is Hayez, of Brussels.

In our report last week (p. 323) of the Physical Society meeting of June 22, in Mr. W. Baily's paper, the expression A cos θ should be A cos 2θ, so that the equation to the ellipse of polarisation would be

$$1 + A\cos 2\theta + B\sin 2\theta = r^{-2}\{1 - (A^2 + B^2)\}$$

The author of the paper on Complementary Colours was Mr. John Gorham, not Graham.

The additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mr. Enoch; a Rhesus Monkey (*Macacus erythrœus*) from India, presented by Miss Davis; a Cape Zorilla (*Ichonyx zorilla*) from Africa, presented by Mrs. J. J. Monteiro; a Common Cuckoo (*Cuculus canorus*), European, presented by Mr. G. D. Careless; three Alligator Terrapins (*Chelydra serpentina*) from North America, presented by Mr. J. H. Thompson, C.M.Z.S.; a Chimpanzee (*Troglodytes niger*) from West Africa, a Golden-headed Marmoset (*Midas chrysomelas*) from Para, two Egyptian Flamingos (*Phœnicopterus antiquorum*) from North Africa, deposited; an Eland (*Oreas canna*) born, five Amherst Pheasants (*Thaumalea amherstiæ*), an Argus Pheasant (*Argus giganteus*) bred in the Gardens.

JOSEPH BLACK[1]

The study of the history of a science is of great importance not only from a psychological point of view, but also as throwing light on the present position of the science. In science, as in other natural products which have *grown*, we find survivals which can only be understood when the development is known. Such historical studies may very conveniently be associated with the biographies of the great scientific leaders under whom progress has been made, and whose individual mental peculiarities have left permanent impressions. I intend on this occasion to direct your attention to the life and work of Dr. Joseph Black both because he was one of the first to give to chemistry the direction which it still preserves, and because his life is of special interest to us as Edinburgh students of chemistry.

Joseph Black was born at Bordeaux, in 1728. His father, John Black, was a native of Belfast, a member of a Scottish family settled in Ireland. His mother belonged to the family of Gordon, of Halhead, in Aberdeenshire, and was a cousin of Dr. Adam Ferguson. In 1740 he was sent home and educated at the Grammar School of Belfast. In 1746 he matriculated at the University of Glasgow, where he remained till 1750, studying in the faculties of arts and medicine. He then removed to Edinburgh, where he graduated as doctor of medicine in 1754. In 1756 he was appointed Professor of Anatomy and Lecturer on Chemistry in the University of Glasgow. He soon exchanged with a colleague the duty of teaching anatomy for that of physiology, and continued to lecture on physiology and chemistry till 1766, when he was called to Edinburgh to succeed his friend and teacher, Dr. Cullen, in the Chair of Chemistry. He died November 26, 1799. Such is a brief sketch of his quiet and uneventful life. His contemporaries Dr. Robison and Dr. Adam Ferguson, give us some account of his manner of life and study. He was minutely accurate and careful in everything he did, and this punctiliousness and his feeble health account for the small *quantity* of work of which he has left a record. As a student he is said to have kept two sets of note-books; into one he entered observations, experiments, hints of experiments, extracts from the works of others, in fact all the miscellaneous additions to his knowledge. These he afterwards transcribed into the other set, arranging them in order of subjects. "In short," to quote Dr. Robison, "he kept a journal and ledger of his studies and posted his books like a merchant." It has occurred to me that possibly this mention of Dr. Black's business-like habit may have been present to the mind of Sir Walter Scott when describing the interview of Francis Osbaldistone on his return from Bordeaux, with his father. "—but what have we here? 'Bordeaux founded, castle of the Trompette, palace of Galienus,'—well, well, that's very right, too. This is a kind of waste book, Owen, in which all the transactions of the day, emptions, orders, payments, receipts, acceptances, drafts, commissions, and advices are entered miscellaneously." "That they may be regularly transferred to the day-book and ledger," answered Owen; "I am glad Mr. Francis is so methodical."

His style as a lecturer is well described by Dr. Robison:—

"He endeavoured every year to make his courses more plain and familiar, illustrating them by a greater variety of examples in the way of experiment. No man could perform these more neatly and successfully. They were always ingeniously and judiciously contrived, clearly establishing the point in view, and never more than sufficed for this purpose. While he scorned the quackery of a showman, the simplicity, neatness, and elegance with which they were performed were truly admirable . . . his students were not only instructed, but (they knew not how) delighted; and without any effort to please, but solely by the natural emanation of a gentle and elegant mind, co-operating, indeed, with a most perspicuous exhibition of his sentiments, Dr. Black became a favourite lecturer." His private life was one of unvaried regularity and order, and was brought to a fit close by his death, which is thus described by Dr. Adam Ferguson.

"His own constitution never was robust, and every cold, or any approach to repletion, affected his breast so much as to occasion a spitting of blood. This he guarded against by restricting himself to a moderate or abstemious diet. As his infirmities increased with age, he met them with a proportionate attention and care, regulating his food and exercise by the measure of his strength; and thus preventing the access of disease from abroad, he enjoyed a health, which was feeble but uninterrupted, and a mind undisturbed in the calm and cheerful use of his faculties. A life so prolonged had the advantage of present ease, and the prospect, when the just period should arrive, of a calm dissolution. This accordingly followed on the 26th of November, 1799, and in the seventy-first year of his age, without any convulsion, shock, agitation, or stupor, to announce or retard the approach of death. Being at table with his usual fare—some bread, a few prunes, and a measured quantity of milk diluted with water, and having the cup in his hand when the last stroke of his pulse was to be given, he appeared to have set it down on his knees, which were joined together, and in this action expired, without spilling a drop, as if an experiment had been purposely made, to evince the facility with which he departed. So ended a life which had passed in the most correct application of reason and good sense to all the objects of pursuit which Providence had prescribed in his lot." . . . "He had long enjoyed the tender and affectionate regard of parents whom he loved, honoured, and revered, with the delightful

[1] Abstract of Lecture to the Edinburgh University Chemical Society, by Prof. Crum Brown, F.R.S.

consciousness of being a dutiful son; one of a family remarkable for sweetness of disposition and manners, he had lived with his brothers and sisters in terms of mutual love and attachment. He had never lost a friend but by the stroke of mortality, and he felt himself worthy of that constancy of regard. He had followed a profession altogether to his taste, and had followed it in a manner, and with a success which procured him the esteem and respect of all competent judges, and set his name among the most eminent, and he was conscious that his reputation was not unmerited; and with a success, in respect of emolument, which secured the respect even of the ignorant; which gave him the command of every rational gratification, and enabled him to add greatly to the comforts of the numerous descendants of his worthy parents—heirs not only of their name, but likewise of their unambitious moderation and amiable simplicity of character."

Such was Dr. Black as described by those who knew him intimately. We at a greater distance from him can, perhaps, more accurately estimate the character and value of his work. This may be considered under the heads of his three great discoveries. 1. The nature of the difference between the mild and the caustic alkalies; 2. The latent heat of liquids; 3. The latent heat of vapours.

As a student of medicine in this University his attention was early drawn to the chemical characters of caustic potash and caustic soda, the merits of which as remedies in cases of urinary calculus were then much discussed.

Two kinds of alkalies, the caustic and the mild, had long been distinguished. The former act in a burning, caustic, destructive way on animal and vegetable tissues, the latter do not; the latter effervesce when mixed with acids, the former do not; the former are typified by quick or in slaked lime, the latter by chalk or calcareous earth. Previous to Dr. Black's experiments the difference was thus explained:—When calcareous earth is burnt it becomes quick-lime by taking up from the fire a fiery, caustic matter; some of this is given off as heat when lime is slaked, but some of it remains and gives the causticity by which slaked lime is distinguished from calcareous earth. This causticity is transferred (because the caustic matter is transferred) from the lime to other alkalies. Thus, when slaked lime is mixed with a solution of potashes we obtain caustic potash and the lime becomes mild, is re-transformed into calcareous earth, having parted with its *causticum* to the potash. Similarly when sal ammoniac is heated with calcareous earth we obtain sal volatile; but when we act on sal ammoniac with slaked lime the *causticum* passes from the lime to the volatile alkali and caustic ammonia is produced. In all these cases the caustic matter or *causticum* originally obtained from the fire was believed to be transferred from one alkali to another. The effervescence which occurs when a "mild alkali" is treated with an acid was of course observed, but it was looked upon merely as a symptom of the violent movements caused by the mutual saturation of acid and alkali.

When slaked lime is exposed to the air it gradually returns to the condition of calcareous earth. On the hypothesis stated above, it must therefore gradually give off its *causticum* into the air. Black's first experiment seems to have been an attempt to catch the *causticum* as it escaped. We have no details of these early experiments, but from a note-book which can be shown to be of the date 1752 Dr. Robison extracts the following statement of the result:—"Nothing is given off, the cup rises considerably by absorbing air." Another memorandum occurs a little later: "When I precipitate lime by a common alkali, there is no effervescence. The air quits the alkali for the lime, but it is not lime any longer, but C.C.C. It now effervesces, which good lime will not."

A full account of his experiments and conclusions is contained in his graduation thesis (1754), and in a more extended form in 1756 in "Essays and Observations, Physical and Literary, read before a Society in Edinburgh"—the society which afterwards became the Royal Society of Edinburgh. In this classical paper he shows in the clearest manner that the mild alkalies differ from the caustic by containing in addition a large quantity of "fixed air," a particular kind of gas, which we now know as carbonic acid gas. This gas is given off, causing effervescence when the mild alkali is dissolved in an acid, and the caustic alkali does not effervesce because it does not contain fixed air. Wherever causticity is acquired this fixed air is lost, and *vice versâ*. When slaked lime is mixed with a mild alkali the lime takes the fixed air, and is converted into calcareous earth, while the mild alkali by the loss of fixed air is rendered caustic. In the same way sal ammoniac with calcareous earth gives a mild volatile alkali, the fixed air being transferred from the lime to the ammonia, but with slaked lime a caustic ammonia, because there is here no fixed air to be transferred. The origin of the causticity in the lime is shown to be due to the loss of fixed air which the heat separates from the limestone, and the loss of weight which is observed when limestone is burnt is shown to be exactly accounted for by the loss of the fixed air. Thus Black proved the "causticum" to be *minus* fixed air. Addition or subtraction of the former is really subtraction or addition of the latter, and transference of *causticum* from A to B is really a transference of fixed air from B to A.

It is impossible to look at such a sketch of this part of Black's work without being struck with the resemblance between the theory of causticity which he overthrew, the nature of the truth which he discovered, and the method by which he discovered it, on the one hand, and on the other the theory of Phlogiston, the true nature of combustion, and the method by which it was discovered by Lavoisier; indeed, Lavoisier himself, in a letter to Black, speaks of the new chemistry as "Une carrière que vous avez ouverte, et dans laquelle nous nous regardons tous comme vos disciples."

The discovery of the latent heat of liquefaction and of vaporisation, was made by Dr. Black while professor in Glasgow. I have occupied so much time with the purely chemical part of my subject that I shall only here point out: (1) That Black's determination of the latent heat of water agrees very closely with the most recent results of experiments conducted with all the refinements of modern science; (2) That he studied the fusion and solidification of bodies, such as resin and sealing wax, which pass *gradually* from the liquid to the solid state, or *vice versâ*; and (3) That it was his teaching which induced Watt to commence the series of experiments and speculations which led to the discovery of the dependence of the latent heat of steam upon the temperature, and to the invention of the condensing steam-engine.

A SCHOOL LABORATORY

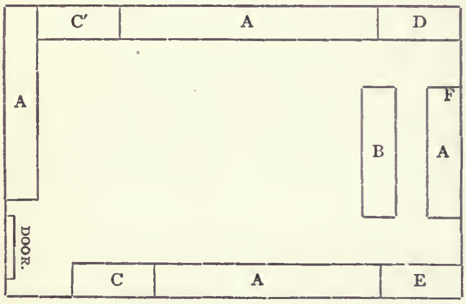
FACTS, not theories. This is the special point in the recent "Head Masters' Report" on science teaching, and, by his interesting account of it in NATURE, vol. xvii. p. 317, Mr. Tuckwell has afforded some opportunity of counting the cost to head-masters projecting a development of their science side, but apparently the Report gives no detail, and it is in this direction, perhaps, that many seek chiefly for information; they would like to know more fully what facilities may be obtained for a particular outlay.

In the hope of giving reliable information of this sort I submit the following particulars:—

The governors of Exeter School, hoping to rekindle the torch of science, lately so unhappily extinguished in the West by the Taunton College authorities, have recently

erected a chemical laboratory; it is intended for temporary use indeed, but affords facilities for school work almost rivalling those of the most costly appliances on Mr. Tuckwell's list, and at an expense not much greater than that of the cheapest; in fact, it has been arranged on a plan based upon experience of the two schools in question. I have said it is intended for temporary use; I do not mean that it is not calculated for a fair term of service; it is temporary because the present location of the school is not a permanent one.

Abutting a stone wall 10 feet high, there is built a room 30 feet by 26 feet, and about 16 feet to the ridge of the roof; it is lighted by skylights on each slope of the roof, and the remainder of the roof is boarded and felted. The building is of timber, floor of wood, and about 9 inches above the ground. The bare building thus described cost 88*l.* The interior arrangements are as shown by the accompanying plan:—



A A A A are working-benches giving accommodation for a class of eighteen or twenty at practical chemistry.

They are of $1\frac{1}{2}$ inch deal, supported on tressels 33 inches above the floor and are 25 inches broad, with an under-bench for holding apparatus, and with three shelves in front of them; these are 6 to 9 inches deep, the lowest hold reagent bottles, the next exercises for analysis, and the top shelves are devoted to lecture apparatus when out of use. They are 14, 30, 48 inches above the bench.

B, a lecture-table; its top is 12 feet long, 27 inches broad, $1\frac{1}{2}$ inch thick, beneath are arranged drawers and shelves; it stands 3 feet above the ground. There are two supplies of gas, one about the centre of the table, fitted with three taps, to which Bunsen burners are attached, another of much larger dimensions, for supplying a combustion furnace, which does double duty, for warming in very cold weather and lecture purposes at other times.

C C' are two slate sinks; C is 39 by 21 inches, and 6 inches deep, with three taps for water; C' is 32 by 21 inches, and 6 inches deep, with two taps; the tops of the sinks are about 2 inches above the level of the working-benches, and a third tap to C' serves to supply the condenser of a still.

D is a fume closet 51 inches long, 23 inches deep, and 64 inches high; it is ventilated by a zinc flue about 6 feet high, under which a jet of gas is burnt when necessary; its doors are 4 feet high and glazed, and the roof slants back from the top of this to the wall.

E is a cupboard 48 inches long, 26 inches deep, 75 inches high, with glass doors; inside are shelves arranged for holding apparatus likely to be damaged by the atmosphere of the laboratory. This and the fume-closet are made fairly substantially of deal and are painted or stained. Except the front, back, and sides of the lecture-table, which also are stained and varnished, all other wood-work is left bare.

Beneath B is another cupboard of plain deal, for chemicals, &c.; it is 33 inches high measuring from the ground.

Gas is supplied to the working benches by 17 jets from a 1-inch pipe, which is carried a few inches above the top of the bench round the room; the lecture-table, and a pendant with four burners for lighting purposes, are supplied by branches from this, and a third branch supplies the fume-closet.

There is a water-tap at F for lecture purposes, but no sink.

The cost of these fittings is as follows:—For gas and water fittings (the gas meter is a hired one), 21*l.* 14*s.* 7*d.*; for woodwork and sinks without water fittings, 51*l.* 3*s.* 3*d.* They are all fairly substantially made, as it is intended to move them to the laboratory to be built at the new school. This sum includes lecturer's desk and stool, waste boxes, &c. &c.

The apparatus and chemicals include a Ruhmkorff coil by Apps, giving $\frac{3}{4}$in. spark, combustion furnace, automatic copper-still, 8 lbs. mercury with suitable vessel for keeping it dry and pure under oil of vitriol, Becker's balance turning to $\frac{1}{10}$ grain, ozone generator, Bunsen's cells, fourteen doz. stoppered reagent bottles, about six or eight doz. bottles for holding solids and solutions for analysis, two doz. 5-pint stoppered bottles for holding stocks of solutions, blowpipe with Fletcher's bellows, a supply of Bunsen's burners, test-tubes, racks, chemicals, and all the other indispensables of a laboratory.

These cost, including carriage by rail, &c., somewhat under 47*l.*, which sum will serve for our wants during the first six months' work.

The total cost amounts to 207*l.* 17*s.* 10*d.*, to which we add about 10*l.*, not more, for some office expenses. For this sum we have a combined laboratory and lecture-room, which is calculated to be sufficiently capacious to afford instruction in theoretical chemistry to 70 per cent. of a school of 150 or 160 boys, and in practical chemistry to about 25 per cent. of the same number; also to serve as an occasional class-room for another science subject.

There is nothing handsome about this laboratory; externally it is tarred, and in summer it will be white-washed; inside it is chiefly innocent of paint, and its walls are unplaned; but for real work there is very little wanting.

W. A. SHENSTONE

A NEW DEEP-SEA THERMOMETER

THE most efficient deep-sea thermometer constructed up to the present has been the one known as Six's thermometer, with the bulb protected from pressure, as invented by Messrs. Negretti and Zambra, and described in NATURE, vol. ix. p. 387. This instrument has been extensively used by the expeditions sent out by various governments and scientific societies.

The disadvantages in the old instrument were the following:—1. The indices were, to a certain extent, unreliable, as, however carefully fitted, they were apt to slip down through their own weight, so that the observations were always more or less doubtful; 2. Its accuracy, even in its most perfect condition, did not attain to fractions of a degree, the closest readings differing at least half a degree; 3. The instrument had always to be carried in a vertical position, or it would become considerably deranged. As long as it was sufficient to obtain temperatures varying not less than a degree from one another the old instrument answered tolerably well in deep seas. Recently, however, the bottom temperatures of shallow seas and rivers have come under investigation, and for that purpose the instrument proved unsuitable.

The difference between the temperature at the surface of the sea and that at the depth of a few fathoms does not amount to a whole degree, as a rule, but only to fractions of a degree; hence the observations, to be of any value at all, must be made with an undoubtedly accurate and delicate thermometer. The investigation of the temperatures of the British seas has been urged upon the Government by naturalists and physicists interested in the question of the food supply of the people in its relation to fisheries. The questions of greatest importance were those regarding the influence of temperature upon the habits and migrations of fish, and the determination of the best seasons and temperatures for the development and capture of the various species. This investigation, commenced with the old instrument, has at present only shown that such instruments are not suffi-

ciently reliable for the purpose; a new, more accurate, and more delicate instrument was therefore a great desideratum, and it seems that Messrs. Negretti and Zambra have now solved this problem in a satisfactory manner.

The construction of the new thermometer will be readily understood by reference to Fig. 3. The bulb is cylindrical, and mercury is the thermometrical fluid. The neck of the bulb is contracted in a peculiar manner at A, and upon the shape and fineness of this contraction the success of the instrument mainly depends. Beyond A the tube is bent and a small catch reservoir is formed at B, for a purpose to be presently explained. At the end of the tube a small receptacle C is provided. When the instrument is held bulb downwards it is seen to contain sufficient mercury to fill the bulb, tube, and a part of the reservoir C, leaving sufficient space in C for the expansion of the mercury when the temperature rises. In this position no scale would be possible, as the apparent movement of the mercury would be confined to the space C. When the thermometer is held bulb upwards the mercury breaks off at A, but by its own weight flows down the tube, filling C and a portion of the tube above C, this portion being in proportion to the existing temperature. The scale accordingly is divided from C upwards. To prepare the instrument for observation it is only necessary to place it bulb downwards, the mercury of course assuming the temperature surrounding the instrument in the same way as any ordinary thermometer. When at any time or at any place it is required to read off the temperature, all that has to be done is to turn the thermometer bulb upwards, and to keep it in that position until the reading has been taken. This may be done at any time afterwards: for the quantity of mercury in the lower part of the stem which records the reading is too small to be appreciably influenced by a change of temperature, unless it be very considerable, while that in the bulb will continue to contract with greater cold and to expand with greater heat; and in the latter case some mercury will pass the contraction A and may fall down and lodge at B, but it cannot go further as long as the bulb remains upwards; and thus the quantity recording the temperature in the stem will remain unaltered. It is seen, therefore, that this new instrument is simply a recording thermometer, which accurately and delicately records the temperature at the time and place when and where it is turned over. It is not intended and cannot be used as a self-registering maximum or minimum thermometer.

Of course some contrivance must be provided for turning the thermometer bulb upwards at any depth in the water. For this purpose the instrument is fitted into a wooden frame loaded with shot, free to move from end to end of it, and with its weight so regulated as to render the whole just buoyant in sea-water.

In using the thermometer a cord is passed through the hole in the frame *nearest* the bulb, and the instrument is fastened by this cord to the sounding-line. In descending, the thermometer will be pulled down with the bulb downwards; but upon being pulled up, the instrument, owing to the resistance offered by the water, will turn over and come up bulb uppermost; the temperature of the spot where it turned over will then be indicated. The illustrations we subjoin will further elucidate this matter.

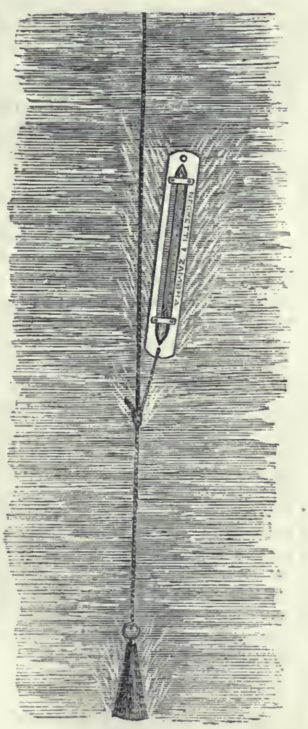


FIG. 1.—Descending.

FIG. 3.

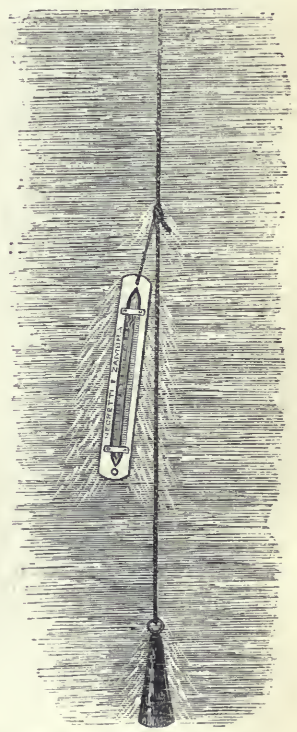


FIG. 2.—Ascending.

As regards the thermometer itself, it was necessary, in order to make it perfectly accurate, to protect it against pressure, even if intended for shallow seas, as well as for the deepest. For whether used in deep or shallow water, unless so protected, its indications would always be more or less faulty. Like an ordinary thermometer, it is devoid of air, and thus differs materially from Six's, which, containing compressed air, has a certain internal power of resistance. Hence the new instrument would be more affected by pressure than Six's, however thick the glass of the bulb. By the simple expedient of placing the entire thermometer into a glass shield or sheath, and hermetically sealing the latter, the effect of external pressure is entirely eliminated. The shield must of course be a strong one. It need not be exhausted of air. Its effect, however, will undoubtedly be to render the thermometer it protects against pressure less sensitive towards changes of temperature; in other words, it will make it sluggish. To counteract this sluggishness, some mercury is introduced into that portion of the shield which surrounds the bulb, and is confined there by means of a partition cemented in the shield round the neck of the thermometer bulb. The action of this mercury is that of a heat-conductor from the exterior of the shield to the interior of the thermometer, and the efficacy of this arrangement has been proved by experiment, the instrument thus protected being, in fact, far superior in sensitiveness to Six's thermometer.

As long as the shield withstands the pressure, the thermometer will be unaffected, and there is abundant evidence already to show that a shield of the above description will resist the pressure even at the bottom of the deepest ocean; doubtless it will be compressed a little at great depths, but the pressure will fail to have any appreciable effect upon the thermometer itself. This method of shielding is quite efficient, and thermometers thus protected need not be tested for pressure in the hydraulic press; all that is necessary is to test them very accurately for sensitiveness and for errors of graduation. The new instrument is intended to be a standard instrument and permits the reading off of at least two-tenths or even one-tenth of a degree. The test for sensitiveness determines how many seconds the instrument requires to note a change of 5° rise or fall, and the time has been found to be from five to ten seconds.

A considerable number of these instruments have already been tested at Kew Observatory with perfectly satisfactory results, which place beyond doubt their value as standard deep-sea thermometers.

Thus, provided the turning-over gear is found to answer, this

new instrument evidently possesses great advantages. It has no scale attached to it, the graduation and figures being distinctly marked on the stem itself, and the shield effectually preserves them from obliteration by sea-water. The back part of the stem is enamelled white, rendering the graduation and column of mercury extremely distinct.

When the instrument is immersed in the water the descending line may be stopped or checked any number of times, and it is of course quite immaterial in what position the instrument enters the water; the illustrations show at a glance that it will infallibly assume the position "bulb downwards" when descending rapidly, and all that is needed is that care should be taken in the pulling upwards. The first pull in this direction should be quick and sudden and be continued for some little time; at the same time the pulling upwards must be continuous, since stoppages would invalidate the readings.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

A MEETING of the members of the Yorkshire College of Science was held on Monday at Leeds. A sum of 56,000*l*. has now been promised in donations, and the endowments from the Akroyd Foundation, the Clothworkers' Company, and the bequest of the late Mr. Brown would, if capitalised, represent a further sum of about 20,000*l*. On the question of the proposed new university the committee reported that the college had held friendly communications with the authorities of Owens College, but could not at present make them the subject of a public report. Mr. Baines stated that the number of students this year was 355, as compared with 288 last year.

We have received a calendar of the Newcastle College of Physical Science, which contains full information concerning the curriculum at that institution, examination papers, scholarships, &c.

SCIENTIFIC SERIALS

Bulletin de l'Académie Royale de Belgique, No. 4, 1878.—In a further paper on the scintillation of stars, M. Montigny here deals with the changes of colour in stars of red and orange tints. From a table giving the general averages of relative frequency of the seven colours in such stars, it appears that the relative frequency of red much exceeds that of any of the other colours, whether in rainy or dry weather; that red, green, and especially orange, are in much greater proportion in dry than in rainy weather; while on the other hand, the frequency of blue and yellow is more marked under the influence of rain. Taking Pollux and Capella as samples of yellow stars, M. Montigny found in them the frequency of red and especially of yellow was much increased, while the proportion of orange was notably diminished. The proportion of blue was the same as in stars of the other type.—M. Masquelin contributes a valuable paper on the development of the inferior maxillar in man, in which he establishes the concurrence of the two modes of ossification in one bone, viz., that by the direct or metaplastic process, and that by the indirect or osteoblastic. It would thus appear that the histological process of ossification cannot serve to determine the morphological value of a bone.—A paper on oscillations of the Belgian coast, by M. Van Rysselberghe, aims at proving a sinking of the coast at Ostend, but the validity of the evidence is doubted by the reporters.—An interesting report on Daltonism in relation to railway-working is presented by M. Delbœuf.—M. Fraipont has a fourth and concluding article on the Acetinians of the Ostend coast, and Dr. Woodward records the discovery of a species of Brachyour crustacean in the coal formation near Mons (to which his attention was called by M. de Koninck).—The theory of the telephone is the subject of a note by MM. Navez.

Journal de Physique, May, 1878.—A new spectroscope here described by M. Thollon offers several advantages; it is direct vision and of perfect symmetry, and can be easily adapted to astronomical telescopes; the prisms (movable) are worked by a rigorously geometrical process, so that a ray coming along the axis of the collimator reaches the axis of the telescope only after twice traversing the whole system of prisms with the minimum of deviation; a considerable dispersive power may be had and may be widely modified in the same instrument; lastly, it affords very exact spectrometric measurements.—Some experiments in which the electro-magnetic rotation of liquids is illustrated with acidulated water containing a little lycopodium powder, the effect being projected by means of Duboscq's new apparatus, are described by M. Bertin. M. Gernez has a note on the production of different hydrates in concentrated supersaturated solutions under the influence of a mechanical action (rubbing the walls of the vessel with a rigid rod).—The metallic reflection of polarised obscure calorific rays is studied by M. Mouton.

Reale Istituto Lombardo di Scienze è Lettere, Rendiconti, vol. xi. fasc. vii.—We note the following papers in this number:—Jealous insanity, by M. Verga.—Expression of pain according to sex, age, individual constitution, and race, by M. Mantegazza.—Contributions to the study of the Italian chiroptera, by M. Regalia.—On the cranium of Volta, by M. Lombrosi.—Examination of the observations made by the committee appointed to adjudicate a prize on the theme, "Programme of a Hospital for Contagious Diseases, suited to the City of Milan," by M. Zucchi.—Study on the prevalent diseases of the vine, by MM. Garovaglio and Cattaneo.

Vol. xi., fasc. viii., ix.—In these numbers we note the following:—On dominant diseases of vines, by MM. Garovaglio and Cattaneo.—Studies on the albumen of milk and on the origin of buttermilk curd, by MM. Musso and Menozzi.—On the causes and circumstances affecting hereditary transmission in animals, by M. Lemoigne.—Observations on elephantiasis in the Arabs in the environs of the Ticinese district, by M. Sangalli.—The third molar in the human race, by M. Mantegazza.—On the distribution and termination of nerves in the tendons of man and other vertebrata, by M. Golgi.

Zeitschrift für wissenschaftliche Zoologie, vol. xxx., supplement, part 1.—On the form of the crystalline cones in arthropod eyes, especially phronima, by Oscar Schmidt.—On anomia, with remarks on the muscular system of lamellibranchs, by H. von Jhering.—The poison apparatus of ants, by A. Forel, 41 pp. two plates.—The post-embryonic formation of limbs in insects, by H. Dewitz, dealing especially with formica, 28 pp.—Contribution to the structure and development of the lungs in mammals, by Ludwig Stieda; figures from embryonic sheep, mouse, and horse.—On the ornamental colouring of Daphnidæ, by August Weismann. The author believes the colour patterns are secondary sexual characters developed by sexual selection.—On the action of the voluntary muscles in land snails, by H. Simroth.

Vol. xxxi., part 3.—Researches on the structure and development of sponges, part 4, by F. E. Schulze; 42 pp., four plates. This part deals especially with the family Aplysiadæ.—Contribution to the development of feathers, by Dr. Th. Studer, Professor at Berne; the feathers of the Penguin, Megapodius, and Dromæus, are dealt with and figured in two plates.—On the fertilisation of the egg in *Petromyzon planeri*, by Ernst Calberla, with a discussion on fertilisation generally; 50 pp., two plates.—On the formation of ova, and on the male of *Bonellia viridis*, by Franz Vejdovsky.

Journal of the Russian Chemical and Physical Societies of St. Petersburg (vol. x. No. 4) contains the following papers:—On the action of peroxide of hydrogen upon the oxygen compounds of thallium, by E. Schöne.—On the action of iodine upon certain urea and amidogen compounds, by W. Roudneff.—On nitrophthalic and oxyphthalic acids, by O. Miller.—On the admixture of zinc in different parts of the body after the intoxication with acetate of zinc, by M. Mazkewicz.—On the action of water and oxide of lead on the halogen compounds of ethylene hydrocarbons, by A. Eltekoff.—On the action of the same substances upon bromide of diamylene, by the same.—On the action of trichlorolactic acid upon urea, by D. Cech.—On the magnetic induction of the two spheres, by O. Chwolson.

SOCIETIES AND ACADEMIES

LONDON

Geological Society, June 19.—John Evans, D.C.L., F.R.S., vice-president, in the chair.—Charles Louis Buxton, Wybrandts G. Olpherts, and William Phelps Richards were elected Fellows of the Society.—The following communications were read:—On the section of Messrs. Meux and Co.'s artesian well in the Tottenham Court Road, with notices of the well at Crossness, and another at Shoreham, Kent; and on the probable range of the lower greensand and palæozoic rocks under

London, by Prof. Prestwich, F.R.S., V.P.G.S. The well-known boring at Kentish Town in 1856 showed the absence at that point of lower greensand, the gault being immediately succeeded by hard red and variegated sandstones and clays, the age of which was at first doubtful, but which were finally considered by the author to approach most nearly to the old red sandstone near Frome, and to the Devonian sandstones and marls near Mons, in Belgium. The existence of some doubt as to this identification rendered the boring lately made at Messrs. Meux's brewery particularly interesting, and the method of working adopted by the Diamond-boring Company, by bringing up sharply cut cores from known depths, gave special certainty to the results obtained. The boring passed through 652½ feet of chalk, 28 feet of upper greensand, and 160 feet of gault, at the base of which was a seam 3 or 4 feet thick, of phosphatic nodules and quartzite pebbles. Beneath this was a sandy calcareous stratum of a light ash-colour, passing into a pale or white limestone, and this into a rock of oolitic aspect. Casts and impressions of shells found in this bed showed it to be the lower greensand, whose place it occupied. The boring was carried further in the hope of reaching the loose water-bearing sands of this formation, but the rock became very argillaceous, and, when 62 feet of it had been passed through, the boring entered into mottled red, purple, and greenish shales, dipping at 35° in an unascertained direction. These beds continued through a depth of 80 feet, when, their nature being clearly ascertained, the boring was stopped. The fossils of these coloured beds, which included *Spirifera disjuncta*, *Rhynchonella cuboides*, and species of *Edmondia*, *Chonetes*, and *Orthis*, show them to be of Devonian age. Thus, the existence of palæozoic rocks at an accessible depth under London and the absence of the Jurassic series, as maintained long since by Mr. Godwin-Austen, is experimentally demonstrated. These facts are of interest in connection with the question of the possible extension of the coal-measures under the cretaceous and tertiary strata of the south-east of England. The beds found at the bottom of Messrs. Meux's boring are of the same character as the Devonian strata which everywhere accompany the coal-measures in Belgium and the north of France, being brought into juxtaposition with them by great faults and flexures. The author refers especially to a remarkable section at Auchy-au-Bois, in the western extremity of the Valenciennes coal-field, which is particularly interesting from its furnishing evidence that the Hardinghen coal-field, between Calais and Boulogne, is a prolongation of that of Valenciennes, and because the same strike and a prolongation of the same great fault observed at Auchy-au-Bois through Hardinghen would carry the southern boundary of any coal-field in the south-east of England just south of Maidstone, thence passing a little north of London. Hence it is in the district north of London that there is most probability of the discovery of the carboniferous strata. The extent of country in which shafts could be sunk to the palæozoic strata will, however, be limited by the presence of the water-bearing lower greensand, which probably reaches close to London in the south, reappears in Buckinghamshire and Bedfordshire, thirty or forty miles north of London, and probably extends some distance towards the city under the chalk hills of those counties and Hertfordshire. The nature of the representative of the lower greensand in the boring, and the characters of the fossils contained in it, lead the author to the conclusion that in it we have a deposit produced near the shore of the neocomian sea, here probably consisting of cliffs of Devonian (or carboniferous) rock. From these cliffs the calcareous material which here replaces the usual loose sands of the lower greensand was perhaps derived by the agency of springs; and the shore-line itself must be situated between the south end of Tottenham Court Road and the Kentish Town boring. The sandy beds of the lower greensand will probably be found to set in at no great distance to the southward, presenting the conditions necessary for storing and transmitting underground waters. A test boring made by Mr. H. Bingham Mildmay at Shoreham Place, about five miles from Sevenoaks, and in which the lower greensand was met with at about the estimated depth (450 feet) and furnished a supply of water, seems to confirm these views.—Notes on the palæontology and some of the physical conditions of the Meux's-well deposits, by Charles Moore, F.G.S. The chief interest of Mr. Moore's investigations centres in the sixty-seven feet of strata intervening between the gault and Devonian. In this marly and oolitic-looking deposit he found no less than eighty-five different kinds of organisms, exhibiting a singular admixture of marine and lacustrine forms of life. Foraminifera are rare, but entomostraca and polyzoa are very abundant. Some genera are found, such as *Carpenteria*, *Saccammina*, *Thecidium*, and *Zellania*, of which the range in time is greatly extended by these investigations. The author fully confirms Mr. Etheridge's reference of the beds in question to the neocomian period, widely as they differ in physical characters from the lower greensand strata of the south-east of England. From a careful study of the nature and condition of preservation of the minute organisms, he concludes that the deposits which contain them were formed at first in shallow lacustrine hollows on the surface of the Devonian rocks now lying buried at a depth of 1,000 feet below London, and that these lakes were invaded by the waters of the neocomian sea, with the deposits of which their sediments were in part mingled, and under which they were finally buried.—The chair was then taken by Prof. Prestwich, M.A., F.R.S., vice-president.—On *Pelanechinus*, a new genus of sea-urchin from the coral rag, by W. Keeping, F.G.S., Professor of Geology in the University College of Wales.—Remarks on *Saurocephalus*, and on the species which have been referred to that genus, by E. Tulley Newton, F.G.S., of H.M. Geological Survey.—A microscopical study of some Huronian clay-slates, by Dr. Arthur Wichmann.—On a section through Glazebrook Moss, Lancashire, by T. Mellard Reade, F.G.S.—On the tertiary deposits on the Solimoes and Javary Rivers in Brazil, by C. B. Brown. With an appendix by R. Etheridge, F.R.S., and communicated by him.—On the physical history of the English lake-district, with notes on the possible subdivision of the Skiddaw slates, by J. Clifton Ward, Assoc. R.S.M., F.G.S.—On some well-defined life-zones in the lower part of the Silurian (Sedgw.) of the Lake-district, by J. E. Marr. Communicated by Prof. T. M'K. Hughes, F.G.S.—On the upper part of the Bala beds and base of Silurian in North Wales, by F. Ruddy. Communicated by Prof. T. M'K. Hughes, F.G.S.

Anthropological Institute, June 11.—Mr. John Evans, D.C.L., F.R.S., president, in the chair.—Dr. J. Beddoe, F.R.S., read a paper on the Bulgarians, referring more especially to the skull-form, on which he quoted Virchow and Kopernicki, but gave also some observations of his own. Not one of sixteen skulls hitherto examined, and procured in different districts of Bulgaria, presented anything like the true Slavonic type, though a few slightly approximated towards it. Almost all were of a cylindrical form, with a considerable parieto-occipital development, and a low, narrow, sloping frontal region; there was an absence of frontal parietal bosses; the skulls inclined to be long, except those few which indicated an admixture of the Slavic type. The majority nowise reminded one of either the Slavic or Turkic form, nor were they much like Esthonian skulls, but they were probably rather Ugrian than anything else. In some of them the great degree of prognathism, the deep nasal notch and horizontal nasal bones reminded Virchow of the Australian type. If the *physique* of the Bulgarians was a difficult and obscure subject, their *morale* presented its own difficulties. They differed from the Serbs in some points favourably; in more, perhaps, unfavourably, though some of their worst faults were doubtless what naturally arose in a subject race. The heroic type which appeared among the Serbs, whether they were Mussulman, Rayah, or free Christian, and culminated in the splendid barbarians of the Montenegro, was absent here. There was no chivalry, but mere ferocity, in their ballads. Their religion was little above Fetishism, and had little connection with morality. Manliness, generosity, truthfulness, and respect for women were scarcely to be expected of such a people; but ambition was there, and industry and acquisitiveness to a degree not found among the Serbs; and the desire of knowledge was there, and the capacity to learn, and, but for the interference of Russia, and the vast amount of moral and physical evil brought about thereby, they might gradually, under a government which, though faulty, was improving, have developed into better things.—Miss A. W. Buckland read a paper on the stimulants of the ancients and of modern savages. The paper commenced by stating that all races have acquired the use of stimulants in some form, but that the stimulants of the lower races, such as the Australian, consists merely of leaves and roots, chewed for their strengthening and invigorating properties, this being only a slight advance upon the instinct which prompts the inferior animals to seek out certain plants for medicinal purposes. The first step towards the manufacture of stimulating drinks is seen in the kava of the South Seas. This art of producing fermentation by the masti-

cating process can be traced in a line across the Pacific from Formosa, where rice is the ingredient thus employed, to Peru and Bolivia, where maize is used for the same purpose, the manufacturers being always women. The next advance is that acquired by agricultural races, who make a kind of beer from the chief cereal grown by them. This liquor probably reached our shores from Egypt, where it was very early known, through the lake dwellers, and still forms the principal drink of all African races. Pastoral tribes, meanwhile, use the milk of their flocks and herds and the honey of wild bees in the manufacture of their fermented drinks: hence the celebrated *koumiss* and *mead* of Scythic nations, the same liquors reappearing among the Kaffirs in South Africa, the vessels used in both countries being the skins of animals, which were also used for storing wines in the East. Later, in Greece and Rome mead was a favourite beverage of the Scandinavians and Anglo-Saxons, and there seems to be a shadow of the Scythic *koumiss* in the Devonshire liquor known as white or grout ale, whilst both liquors may be traced more distinctly in the famous *amrita* and *soma-wine* of the Vedas. Various plants and fruits have been used in all civilised and semi-civilised countries from very ancient times in the manufacture of wines, but grape juices had formerly a circumscribed range, having been confined to Western Asia, Egypt, Greece, and Rome, but forbidden in China and the vines extirpated. The religious ceremonies and prohibitions attached to these various beverages were briefly noticed, as also the deification of plants on account of their medicinal properties and the form and material of drinking vessels, whilst alcohol, the latest and most pernicious development of the art of manufacturing stimulants was only mentioned as not having been included among the beverages of the ancients nor known to savages until introduced by Europeans.—The Director then read a paper by Mr. John Sanderson on polygamous marriage in South Africa.

PARIS

Academy of Sciences, July 16.—M. Fizeau in the chair.—The following among other papers were read:—Remarks on the influence of atmospheric electricity of weak tension on vegetation, by M. Berthelot. *A propos* of M. Grandeau's experiments, M. Berthelot recalls his own, proving fixation of free nitrogen on organic matters under weak electrical action, &c., such action (in nature), being probably more efficacious than that of thunderstorms, owing to its duration and extent.—On a *brochure* of M. Hirn relating to whirlwinds, by M. Faye. M. Hirn distinguishes two kinds of descending whirling movements, represented by cyclones and by trombes. The former (he considers) are propagated naturally downwards by simple lateral communication of a gyratory motion, originating in the upper regions. They enlarge and diminish in rapidity (owing to friction). For the other class, which become more and more restrained in their transverse dimensions (and take the figure of an upright, not an inverted, funnel), he calls in a small force in the form of electricity of the clouds, and the attraction between them and the ground through an imperfectly conducting medium. M. Faye doubts this view, and supposes mechanical identity of the two phenomena.—Processes and apparatus for study of the velocity of propagation of excitations in different kinds of motor nerves in mammalia, by M. Chauveau. In this note he merely describes his mode of experimentation, which was on mammalia of large size, chloralised, or subjected to section of the bulb and artificial respiration. He used induced currents for the (uncovered) nerve, by the unipolar method. At each turn of an automatic distributer the current is passed to a different point of the nerve. The results of experiment are stated to differ from those of Helmholtz in his experiments on the nerves of a killed frog.—General Morin referred to the loss sustained by the Section of Mechanics in the death of General Didion, author of a "Traité de Balistique," &c.—On galvanoplasty of cobalt, by M. Gaiffe. Cobalt may be advantageously substituted for iron and nickel, as a protective layer for engraved and typographic plates. M. Gaiffe uses a bath of neutral solution of the double sulphate of cobalt and ammonia; the anode a sheet of platinum or (better) a plate of cast or forged cobalt. The current is kept at about six B.A. units, and reduced to three, when the whole piece has become white. The deposition may be made nearly as rapid as that of nickel.—On the existence of lesions of the anterior roots in acute ascending paralysis, by M. Vulpian. These lesions, found in every preparation (the spinal cord being unaltered), consisted in fragmentation of the myeline into drops and droplets, hypergenesis of the protoplasm of each inter-annular segment, and multiplication of the nuclei of Schwann's sheath. The cylinder-axis had completely disappeared.—M. Ducretet presented (through M. Du Moncel) a stethoscopic microphone of great sensibility. He utilises M. Marey's delicate tambours, the vibrations of sound from the body acting on the elastic membranes.—Discovery of a comet by Mr. Lewis Swift, at Rochester, U.S. (telegram from the Smithsonian Institution).—Measurement of the calorific intensity of solar radiations, by M. Crova. This relates to observations last year. The intensity at mid-day increased from the end of January to March 15, when there was a maximum of 1·320 cal. The minimum was on June 28 (1·023 cal.); then the radiation increased, and on October 16 reached the pretty high value of 1·260 cal. As before, the weakest radiations were with S. or S.W. winds and comparatively high temperatures; the strongest with N. or N.W. winds and low temperatures; the former winds increasing, the latter diminishing the vapour in the atmosphere. The author adds some observations on this last point.—On the reform of some processes of analysis in laboratories of agricultural stations, and observatories of chemical meteorology; volumetric determination of sulphate contained in water, by M. Houzeau. A new method for the latter is described.—The septicity of putrefied blood is destroyed by a very long contact with compressed oxygen at high tension, by M. Feltz.—Identity of nature of spontaneous and traumatic erysipelas; consequences, by M. Real. A discussion took place on explosions in flour-mills, *à propos* of a recent letter of Mr. Lawrence Smith from the United States.—Structure of the stem of Sigillaria, by M. Renault.

GÖTTINGEN

Royal Society of Sciences, March 2.—The following, among other papers, were read:—Comparative anatomy of the crystalline lens, by Prof. Henle.—The bursæ of Ophiura and their homologies with the Pentremites, by Prof. Ludwig.

May 4.—The systematic position of Sclerophylax and Cortesia, by Prof. Grisebach.—Observations on the pharmacology of Salicin, by Dr. Marmé.—Relation of the left intercostal vein to the vena azygos, by Prof. Brunn.—Some Avestic words and forms, by Prof. Bezzenberger.—Coptic-Arabian inscriptions in the University library, by Prof. Wüstenfeld.

CONTENTS

THURSDAY, AUGUST 1, 1878

THE ECLIPSE OF THE SUN

THE following telegram was received from Mr. Lockyer at the moment of going to press. It contains the fullest account of the results of the Eclipse, and of the conclusions arrived at by some of the most eminent of the observers engaged on it, which has yet been published:—

Corona smaller and less brilliant than in 1869 and 1871. Hydrogen faint in corona. Generally invisible, as was also the case in 1874. Ranyard's polarisation confirmed Young's law, both (?) and lines brilliant. Corona probably photographed in Siam (?). Fluorescent eyepiece worked well. Bright line near B; heat line in ultra red by thermopile. Watson's Vulcan, Right ascension 8° 26′, declination 18° N. Result—lower temperature of corona gas, as confined to chromosphere; almost entirely continuous spectrum, isolated from gaseous spectrum. Corona changes with sun-spots, and prominences increased. Continuation of absorption with deficit in ejected hot matter induces solar radiation. Young, Watson, Draper, Lockyer cable above.

The following telegram appears in the *Daily News* of yesterday from its special correspondent at Rawlings, Wyoming, U.S., under date July 29:—

The eclipse has been most satisfactorily observed at all the northern stations, and at all the southern ones from which news has been received up to the present time.

The corona was markedly different from those observed in 1869, 1870, and 1871, and this year the observations have demonstrated the great variation in the structure and condition of the sun's outer atmosphere when there are most and fewest spots on his disc. The corona was small, of a pearly lustre, and the indications of definite structure were limited to two portions. Several long rays were seen, and Prof. Newcomb, who had erected a screen on a high pole, thinks he detected the zodiacal light extending six degrees from the sun. Prof. Draper, who used a Rutherfurd grating two inches square and a camera of large aperture, and Mr. Lockyer, who placed a small grating in front of an ordinary portrait camera, both obtained photographs of the spectrum of the corona. A continuous spectrum only was recorded, and in ordinary spectroscopes the bright lines usually seen were altogether absent. Mr. Lockyer, who observed with a simple grating, saw no rings.

All these are so many indications of a wonderful change since 1871, and there is great probability that the substance which gives rise to the continuous spectrum is not that which produces any of the lines.

Prof. Newcomb's party and Prof. Barker made a careful search for the dark lines in the corona, but none were observed. Prof. Young has telegraphed that there were no lines observed in the ultra violet at Denver. It would appear, therefore, that he also has obtained photographic evidence of a continuous spectrum. The radial polarisation observed in 1871 has been confirmed by Prof. Holden.

A new use of the eclipse has been introduced on this occasion. Professors Newcomb, Watson, Holden, and others have included a search for intramercurial planets in their programme, and Prof. Watson has been fortunate enough to detect a body of four and a half magnitude near the sun, which certainly is neither a known star nor a planet.

Every facility has been afforded to the astronomers, and a fourth station along the northern line crossing the belt of totality was at the last moment organised by the Union Pacific travelling photographic car being run to a point between the Eclipse camps at Separation and Preston.

The tasimeter, the new instrument on which Mr. Edison has been working unceasingly here, has proved its delicacy. During the eclipse he attached Thomson's galvanometer, the index being set to zero, when the telescope carrying the tasimeter was pointed several degrees from the sun. The point of light rapidly left the scale, when the corona was brought upon the fine slit by which the tasimeter itself was protected. There was no chromosphere to speak of, and only one prominence, like the horn observed in 1868, but very dim.

OUR NATURAL HISTORY COLLECTIONS

ON referring to the "Bill to enable the Trustees of the British Museum to remove Portions of their Collection," our readers will see that our correspondent, "Naturalist," in our last issue (p. 328), is correct in supposing that this measure contemplates no change whatever in the administration of the natural history collections when placed on their new site in South Kensington. The fourth clause of the Bill expressly reserves all the "rights, powers, duties, and obligations" of the fifty trustees, except as regards the mere removal of the portions of the collections specified in the schedule. And we must agree with our correspondent that this measure will be directly contrary to the opinion of many of our leading naturalists, and to the recommendations of the Royal Science Commission.

As regards the first point, it is only necessary to refer to the memorial presented to the Chancellor of the Exchequer in 1866, when the subject of the removal of the natural history collection was under discussion. The memorial, which was drawn up primarily in *support* of the removal of the natural history collections from the rest of the British Museum, and is signed by Mr. Bentham, Mr. Darwin, Sir J. Hooker, Prof. Huxley, and other well-known names, states that in the opinion of the memorialists "it is of *fundamental importance* to the progress of the natural sciences in this country that the administration of the natural history collections should be placed under one officer, who should be immediately responsible to one of the Queen's ministers." The Bill as drawn proposes to leave the natural history collections exactly as they are, under the rule of fifty trustees responsible to no one but to themselves.

Let us now turn to the Fourth Report of the "Royal Commission on Scientific Instruction and the Advancement of Science," issued in 1874, in which the affairs of the British Museum, and more especially of the natural history collections belonging to it, are discussed. After an exhaustive account of the circumstances of the case and an analysis of the evidence given before them by the

leading naturalists of the day on the subject, the Royal Commissioners came to the following conclusions :—

1. That the occasion of the removal of these collections to the new buildings now being erected at South Kensington for their reception be taken advantage of to effect a change in the *governing authority and official administration* of that division of the museum.

2. That the director of the natural history collections should be appointed by the Crown, and should have the entire administration of the establishment under the control of a Minister of State, to whom he should be immediately responsible.

Now it is hardly necessary to point out that if the Bill before the House of Commons be passed in its present state (whereby all "the rights, powers, duties, and obligations" of the Trustees of the British Museum are expressly reserved), the recommendations of the Royal Commissioners will be treated as so much waste paper. If the Government appoints a Commission of the best men of the country to advise them on a subject of which they know nothing, it seems to us to be hardly civil to allow an Act of Parliament to be passed in the teeth of their deliberate recommendations without even taking the trouble to explain why these recommendations are not to be carried into effect. Yet this is what is now proposed to be done.

HULL'S GEOLOGY OF IRELAND

The Physical Geology and Geography of Ireland. By Edward Hull, M.A., F.R.S. (London: Stanford, 1878.)

THE great map of the veteran Sir Richard Griffith, followed by the detailed labours of other geologists, especially of the Geological Survey, and of its lamented director, the late J. B. Jukes, has explained the general geological structure of Ireland, and sketched, partly in outline, partly in considerable detail, the curious problems which that structure suggests. As yet, however, the abundant published information to be gleaned from papers and memoirs regarding Irish geology lies chiefly scattered through the *Transactions* of various scientific societies, and the Explanations of the Survey. Some of these publications are not nearly so widely known as they deserve to be, or as they assuredly would be if it were more easy for geological students in general to procure a reading of them. Mr. Hull has, therefore, done good service in preparing this little handbook to the geology and geography of Ireland. It is a most useful compendium of information, and its utility is greatly enhanced by the references to those works and papers where the subjects he discusses are more fully treated.

The volume is divided into three parts. In the first of these the author gives a digest of what is known regarding the geological formations of Ireland. In treating of the palæozoic rocks, he follows Harkness and others in regarding the metamorphic rocks of the north-western counties as the general equivalents of the unaltered Lower Silurian masses of the rest of the island, thus identifying both groups of rocks with those which occupy a similar position in Scotland. In this he is undoubtedly correct, and is quite justified by the sections published by Murchison and others. In these days, however, when almost every dictum of our fathers is called in question, and when able observers on both sides of the Atlantic are loudly proclaiming that they can find no true palæozoic gneiss and schist anywhere; when Alpine rocks—once devoutly regarded as metamorphosed Cretaceous strata—have been pushed back and back till their enemies will not let them have a footing among any even of the palæozoic formations, it certainly would be a good and serviceable piece of work to fix, if possible, by means of fossils, the horizon of the quartzites and limestones of Donegal, and to demonstrate, by numerous transverse sections, that these rocks pass truly, and with no deceptive overturn, beneath the younger gneissose and schistose masses. In Prof. Hull's necessarily brief summary he does scant justice to the Old Red Sandstone. To some extent he makes up for this by the greater fulness of his account of the Carboniferous system, to which he gives considerable interest by the parallelism, suggested by his long experience in Lancashire and elsewhere, between the established divisions of the system in England and the grouping which he has been able to recognise in Ireland. The fragments of Permian and Mesozoic deposits in the north of Ireland are duly mentioned; a more detailed description is given of the huge volcanic plateau of Antrim, and the successive stages of its history; while the Glacial and Post glacial formations receive tolerably ample illustration.

Having laid his foundation of facts, Mr. Hull proceeds, in Part II., to build upon it his explanation of the present physical geography of Ireland. Beginning with the mountains he arranges them in groups, and points out in each case the evidence of their age. The remark just made regarding the metamorphic rocks of Donegal may be repeated here in reference to the alleged age of these north-western mountains. Of course as Upper Silurian rocks lie against them and contain conglomerates derived from them, these heights must be far older than Upper Silurian times. The author assigns them to a long unrepresented interval between the Upper and Lower Silurian periods—a date to which the corresponding Scottish Highlands have also been referred. In dealing with the Wicklow Highlands so admirably worked out by Jukes and his colleagues, Mr. Hull suggests that as the granite there was certainly protruded before the Old Red Sandstone had been laid down, it may even have been earlier than Upper Silurian time, and "therefore synchronous with the mountains of Donegal, Mayo, and Galway." But the Old Red Sandstone of the South of Ireland, thick though it be, seems to represent only the upper member of that system. The vast period of the Lower Old Red Sandstone, so rife elsewhere in subterranean movements and volcanic outbursts, is not known in the south of the island, unless we may conjecture the Wicklow granite to belong to that epoch. The numerous and characteristic ridges and isolated eminences which in the south-western counties and in the central plain rise out of the Carboniferous plain, often with a central core of contorted Silurian rocks, are assigned to an interval of terrestrial disturbance between the Carboniferous and Permian periods. The evidence for this conclusion is fragmentary and has been skilfully marshalled into form by the author; but it cannot be regarded as by any means conclusive. Yet more uncertain is the reference of the Mourne Mountains to the Permian period. That

these heights are remnants of an ancient volcanic centre of later date than the Carboniferous Limestone has been made satisfactorily evident by the careful maps and sections of the Geological Survey. The rocks differ a good deal from those of the Tertiary volcanic region of Antrim. Mr. Hull thinks that they have not the same "appearance of recentness" as the latter, and as Permian volcanic rocks have been recognised in the south-west of Scotland, he thinks it a pity that the Emerald Isle should not have a share of them, and so he would fain regard the peaks of Mourne and Carlingford as the stumps of volcanoes which were blazing in the west when those of the Rothliegende were active in central Germany. All that, in the present state of our knowledge, can be affirmed about these rocks, is that they are later than the Carboniferous Limestone. They may be called Tertiary with about as much probability as Permian.

The wrongs of Ireland go back at least as far as the close of the Carboniferous period. Mr. Hull, with praiseworthy calmness, sketches the process by which his country has been despoiled of its once extensive coal-fields, and, while pointing regretfully to the few little scraps left here and there to tell of former mineral wealth, doomed to irretrievable destruction before either Celt or Saxon set foot upon the land, he consoles us with the just reflection that "the character of the inhabitants and their destiny as an agricultural or pastoral people were fixed altogether independently of social or political considerations." The author, following Jukes in his explanation of the history of Irish rivers, gives some interesting details regarding a few of the principal water-courses of the country. His account of the numerous lakes of Ireland is well arranged, but provokingly brief.

In the third part Prof. Hull deals with the glaciation of Ireland, and presents us with a readable summary of what is known up to this time on that subject, his narrative being accompanied by a small coloured map, on which the chief lines of ice-movement are drawn. Though certain tracts are marked on this map as "snow-fields," it is to be presumed that at the time the rocks were being striated in the directions there indicated, the whole island was one vast snow-field, with no boundary of any kind between the tracts here separated and the rest of the country. In a closing chapter the author brings before his readers the days of the mammoth, red-deer, rein-deer, great Irish deer, wolf, bear, and wild boar. To that venerable Irishman, the *Megaceros hibernicus*, a couple of pages are lovingly devoted, where we learn that the reason why he flourished so abundantly in the sister island was "the absence of many of the natural enemies with which he had to contend in Britain and Europe." Happy days these must have been! Who knows but Megaceros may have lived in brotherhood with the earliest human Irishmen until in after ages the "natural enemies" of both crossed over to them from Britain.

The volume is certain to prove useful. To geologists at a distance it presents in brief and readable form a compendium of all that is most striking and interesting in Irish geology. To those who can avail themselves of the numerous opportunities now afforded of visiting and travelling in Ireland it forms an admirable guide-book. Its appearance before the approaching meeting of the British Association is opportune. No member of the Association who means to see a little of Ireland after the Dublin congress is over should neglect to stow a copy of the book into a corner of his portmanteau.

ARCH. GEIKIE

OUR BOOK SHELF

A Treatise on the Cycloid and all Forms of Cycloidal Curves, and on the Use of such Curves in dealing with the Motions of Planets, Comets, &c., and of Matter projected from the Sun. By Richard A. Proctor. With 161 Illustrations and many Examples. (London: Longmans, 1878.)

THIS is a very full book on the curves enumerated; marked by much elegance in the geometrical portion of the work. It is by far the completest treatise we know, and is likely to take its place as a standard work on the subject. It is marvellous how much can be said about these curves, and one is ready to indorse Chasles' opinion—referring to the cycloid—"Cette courbe merveilleuse." Mr. Proctor only slightly glances at the historical side, and merely refers to Pascal's famous questions, a proof of which, we believe, could hardly, if at all, be effected by purely geometrical methods. Use has been made of De Morgan's article on trochoidal curves, the fullest previous exposition of the properties of these curves in relation to epicyclics, and the work, which is admirably printed, has had the advantage of being embellished with drawings from Mr. Perigal's well-known mechanically-traced curves (bicircloids). One section is devoted to the analytical equations to the curves, and the last section is a reprint of two papers which have already appeared in the *Monthly Notices* of the Astronomical Society, entitled "The Graphical Use of Cycloidal Curves to determine (1) the Motion of Planets and Comets, (2) the Motion of Matter projected from the Sun."

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

The Microphone

THE pleasure with which those beautiful discoveries and inventions, the telephone, the phonograph, and the microphone, have been appreciated by the world, has been unhappily, and I must say I think unnecessarily, marred by one of the most disagreeable things that can be thrust on the public—a personal claim of priority, accompanied by accusations of bad faith, especially when made against any one of whose name and fame the public has come to feel concerned.

Before troubling the public at all with such a matter, Mr. Edison might surely have reasoned out his claim with Mr. Preece, with whom he had been from the beginning in correspondence, or he might have written immediately to public journals, calmly pointing out the close relation between his own "carbon telephone" and Mr. Hughes' subsequent "microphone." The scientific public could then have calmly judged, and would have felt much interest in judging, how much in common or how much not in common there may be in the physical principles concerned in the two instruments. But by his violent attack in public journals on Mr. Preece and Mr. Hughes, charging them with "piracy" and "plagiarism," and "abuse of confidence," he has rendered it for the time impossible for either them or others to give any consideration whatever to his claims. Nothing can be more unfounded than the accusations! Mr. Preece himself gave, at the Plymouth meeting of the British Association last August, a

clear and thoroughly appreciative description of Edison's carbon telephone, and published it in the printed reports of his lecture which appeared in the public journals. The beautiful results shown since the beginning of the present year by Mr. Hughes with his microphone were described by himself in such a manner as to leave no doubt but that he had worked them out quite independently, and that he had not the slightest intention of appropriating any credit due to Mr. Edison. It does seem to me that the physical principle used by Edison in his carbon telephone and by Hughes in the microphone is one and the same, and that it is the same as that used by M. Clérac, of the French "Administration des Lignes Télégraphiques," in the "variable resistance carbon tubes," which he had given to Mr. Hughes and others for important practical applications as early as 1866, and that it depends entirely on the fact long ago pointed out by Du Moncel, that increase of pressure between two conductors in contact produces diminution of electric resistance between them.

I cannot but think that Mr. Edison will see that he has let himself be hurried into an injustice, and that he will therefore not rest until he retracts his accusations of bad faith publicly and amply as he made them. WILLIAM THOMSON

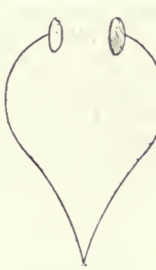
Yacht *Lalla Rookh*, Cowes, July 30

IT may be of interest at the present time to recall the fact that the word "microphone" was first employed by Sir Chas. Wheatstone upwards of fifty years ago. In a paper entitled "Experiments on Audition," published in the *Quarterly Journal of Science* for 1827, Wheatstone remarks:—"The great intensity with which sound is transmitted by solid rods at the same time that its diffusion is prevented affords a ready means of effecting this purpose [augmenting the loudness of external sounds], and of constructing an instrument which from its rendering audible the weakest sounds may with propriety be named a microphone." As the original paper may not be readily accessible, an extract from it is appended to this letter, wherein will be found a description of the simple arrangement proposed by Wheatstone—it is in fact a metallic binaural stethoscope—together with some experiments with the instrument given by the author. The entire paper will appear in the republication of Wheatstone's scientific papers, which the Physical Society will shortly issue, and the instrument itself can be obtained for a trifling sum from Mr. Yeates, of King Street, Covent Garden.

Monkstown, Dublin, July 29 W. F. BARRETT

"Procure two flat pieces of plated metal, each sufficiently large to cover the external ear, to the form also of which they may be adapted; on the outside of each plate, directly opposite the meatus, rivet a rod of iron or brass wire about 16 inches in length, and one-eighth of an inch in diameter, and fasten the two rods together at their unfixed extremities, so as to meet in a single point.

The rods must be so curved, that when the plates are applied to the ears, each rod may at one end be perpendicularly inserted into its corresponding plate, and at the other end may meet before the head in the plane of the medial line.



Wheatstone's microphone, 1827.

The spring of the rods will be sufficient to fix the plates to the ears; but for greater security ribands may be attached to each rod near its insertion in the plate, and be tied behind the head. A more simple instrument may be constructed to be applied to one ear only, by inserting a straight rod perpendicularly into a similar plate to those described above.

The microphone is calculated only for hearing sounds when it is in immediate contact with sonorous bodies; when they are diffused by their transmission through the air this instrument will not afford the slightest assistance. It is not my intention in this place to detail all the various experiments which may be made with this instrument; a few will suffice to enable the experimenter to vary them at his pleasure:—

1. If a bell be rung in a vessel of water and the point of the microphone be placed in the water at different distances from the bell, the difference of intensity will be very sensible.

2. If the point of the microphone be applied to the sides of a vessel containing a boiling liquid, or if it be placed in the liquid itself, the various sounds which are rendered may be heard very distinctly.

3. The instrument affords a means of ascertaining, with considerable accuracy, the points of a sonorous body at which the intensity of vibration is the greatest or least; thus, placing its point on different parts of the sounding-board of a violin or guitar whilst one of its strings is in vibration, the points of greatest and least vibration are easily distinguished.

4. If the stem of a sounding tuning-fork be brought in contact with any part of the microphone, and at the same time a musical sound be produced by the voice, the most uninitiated ear will be able to perceive the consonance or dissonance of the two sounds; the roughness of discords and the beatings of imperfect consonances are thereby rendered so extremely disagreeable, and form so evident a contrast to the agreeable harmony and smoothness of two perfectly consonant sounds, that it is impossible that they can be confounded."—*Quarterly Journal of Science*, 1827, Part II.

The Meteor Shower of Aquarids (July)

ON July 27 ninety-three shooting stars were seen here, between 10h. 30m. and 14h. 30m., which, after making allowance for time occupied in charting the paths, is equivalent to about twenty-nine per hour for one observer. There was a rich shower of *Aquarids* from a point near μ Aquarii, at R.A. 343°, 14° S. declination, which gave twenty-two meteors. These were rather bright, not very swift, with moderately long paths (averaging 17°), and quite devoid of streaks. I had seen about five meteors of this system on the preceding night, and on the 28th I watched a very hazy condition of sky—in which the stars shone dimly—for four hours, and of forty-four meteors seen five or six others were *Aquarids*. This active shower was well seen by Capt. Tupman on July 27, 1870, with an accurately-defined radiant at 340°—14°, from fourteen meteors (see No. 43 of his catalogue), and on the four following nights he traced about forty-seven others from the same shower, though the centre seemed a little further south, at 340°—19° on the 28th. On the 27th he recorded seven meteors of this stream within the twenty-one minutes, from 14h. 3m. to 14h. 24m. It was also seen by me, at 342°—12° (ten meteors), on August 3-17 last year; and Schmidt gives radiants at 337°—11° for July 20-31, and at 344°—11° for August. Neumayer, in the southern hemisphere, also has a position at 337°—10° (July), and Heis at 339°—10° for July 27-31. The average centre from these eight independent determinations is at 340°—13° for this important shower, which evidently comes to a strong maximum on about July 27-29. The end of July has long been known as a meteor-epoch of considerable intensity, with a maximum, according to Quetelet, between the 27th and 29th, which is thus amply confirmed by recent observations, and proves these *Aquarids* to be but little less in importance to the annually-recurring showers of *Lyrids*, *Orionids*, *Taurids*, and *Geminids*.

It should be pointed out that, in future observations of this special shower, care must be taken not to confuse it with other contemporary showers in Aquarius. There are two radiants on or near the equator in about R.A. 334° and 349°, and one (of very slow meteors) at about 336°—6°, also a fourth at 326°—12° (mean position of five radiants seen by Schmidt and Tupman). They are distinct showers, though separated with difficulty, owing to proximity of position; and it is interesting to note that, if we average them with the strong radiant at 340°—13° referred to above, we have a central radiant at 337°—6°, which coincides exactly with Mr. Greg's position for the *Aquarids* (Nos. 109 and 137 in his catalogue of 1876), which apparently continue from July 5 to October 31. W. F. DENNING

Ashleydown, Bristol, July 29

P.S.—My observations were continued on the night of July 30, when seventy-six shooting stars were seen in four hours; thirteen of these were *Aquarids*, and seven of them visible in the half an hour preceding midnight, after which few were observed. From this I infer that the maximum had probably occurred on the morning of the 30th, when, unfortunately, a thick haze prevented work.—W. F. D.

Physical Science for Artists

WILL you permit me, through your columns, to tell Mr. Abbay that the phenomena—"les rayons de crépuscule"—he

refers to in his letter in NATURE, vol. xviii. p. 329, are of not unfrequent occurrence on the west coast of India, in the plains as well as the highlands. Moreover I can recollect being much struck with the appearance whilst travelling in Ireland in the autumn of 1863. The convergence of rays in the east, while the sun was setting, was then new and singular to me, but I have since often observed the phenomenon.

On some occasions the atmosphere has seemed clear, until sunset, when the blue sky has literally reddened, and it is then that the cloud shadows show best as bands of blue stretching from west to east, and visible in the zenith as well as nearer their converging points. E. H. PRINGLE

Clevedon, July 27

Taunton College School

IN your last number, in an article by Mr. Shenstone, of Exeter School, on the formation of a laboratory, he incidentally speaks of the council of this school, of which I am headmaster, as having "quenched the torch of science in the west." As this statement may be prejudicial to me, and can only arise from Mr. Shenstone's being imperfectly acquainted with the facts, I trust to your spirit of fairness to allow me to state publicly (1) that the senior half of the school in the last six months has been through a course of electricity and magnetism illustrated by experiments, and has just passed a creditable examination in those subjects under a Cambridge examiner of high reputation; (2) that there has been a course of lectures on botany this term; (3) that all those boys whose interests require it have been taught chemistry; (4) that we have had a very favourable report of the boys' proficiency in mathematics, which I presume, has some claim to be considered science, though to my great surprise at the present day it is often spoken of as if it had none. The council have left me perfectly free to teach as much or as little science as I choose; I choose, and in all probability always shall choose, to give every opportunity of acquiring scientific knowledge to my pupils, consistent with their instruction in other necessary subjects, and if I find any with strong scientific tastes, to foster them to the utmost. As my academical position was mainly, though not entirely, due to my scientific knowledge, it would be strange if I acted otherwise.

HENRY PEARCE KNAPTON

Taunton College School, July 27

Deep-sea Dredgings off the Gulf of Mexico

I WISH to correct an omission of mine in a notice of the work of the United States schooner *Blake* (NATURE, vol. xviii. p. 198). Capt. Sigsbee's modification of Thomson's wire-sounding machine was used for the deep-sea soundings. The steel wire was No. 20 American gauge, and the time required to reel in with it was always one minute per 100 fathoms. The steel wire rope used for dredging, which was made expressly at the suggestion of Prof. A. Agassiz, was the one mentioned in the notice as being of galvanised steel, with a hemp core (not coil, as printed), and which in the notice appears as if it were the same wire rope that was employed for sounding. The sentence will read correctly if the words "used for dredging" be inserted after the words "the wire rope" in the paragraph. The importance of this suggestion of Prof. Agassiz will be best understood when the immense saving of space gained—one coil of 3,000 fathoms of this wire rope occupying on the reel only a space of 5 feet long, 5 feet high, and 4 wide—and the immense saving in time of lowering and hoisting the dredge, are taken into account.

E. P. W.

ANTHROPOLOGY IN FRANCE [1]

IN the numbers of the *Bulletins* of the French Anthropological Society for 1876 are many papers of importance, some of which we shall briefly bring before our readers.

An admirable series of photographs of natives of New Caledonia, taken by order of the local French authorities, has been made the subject of some interesting notices by M. Paul Topinard. Comparing these pictures and a number of the skulls of indigenous New Caledonians with those of native Australians, he finds that while the former exhibit a greater unity of type than the latter, they also differ from them in presenting a roundness in the contour of the face, due to a predominance of the cellular tissue, which contrasts strongly with the well-marked muscles of the Australian. Their affinity with the negro and New Guinea Papuan types is incontestable. M. P. Topinard discusses in another paper the relative merits of the craniometric and facial modes of measurement, adopted the one by Daubenton and Camper, the other by Blumenbach, and Prichard, and the results obtained by M. de Quatrefages with the instrument invented by him, and named *goniomètre pariétal.* The speaker considers Prichard in error when he attempts to include all Mongolian groups generally under his so-called "ogival" cranial form, this form belonging, in fact only to the Esquimaux.—M. Lagneau wishes to draw the attention of anthropologists to the peculiarities still observable in the people of some parts of La Gironde, known as "Gavaches," or "Marotins," who are descended from certain Angevins, called into the district in about 1525, to repair the ravages caused by a virulent epidemic. In their indolence and slowness they differ strikingly from the vivacious Gascons, among whom they live.—The discovery in the lacustrine houses of Switzerland and Savoy, and in the Lake of Bourget of bronze rods, surmounted with movable rings, has called forth explanations from all quarters. Carl Vogt, among others, has come forward in response to M. Mortillet's invitation to supply him with a clue to their use, and according to him we still have a similar instrument in the "Ringelstock" of the German herdsman, which is formed of a stout nut-stick, terminating in a lateral branch, on which are hung several metal rings. If the noise is not successful in bringing back the animal, the instrument is thrown at its head with an alarming clatter of bells. M. Hamy entertained the members of the society with an account of the eccentricities of Siamese and Chinese fashion in the development of the finger-nails, which, according to some of the fac-similes laid before the meeting, at times attain a length of forty, and even forty-five centimetres, although usually only that of ten or twelve centimetres; in most instances this process of lengthening carries with it a corresponding twisting and interlacing of the nails, which acquire the semblance of antlered horns.—M. Bertillon has for some time been engaged in a careful analysis of the preponderance of one sex over the other in first or second legitimate, and illegitimate births in France, and some other countries, more especially Austria. He finds that in France to every 100 females born alive, there are 105 males; while in regard to still-born births the excess is as 137 to 100. These relations are, however, found to differ essentially when illegitimate births are considered alone, in which case the proportion is as 1,031 males to 1,000 females. In Austria, where the official reports kept of the sex of first and second births admit of more precise calculations in regard to this point, it is found that first births are more frequently masculine, in the proportion of 110·3 to 100; second births being as 105·2 males to 100 females, while the general relation of the sexes for all births is as 106 males to 100 females. This, however, applies only to legitimate births; the proportion falling in the illegitimate to 105 males against 100 females. MM. Lagneau, Broca, and others, took part in the discussion which followed on the reading of M. Bertillon's paper on the Influences of Primogeniture on Sex. In relation to this subject we may refer to M. Sanson's report (laid before the society on May 4, 1876) of the influence of age, vigour, &c., on the offspring, as noted by himself, on sheep and other domesticated animals.—French local archæology and palæontology have received careful attention from the society during the past year. M. Pommerol has described the curious megalithic monuments which are to be found in close vicinity to the mineral springs, ancient mortuaries, and church at Saint-Nectaire, in Auvergne. A dolmen, hitherto known only to the local peasantry

[1] *Bulletins de la Société d'Anthropologie de Paris.* Tome onzième, 1876.

has been cleared from the superincumbent foliage and opened, revealing remains of skeletons which appear to belong to the polished stone-age. M. Lepic has discovered a series of bone-caves near Soyons on the Rhone, and known as La Caverne de Néron. Here human remains are found intermingled with rhinoceros, elephant, horse, and reindeer bones, an immense number of flints, hatchets, arrow-heads, &c. M. Lepic, in extending his explorations to the plain of Soyons, above these caverns, discovered unmistakable remains of human habitations, together with the ordinary kind of bone, silex, and other *débris*. In the latter, however, was found a large hatchet formed of hematite, the first of the kind met with; fragments only of the stone having been hitherto obtained. In the grotto of Savigny stag-horn tools have been found precisely similar to those in use among the Lapps and Esquimaux for smoothing down the rough seams of their skin garments. At the aqueduct at Nîmes a Celtic inscription in Greek characters has been brought to light, this being the third of the kind found in Southern Gaul. The Abbé Maillard has made a careful survey, and drawn up a comprehensive plan of the so-called prehistoric stations at Thorigné-en-Charnie. M. de Prunières has laid before the society the results of his examination of the dolmens of l'Aumède Lozère, in which he had found an enormous mass of human bones belonging, for the most part, to a dolichocephalic race. The great number of the cranial bones, which presented perforations and cicatrised margins, confirms the view that trepanning was resorted to by primitive men for various pathological conditions.—M. Fischer's paper on cave-conchology and his reference to the identification of Isle of Wight fossils at Langerie-Basse, led to an interesting discussion on the question whether navigation could date as far back as the age of bone caverns, or whether different geographical relations alone could explain the presence of shells far from their centres of origin.—M. Boyer has made the skulls found in the Puy-de-Dôme caves the subject of careful study, and shows that a greater variety of type is to be met with than is usually admitted, while M. A. Roujon has turned his attention to the general anthropology of the district, which has led him to the assumption that five distinct types have succeeded one another in France.—M. Mortillet has presented the society with a copy of a map of prehistoric France, drawn up by him for M. Elisée Réclus' "Géographie Universelle," in which he has noted down all stations, caves, and dolmens discovered up to the present time.—M. Piette has drawn up a report of the remains in France of a Gallic civilisation, as exemplified in the collections brought together in the exhibition held at Rheims in 1876. One of the most important of the papers included in the *Bulletins* under consideration is M. Lagneau's exhaustive review of the ethnic distinctions between Celts and Gauls, a question which necessitates a careful reference to classical authorities, and seems still far removed from any satisfactory determination.—Another question similarly open to discussion, although widely different in character, is considered by M. Topinard, who has made the publication of his manual of anthropology the occasion for discussing the differences of meaning, to be practically attached to the terms ethnology, anthropology, and ethnography. A summary of such a paper would be of little use, and from the minutely-detailed series of definitions which the writer has thought it necessary to give, it would appear that Frenchmen have been less ready than ourselves to accept the more special meaning of anthropology as applied to man zoologically, and distinct from man when considered in reference to characteristics of race.

OUR ASTRONOMICAL COLUMN

THE TOTAL SOLAR ECLIPSE OF MAY 28, 1900.—In a recent note upon solar eclipses that will be total upon the North American continent, reference was omitted to one in the last year of the present century. On May 28, 1900, the moon's shadow will traverse the southern part of the United States territory, entering it near New Orleans and passing off in Chesapeake Bay. The elements of this eclipse are as follow:—

G.M.T. of Conjunction in R.A., May 28, 1900, at 2h. 56m. 22s.

	°	′	″	
R.A.	64	56	48·5	
Moon's hourly motion in R.A.		37	16·9	
Sun's „ „		2	32·4	
Moon's declination	21	50	17·1	N.
Sun's „	21	27	15·3	N.
Moon's hourly motion in declination ...		2	38·5	N.
Sun's „ „ „ ...		0	24·2	N.
Moon's horizontal parallax		58	26·6	
Sun's „ „			8·8	
Moon's true semi-diameter		15	55·6	
Sun's „ „		15	47·0	

The central eclipse begins in 116° 34′ west of Greenwich, latitude 18° 0′ N.; it occurs at apparent noon in 44° 50′ W. and 44° 57′ N., and ends in 31° 45′ E. and 25° 21′ N. Other points upon the line of central eclipse in American longitudes are:—

	Long. ° ′	Lat. ° ′		Long. ° ′	Lat. ° ′
Long.	96 15 W.	Lat. 27 9 N.	Long.	76 4 W.	Lat. 36 45 N.
„	90 9	„ 30 7	„	70 29	„ 39 1
„	86 28	„ 31 57	„	58 23	„ 42 47
„	79 17	„ 35 20			

At New Orleans the eclipse will be total for about twenty-five seconds, commencing at 7h. 29m. 23s. A.M.; local mean time with the sun at an altitude of 30°, and at the point 76° 4′ W. and 36° 45′ N., totality will begin at 8h. 47m. 27s. A.M., and continue 1m. 39s., which is about the longest duration of the total eclipse in United States territory, and indeed the longest available for observation upon land, on this occasion. After traversing the Atlantic the moon's shadow enters Portugal near Ovar, where totality lasts 1m. 30s., and passes off Spain about ten miles south of Alicante. In Alicante the total phase will commence at 4h. 10m. 11s. P.M., local mean time, ending at 4h. 11m. 29s. At Greenwich a partial eclipse is visible, magnitude 0·68, at 3h. 54m. P.M.

COMETARY NOTES.—Tempel's comet was detected at the observatory of Arcetri, Florence, on July 19, and as stated last week by Prof. Winnecke at Strasburg on the following evening. From the Strasburg observation it appears that the comet will arrive at perihelion September 7·1646 G.M.T. It has also been observed in this country with the aid of the ephemeris, given in this column.

Of the supposed comet reported to have been discovered by Mr. Lewis Swift on July 7, we have no further intelligence.

In a communication to the Royal Astronomical Society, Mr. Tebbutt, of Windsor, New South Wales, puts upon record the circumstance of his having first determined the orbit of the great comet of 1861, in addition to having been its first discoverer (on the night of May 13). Mr. Tebbutt's name was not associated with this grand object in Europe, in the same manner that the great comet of 1858 had been associated with the name of Donati, for the simple reason that there being no telegraph from Australia in that year, the news of his discovery did not reach Europe till the comet had so far diminished in brightness as to be of interest only to the astronomer. Otherwise a message by cable that a large comet discovered by Mr. Tebbutt might be looked for above the European horizon at the end of June, would doubtless have led to "Tebbutt's comet" being as universally known as was "Donati's comet" three years earlier. The period of revolution of Tebbutt's comet is just under 400 years, while that of Donati's is little short of 2,000.

Encke's comet will just be coming into view at the

observatories of the southern hemisphere, where it is important for the theory of this body that observations should be continued as long as practicable. It was in perihelion on July 26. At the next return in 1881, its track in the heavens will be very favourable for observation in these latitudes.

THE SATELLITE OF NEPTUNE.—In the *Monthly Notices* of the Royal Astronomical Society for June, Mr. Marth has furnished data founded upon Prof. Newcomb's tables, from which the position of the satellite of Neptune may be readily determined for any time during the approaching opposition; but with the approximate times of superior and inferior conjunctions (the angles being 311° and 131° respectively) which are appended, the part of the orbit in which the satellite must be found is easily ascertained without calculation. As has been known since the year 1853, the orbital motion of the satellite is retrograde.

GEOGRAPHICAL NOTES

THE distinguished Italian traveller and naturalist, Sig. L. M. D'Albertis, who has been exploring different parts of New Guinea since 1872, and in 1876-77 made two expeditions into the interior of that country by the Fly River, has arrived in London with his large collections in every branch of natural history.

DR. VAN DER HORCK, who lately made so valuable a tour through Lapland to the Arctic Ocean, is at present organising in Berlin an expedition for the especial purpose of studying the question of the original settlement of America by Asiatics. It is intended that the expedition, which will last from three to four years, shall coast along the entire eastern shore of Asia, up to the Polar Sea, visiting all the chief islands by the way, and then, crossing at Behring's Straits, follow the American coast to San Francisco. The expedition will follow the customary programme of a scientific voyage, making surveys, dredging, collecting objects in natural history, &c. The anthropological features will, however, be the most important, and every effort will be put forth to collect and classify all existing clues to a distant emigration from Asia to America. It is hoped that evidences in this direction may be found on the isolated groups of islands in the more northerly regions. Ample funds have been put at the disposal of the expedition, and it starts under the auspices of the German government and the Berlin Geographical Society.

THE services of the scientific element in the recent Berlin Congress have been recognised by Her Majesty's conferring the honour of knighthood on Mr. Edward Hertslet, C.B., F.R.G.S., Librarian of the Foreign Office and Keeper of the Archives, who perhaps had as much to do with the settling of the boundaries of the new states as the Prime Minister himself.

THE United States Coast Survey steamer *Blake*, which returned some weeks ago from dredging operations in the Gulf of Mexico, is now refitting for another expedition in November. Capt. Patterson, of the Coast Survey, we learn from the *Tribune*, says that the extensive and accurate soundings of the Gulf taken by improved scientific methods in the recent expedition, do not tend to confirm the belief, long held, that the equatorial current, after rushing from the Caribbean Sea through the channel formed by the West India Islands and the northward projection of Yucatan, makes the whole tortuous circuit of the Gulf close by the shores of Central America, Mexico, and the southern coast of the United States, before emerging into the Atlantic between the point of Florida and the Bahamas. The observations tend rather to prove that the force of the incoming equatorial stream expends itself in one direction against the mass of the Gulf long before it reaches the Texas coast, and then turns directly toward and reissues into the ocean. The old theory in regard to the current being unsettled, the expedition now to be made will proceed to repeat certain experiments, and to make others, with the view of either confirming or destroying the latter hypothesis. Exhaustive observations will be made of the region of ocean in and around the eastward islands of the Caribbean Sea, through which the equatorial current makes its entrance, as through a sieve, from the Atlantic into the long channel, 1,500 miles long, formed by the West Indies on the one side and Central and South America on the other, and leading to the Gulf of Mexico. From the experiments and observations of this expedition Capt. Patterson hopes for results which will go far toward laying at rest all merely speculative theories relating to the Gulf Stream. In connection with the investigations hitherto conducted by the Coast Survey expeditions in the Gulf of Mexico, it has been ascertained that the vast current of water pouring from the Gulf into the Atlantic, through the Florida and Bahama gate, has neither the same velocity nor the same temperature. It is believed by Capt. Patterson and his associates that further attention to this curious fact may develop results having an important bearing upon the science of climate and meteorology, making predictions possible as to changes in the seasons of the European countries affected by the Gulf Stream from the observed quantity and temperature of the flow through the Florida Straits.

THE Paris Geographical Society has recently received some interesting intelligence respecting M. Savorgnan de Brazza's explorations on the upper part of the River Ogowé, in Western Equatorial Africa. Having learned from the natives at Dumé that there were some falls on the Ogowé, in the Aduma country, he got together with some difficulty a party to show him their locality. On the way up the river the inhabitants fled from their approach, as they believed that white men brought small-pox with them. On the fourth day the expedition encountered the first rapids on the Ogowé after leaving Dumé; they became more and more numerous, and the breadth of the river diminished considerably. Further on sand-banks extended across the river, which became a succession of rapids in the country of the Atzianas. Among the Akanigues, higher up, the villages are described as very numerous and close together, and the country well cultivated and mountainous; the inhabitants collected on the banks to see the expedition pass. On the tenth day the explorers arrived at the River Passa, which has a strong current, and which is nearly as important as the Ogowé, there become comparatively narrow. On the following day they found themselves face to face with a large sandbank and a strong rapid, which their guides declared to be impassable. Dr. Ballay, one of the party, went by land to Pubava, some twelve miles above the rapids, and found that the Ogowé is not more than twenty metres broad above the falls; he thinks that the River Passa will probably furnish a more convenient route to the eastward than the Ogowé.

AS might be expected, Petermann's *Mittheilungen* for August is largely devoted to papers bearing on the recent changes in the East. First we have an article, written before the conclusion of the Berlin Congress, on the territory claimed by Russia from Turkey in Asia. Next comes an important study on the Kara Kum desert, with reference to the proposed Central Asiatic railway. Following this is a series of tables of population statistics on the Sanjak of Seres, in Macedonia. There is also an important paper on Herero Land, in South Africa, to accompany a new map of the region, based on the observations of the Rhenish missionaries, especially those of Herren Böhm and Bernsmann. The number concludes, as usual, with Dr. Behm's admirable monthly summary. Besides the map of Herero Land, there are maps of Armenia, to show the region claimed by Russia, and of

the south-east of Europe and Turkey-in-Asia, as re-adjusted by the Berlin Congress.

THE Buenos Ayres *Standard* furnishes some particulars of interest concerning Don Francisco P. Moreno's explorations in Southern Patagonia. The River Gallegos, it appears, has a mean velocity of four to five miles an hour, and is fed by the snows which fall in winter on the volcanic lands. It has two sources, which unite after a short distance, and two small tributaries to the south. The valley could be utilised for agriculture; on both banks mounds of lava are met with towards the south, forming black rocks with broken fragments like columns, wearing an appearance of an ancient city in ruins. All these peaks as far as Cape Virgin are extinguished volcanoes, of an average height of 860 feet. Don F. Moreno believes these eruptions to have been entirely independent of the Andine volcanoes. Between Gallegos and the banks of the San Gregorio, where these peaks are found, they have risen more irregularly than in other parts of Patagonia. The road winds capriciously through low valleys, watered by pools and rivulets, then through sterile tracts, and now and again over grassy elevations. At the confines of the Meseta (table-land) the face of the country changes. To the right the blue and white line of the snow-clad mountains stands out in relief. To the left is seen the summit of San Gregorio, then the narrow Straits, and further off the Fuegian Plains, enveloped in fog and lurid fires. At the Brunswick Peninsula on the Straits the landscape is particularly verdant and undulating, with mineral veins here and there and small woods of the "Calafate" (*Berberis*), which produces a delicious fruit. With regard to the climate of these southern regions, in the western part rain and storms are continual, and it would be difficult to populate it, but on the east the climate is more favourable, and those sudden and terrible atmospheric changes that cause so many shipwrecks are unknown. Don F. Moreno is of opinion that the climate from the River Santa Cruz to Cape Horn may fairly be compared to that of Great Britain, from the English Channel to the north of Scotland. On the high lands it is dry, with night dews, but little rain. In winter snow falls, but in spring, summer, and autumn, the climate is delightful, with some few days of intense heat.

THE new part of *Appalachia*, the journal of the Appalachian Mountain Club, contains Prof. Scudder's presidential address, an exhaustive summary of the various North American surveys, and explorations during the previous years. There are other valuable papers connected with the exploration of the Appalachians; one by Prof. C. H. Hitchcock gives an account of a large number of glacial markings in the White Mountains.

A WORK of great interest is now in course of publication at Vienna (Hartleben): "Die Sahara—von Oase zu Oase," by Dr. Joseph Chavanne. Up to this date twelve parts have appeared; they give excellent descriptions of the great African desert itself and of the tribes frequenting it. The scientific materials are worked out into attractive pictures with considerable skill; the reader travels in thought through the Oasis of Ziban, then makes the acquaintance of Biskra and its people, the so-called Paris of the desert, passing across the El Arnat and through the land of the Beni Mzab as far as In Salah and Tafilet. Numerous well-drawn illustrations add considerably to the attractions of the work.

A NEW book of special interest at the present time has just been published by Cotta, of Stuttgart. Its title is "Cypern, Reiseberichte und Landschaft, Volk und Geschichte." The author is Herr Franz von Loeher.

LETTERS have been received in Holland from Vardö, reporting that every one on board the *Wilhem Barentz*, the schooner of the Dutch Arctic Expedition, was in excellent health, and that hitherto the vessel had behaved admirably.

ON PREHISTORIC REMAINS IN BRUNSWICK[1]

A LARGE quantity of prehistoric remains have been found in the diluvial loam of Thiede and Westeregeln, in the Duchy of Brunswick. This fossil fauna of Thiede numbers about thirty species—the common loess molluscs (*Helix hispida*, *Pupa muscorum*, *Succinea oblonga*), together with remains of horse, reindeer, *Rhinoceros tichorinus*, *Elephas primigenius*, some carnivora, and rodents (as lemming, *Myodes torquatus*, *Arvicola gregalis*, &c.). The lemmings appear especially in the lowermost beds, with some few remains of reindeer and Arctic fox; in the middle beds they are associated with remains of horse, rhinoceros, and elephant, and gradually disappear in the higher horizons.

The fossil fauna at Westeregeln, numbering more than thirty species of mammals alone, is far more abundant and more varied. The mammals are bat, shrew, hyena, lion, wolf, Arctic fox, bear, badger, and especially rodents, all of them (the hare excepted), such as habitually live in holes underground, in steppe-like regions. Among them is *Arctomys bobak*. The remains of *Spermophilus altaicus*, of *Dipus*, and of several species of *Arvicola*, are extremely abundant. Lemming, hare, and lagomys are also met with. The presence of horse, reindeer, bos, and antelope has been ascertained, together with some few bones of *Elephas primigenius* and *Rhinoceros merki*. A well-developed trapezium of a peculiar horse may be possibly interpreted as a connecting form between *Equus* and *Hipparion*. The dental system of *Spermophilus* offers likewise some remarkable particularities. The birds are represented by eleven species—ducks, *Gallinaceæ*, pigeon, lark, bustard, vulture, and largely by species of *Fringilla* and *Hirundo*. Remains of bactrachians (*Bufo*, *Rana*, and *Hyla*) abound in certain localities. Of fishes only one-half of a pike's lower jaw has been met with. The number of molluscous species amounts to seven. The co-existence of insects may be inferred from the presence of many insectivorous mammals and birds.

Man's co-existence is attested at Thiede by axes of the stone period, found immediately beneath the humus; also by flint implements, and traces of fire-places in the lower beds of the loess. At Westeregeln splinters of flint, small fragments of charcoal, and broken bones of animals, indicate the presence of human beings, either permanent or occasional, during hunting excursions. According to Mr. A. Nehring, the discoverer of these deposits, their fauna approximates to the Steppe fauna of south-west Siberia, and is co-eval with the post-glacial period, when Northern Europe had an extreme continental climate. In the neolithic period, this fauna, as indicated by the remains of deer, bear, and beaver, assumed a sylvan character, persisting down to the times of Cæsar and Tacitus.

Mr. Nehring supposes the loam in question to have been transported and deposited by water, admitting, however, the occasional transport of materials by storm winds. Dr. Tietze thinks that the presence of fishes and batrachians offers no objection to the sub-aerial, or wind-origin, of this loess, as fishes, frogs, lizards, and tortoises, live on the steppes of Persia, and are even characteristic of their fauna; and fishes are also found in the artesian wells of the Sahara. Some known facts support the idea of the sub-aërial origin of deposits in localities having nothing in common with steppes. In 1866, the wind blowing from the south, the snow was covered with a stratum of yellow dust; the same phenomenon was observed at Kasan in 1872. Mr. D. Stur ascribes to the action of winds the recent thin layers of quartzose and micaceous particles accumulated on the slopes of the limestone Alps.

[1] By M. A. Nehring and Dr. Tietze. (*Proc.* Imper. Geol. Instit., Vienna March 5, 1878.)

THE TAY BRIDGE

THE new bridge across the Tay, at Dundee, recently completed according to the plans of Mr. Bouch, M.I.C.E., by Hopkins, Gilkes, and Co., of Middlesborough, is the longest structure of its kind in the world. What renders it more remarkable than its enormous size even is the originality in its conception and mode of execution, the result of which has been a structure of great stability and comparatively small cost. Popular accounts of the work have appeared in various publications during its progress and since its completion; but there are various scientific aspects of the undertaking which have not been adequately described. The present article will refer to such technical details as may be of value to those who study the application of scientific principles to industrial ends.

The new methods employed in the construction of the Tay Bridge cannot any longer be regarded as experiments. The severe tests imposed by the Government Inspector and by a heavy traffic uninterrupted during several months removed all uncertainty as to their success and make them of greater value. We may now therefore speak of the details of the structure, so far as these are based on scientific principles, with confidence, seeing that the trial of the bridge may now be regarded as conclusive.

The bridge consists of 85 spans varying in length from 27 to 245 feet. Beginning on the south side there are first 3 spans of 67 feet, then 2 of 88, then 10 of 130, then 13 of 145, 13 of 245, 1 of 162, 11 of 130, 25 of 69, 1 of 170, and 6 of 27 feet. The direction of the approaches on both sides made it necessary to introduce a curve of 1,320 feet radius at each end of the bridge; that on the north side extending over 88°, and that on the south over 16° 30′, the central part of the bridge, 7,960 feet long, being straight. Of the thirteen 245 feet spans, 7 are level, and placed so high that they offer a clear water-way of 88 feet at high water. From these northwards, a slope of 1 in 73 brings the structure to the required height to join the land line, while southward an incline of 1 in 365 serves the same purpose. In order to gain as much clear height as possible in the channel used for navigation, the roadway is laid on the bottom of these large spans, the trains running between the girders, while on the other parts of the bridge, with the exception of the 170-feet span on the north side, the sleepers are fastened on the top of the main girders.

Nature of the River and its Bottom.

The Tay is a tidal river about two miles wide at the site of the bridge. The rise and fall of the tide in ordinary spring tides amounts to about 15 feet, but local circumstances affect this height to a great extent, the difference between the observed and the computed heights sometimes amounting to 3 feet. The strength of current is about five knots per hour. Fig. 1 shows the depth of the water and the nature of the river bottom. The greatest depth is found between piers 15 and 20, but a bank a short distance above the bridge closes up what otherwise would be the best channel for navigation. The rocks on both sides consist of trap. At the south side they rise abruptly to a height of 53 feet, the base being washed by the flood-tide. The river bed to 100 yards out from the south shore is composed of gravel overlaying blue clay with beds of peat and large layers of decayed hazel nuts which at one time grew in abundance near the site of the bridge. Further out the gravel gets coarser, and boulders are found on the river bottom. The trap rock disappears at pier 5, and its place is taken by red sandstone with a rather worn surface. At pier 20 the surface soil consists of sand, and continues to do so to within a few yards of the north side of the river. The sandstone has a northerly dip increasing at pier 15, and from this point northward to pier 78 rock is not any longer found at a depth which makes it available for foundation.

The old river-bed, consisting of gravel and clay, continues, however, almost level along the whole line of the bridge. The sand overlaying it is very sharp and pure and contains a large number of shells. Underneath the gravel is a mixture of sand and clay. From pier 70 northward layers of peat and decayed wood are often found in the sand.

Plan for the Foundations.

The following plan was followed for the foundations:—From pier No. 1 to No. 14 the rock had to be reached, the weight per square foot of surface amounting to eight tons. Piers No. 15 to No. 19 consist of a caisson sunk only a few feet into the bottom, and then piled, each pile having to carry a weight of twenty-five tons. Piers No. 20 to No. 80 are founded on the gravel layer, the pressure per square foot being about two tons. Thence to the north shore the piers rest again on rock.

General Description of Piers.

Before describing the *modus operandi* adopted in building the bridge it will be well to give a short description of the piers. The first three commencing from the south side have a rectangular section. At the base they are 14 feet 6 inches by 7 feet 6 inches, and at the top 11 feet 6 inches by 4 feet 6 inches. The clay is excavated at low water, and the rock levelled. A concrete foundation is put in, and on these the piers are erected solid in brick and cement. On the top they have a cope of stone 15 inches in thickness, and on this rest the bed plates and girders. For piers No. 4 to No. 14 two cylinders having at the base a diameter of 9 feet 6 inches are placed at a distance of 12 feet, centre to centre, and connected by a web wall 2 feet 6 inches thick. Under water they consist of brick tubes surrounded by a cast-iron casing 1 inch thick, the centre of each cylinder and the common base being filled up with concrete. Above water the diameter is 8 feet 6 inches below and 6 feet 6 inches on the top, and they are finished off with a course of stone in the same manner as the first three. From piers Nos. 15 to 58 the work above high water consists of iron columns varying in diameter from 12 to 20 inches, and having a thickness of metal of 1 inch. These are braced together by horizontal and diagonal bracings and filled with cement to prevent interior corrosion. In the under water work they differ widely according to the nature of the river bottom and the weight they have to sustain. For those from 15 to 19 a malleable iron caisson 10 feet high, oval in shape with a major axis of 23 feet, and a minor axis of 16 feet is provided with a lining of brick-work 9 inches thick and sunk a few feet into the river bottom. Forty piles are then driven inside as far as a one-ton ram falling from a height of 10 feet will drive them. The depth to which they penetrate varies from 15 to 20 feet. These piles are cut off 3 feet above the ground, and the caisson is filled up level with concrete. On top of this concrete a hollow brick pier, hexagonal in shape, is placed, and the space inside up to low water is also filled with concrete. From low to high water the concrete is replaced by solid brickwork, and a course of stone brings the height of these piers to 5 feet above high water. Piers Nos. 20 to 27 are constructed by joining two 15-feet diameter cylinders above low water, after sinking them through the sand to the gravel bed. They are then carried up in the same manner as described for 15 to 19. The fourteen piers supporting the thirteen large spans have a 31-feet malleable iron cylinder with a lining of brickwork 14 inches thick, and are sunk to the gravel layer, and of such height that after reaching the solid bottom their top is slightly projecting above the river bottom. They are then filled in with concrete, and a brick block corresponding in shape with those used for the other piers, but of greater dimensions,

measuring 27 feet in the longest, and 16 feet in the transverse direction, is put on the foundation, filled with concrete, and carried up solid to high water, where it has a cope 5 feet thick. For the 162 and 130 feet spans on the north side the construction is similar to that adopted for piers 20 and 27, while the shorter spans in

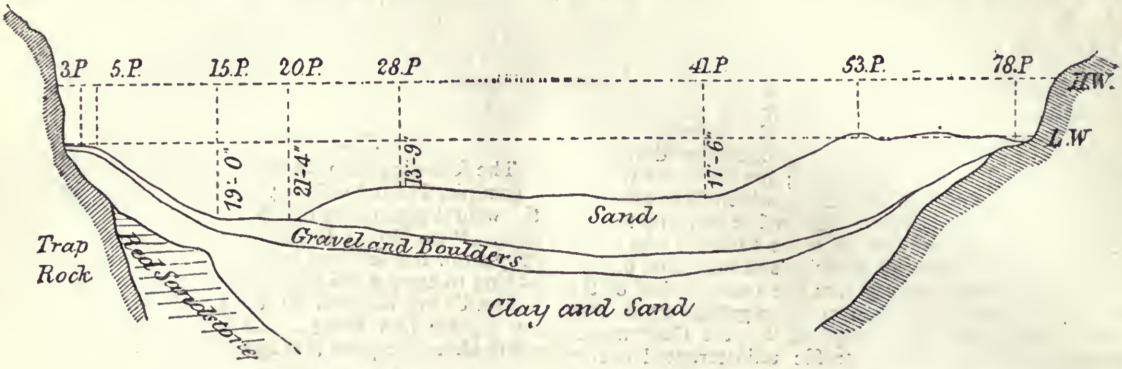


FIG. 1.—Section of river. Horizontal scale 2,000 feet to 1 inch. Vertical scale 50 feet to 1 inch.

the curve are supported by columns set in 6-feet cylinders sunk to the required depth and filled up with concrete.

No Staging to be Used in the Construction.

The change in the nature of the river bottom, the great height at which the bridge had to be constructed and the heavy gales which are prevalent in this part of the country made it necessary from the beginning to depart from the rules generally laid down for the construction of such works. The erection of scaffolding and staging was by these circumstances rendered impossible; and a series of operations were substituted for these old methods which by their success proved to be great improvements, and must have great interest to engineers and others less directly connected with such

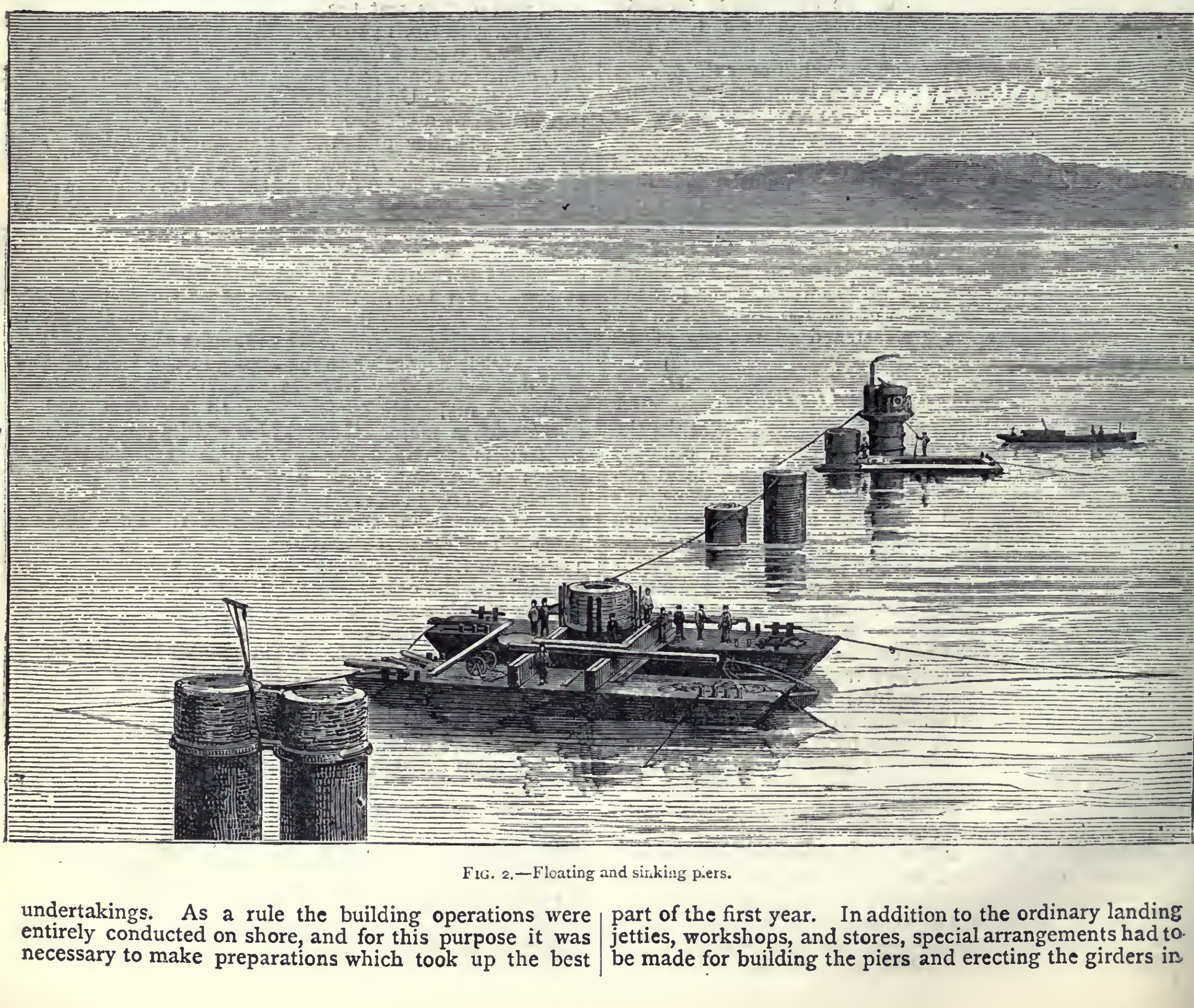


FIG. 2.—Floating and sinking piers.

undertakings. As a rule the building operations were entirely conducted on shore, and for this purpose it was necessary to make preparations which took up the best part of the first year. In addition to the ordinary landing jetties, workshops, and stores, special arrangements had to be made for building the piers and erecting the girders in

such a manner that they could be transported to their permanent places without great difficulty. Level foundations of concrete and stone were constructed on the foreshore, some of which were used to put together and rivet the iron caissons forming the outer shell of the piers, others to build the brick work for that part of the piers between the river bottom and low water.

Building and Floating the 9-feet 6-inch Cylinders.

After the caissons had been put together and the brickwork built in, they were floated between barges to the point where they had finally to form part of the structure.

The apparatus employed for lifting them consisted of a system of iron girders resting on barges. Flat bars or links having 2″ holes 12″ apart, connected the base of the piers with hydraulic rams placed on these girders. The weight could either be taken on the girders direct or on the rams. At high water the girders were floated over the top of the piers, and after the tide had receded, and the base was accessible, the connection between the lifting links and the base of the cylinder was made. As the tide rose again it lifted the barges, and with them the pier, hanging between them off its resting-place, when it could without difficulty be towed to its destination, where previously moorings had been prepared for it. For this operation it was of course necessary to have a tolerably calm day, and as the weather in the Tay is subject to very

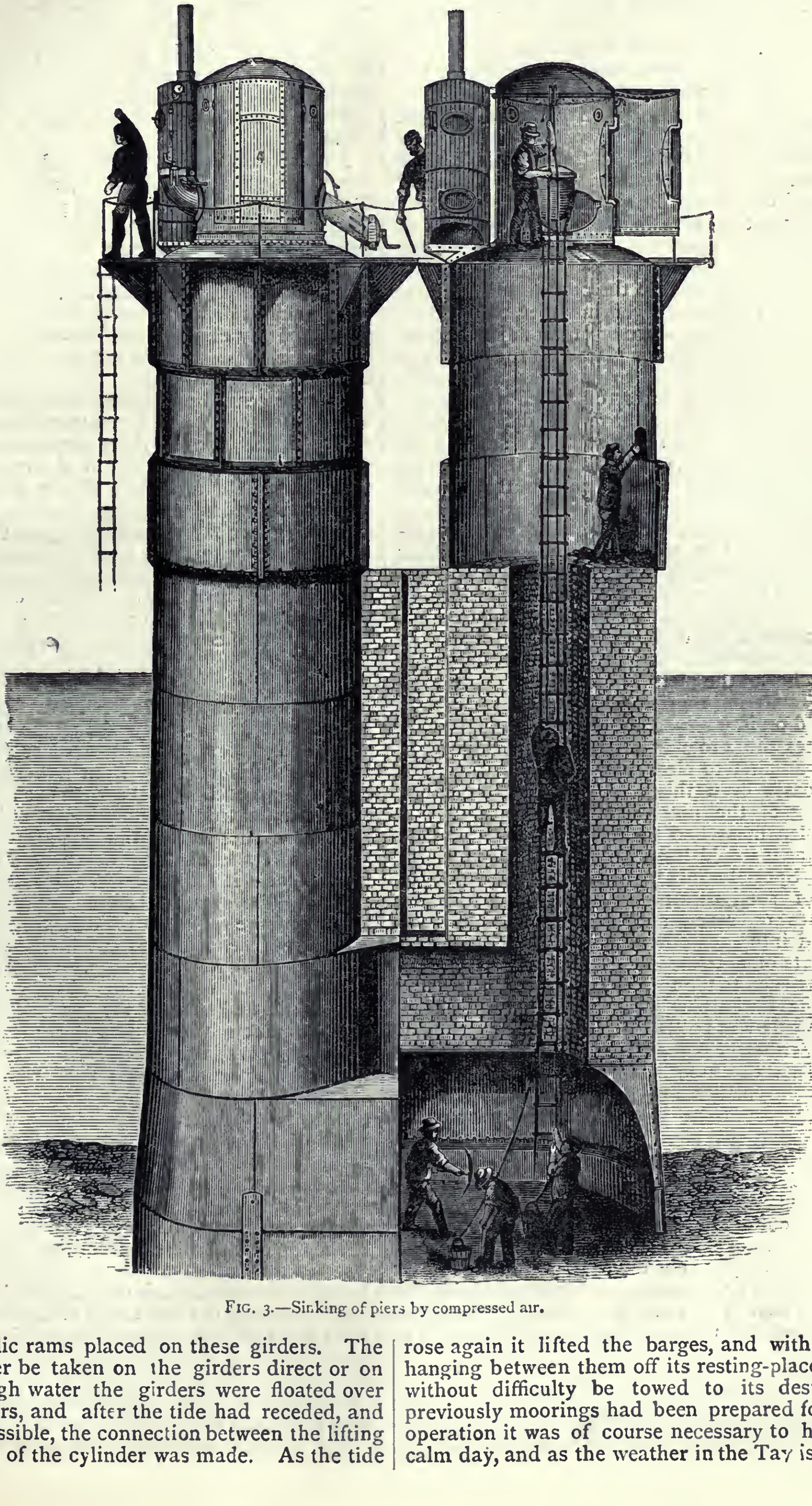


FIG. 3.—Sinking of piers by compressed air.

rapid changes, the operation was sometimes interrupted and frequently endangered by sudden gales, it becoming necessary to take the piers away from the place where it was intended to lower them, and as they could not be placed back in the same position they had to be taken to Dundee Harbour for shelter. The weight of these piers was about 100 tons, and the carrying barges were 60 feet long by 12 feet wide, and 6 feet deep. If the weather continued favourable, the work of lowering took place the same tide. For this purpose the rams were pumped up 12″ while the weight was on the steel pins resting on top of the girders. The connection between the rams and the links was then made, whereupon a few additional strokes relieved the lower pins of the weight, so that they could be withdrawn and inserted in the next hole. By allowing the water to leave the rams, the pier would of course be lowered till the weight came again on the lower pins, after which matters were in the same position as at the beginning, and the operation could be repeated, and the pier lowered foot by foot.

No great attention was paid to the position of the pier till it neared the river bottom. Then, however, an observer with a theodolite placed on shore or on an adjacent pier, directed the men at the capstans placed on the barges to draw them to such a position as to have the pier exactly in line, while double steel wires, of which the length had been carefully determined, gave the distance from the pier previously placed in position. As soon as the position was in every respect satisfactory the lowering was proceeded with quickly, and it became in this manner possible to put a pier very nearly in its right place. Especially in the curve this work required great attention and care, but it has admirably succeeded. After disconnecting the links which joined the lifting girders to the piers the rising tide would float these with the barges over the top, leaving the cylinder standing on the river bottom. Fig. 2 shows one of these cylinders moored in the river and the general arrangements for floating and lowering. Another of the piers is ready to 5′ above high water, and others are in progress. One has the pneumatic apparatus placed on it.

In most cases the ground was sufficiently level, in others divers had first to clear the bottom of boulders, and in three cases even this was not found effectual, and the settling of the pier on one side caused it to upset. The fallen mass had to be lifted out of the road and broken up as it would have been too costly to set it on end again. Special struts to prevent the possibility of a recurrence of such disasters were then introduced. Two iron tubes of seven and eight inches diameter respectively slid into each other by means of a stuffing-box like the parts of a telescope. The upper end of the top part was fastened to the top of the pier and connected with a pump, the lower part had a mushroom-like base, and when this part was pulled away from the pier under an angle and then water pumped in it took a firm hold in the ground. Four of these struts applied to each pier held it perfectly in position as long as the water pressure was not withdrawn from them. In the subsequent operation of sinking the pier the cocks giving egress to the water were left open, and as long as the motion of the pier was a vertical one the water would escape from all four struts in a gentle flow. As soon, however, as the pier went out of the perpendicular the strut on that side would eject a stronger stream of water, and the man in attendance had only to shut the cock pertaining to that strut to make it answer its purpose of supporting the pier on the side to which it was inclined to fall. This apparatus fully answered the purpose for which it was constructed.

Sinking by the Pneumatic Process.

In order to sink the piers through the layer of clay overlying the rock the pneumatic process was employed. Temporary castings were added to the top of the pier bringing the latter above the highest water, and a malleable iron air-bell, having the usual air-lock for the passage of the workmen, and smaller locks for the passage of excavating material to the outside or concrete to the inside was placed on the top (Fig. 3). A six horse-power steam-engine, connected with an air-pump, supplied the air by the pressure of which the bottom of the pier was kept dry. The breathing of the men and the burning of candles required, of course, a more liberal supply than would have been necessary for the first purpose only. Instead of allowing this surplus to escape at the bottom of the pier it was used to drive a pneumatic hoist lifting the excavated materials up to the air-lock.

As the excavations proceeded, the pier would sink down till it reached the rock, and this movement was facilitated by the bottom of the pier being slightly conical. The rock was then levelled down by pick-axes and chisels so as to give a firm bearing, and concrete material introduced filling up the bottom chamber and the shafts which had been left in the brick-work for the passage of the men. When this work had proceeded to above low-water, the temporary castings and air-bells were removed and the pier built up between low and high water in the usual manner from a barge.

This process, though slow and costly, worked on the whole satisfactorily, so far as the piers were founded upon rock. It was carried through without many accidents, though one occurred in which six men unfortunately lost their lives. On a stormy day in August, 1873, a barge loaded with coals, moored near one of the piers in process of sinking, probably knocked against the structure and must have damaged the iron—though it was not observed at the time—to such an extent that at the next high tide, when the air pressure was greatest, one of the top plates was blown out, the water rushing in from both top and bottom, and of course making escape an impossibility.

Piled Piers.

The oval caissons for piers Nos. 16 to 19, were built, and floated out in precisely the same manner, and then excavated under water by a diver (the excavated substance being hoisted up from a barge) and sunk 2 feet into the ground. A staging constructed of angle iron was then floated out and put over this caisson, and was used as a support for the pile driver. The piles had a length of 60 feet. The depth at the highest tide was 45 feet, and it was therefore possible to drive them 15 feet into the ground, before any means of lengthening them for the purpose of driving, had to be adopted. Divers were engaged to cut them under water, leaving 3 feet standing in the caisson. Concrete lowered in large buckets, which only opened when getting to the bottom, surrounded and covered the heads of the piles, the height of the concrete being 7 feet above these heads. During all these operations the caisson extended temporarily to above high water, so that the diver could continue his operations during the whole of the tide, notwithstanding the strong current, and the danger of washing the essential parts of the concrete away was considerably lessened. An inspection, undertaken for the purpose of ascertaining how far the latter had been successful, gave very satisfactory results, the whole mass of the concrete being found perfectly hard after a few days. The disconnection of the temporary caisson from the lower part, and floating away in the same manner as that by which it had been brought out was a work of little difficulty.

The floating out and fixing of the brick pier which was put on top of this concrete foundation will be described more fully with the operations necessary to complete the large piers. These are certainly the most interesting ones on account of their magnitude, although this very size may have been the means of avoiding particular difficulties which stood in the way of the work connected with the

smaller piers. It has been found throughout the operations that in a river subject to such vicissitudes as the Tay, the handling of a very heavy mass is a far less intricate and difficult matter than that of lighter weights, and during the execution of the work connected with the larger spans the operations were not in the least interrupted by weather which would have been absolutely fatal to the smaller piers.

Work on the 245-feet Span Piers.

The under-water work of the large piers can be conveniently divided into three parts: that of building, floating, and sinking the caisson; that of building, floating, and fixing the brick pier; and that of bringing it up to the required height five feet above high water. The first two are, of course, by far the most important. The lower caisson consisted of $\frac{5}{8}$ inch malleable iron plates riveted together so as to form 31 feet cylinders 20 feet high. After erecting this structure on the foundation on the foreshore prepared for it, a lining of brickwork 14 inches thick was built inside. This lower part was to form part of the permanent structure. On top of it and connected with it by a bolted horizontal joint, another length of plates was built bringing the whole up to 40 or 44 feet of height, according to the depth of water. This top part merely served a temporary purpose, and in order to resist the effect of the waves and current it was provided with angle iron stiffeners to a far greater extent than the permanent part, which derived sufficient stiffness from its lining of brickwork.

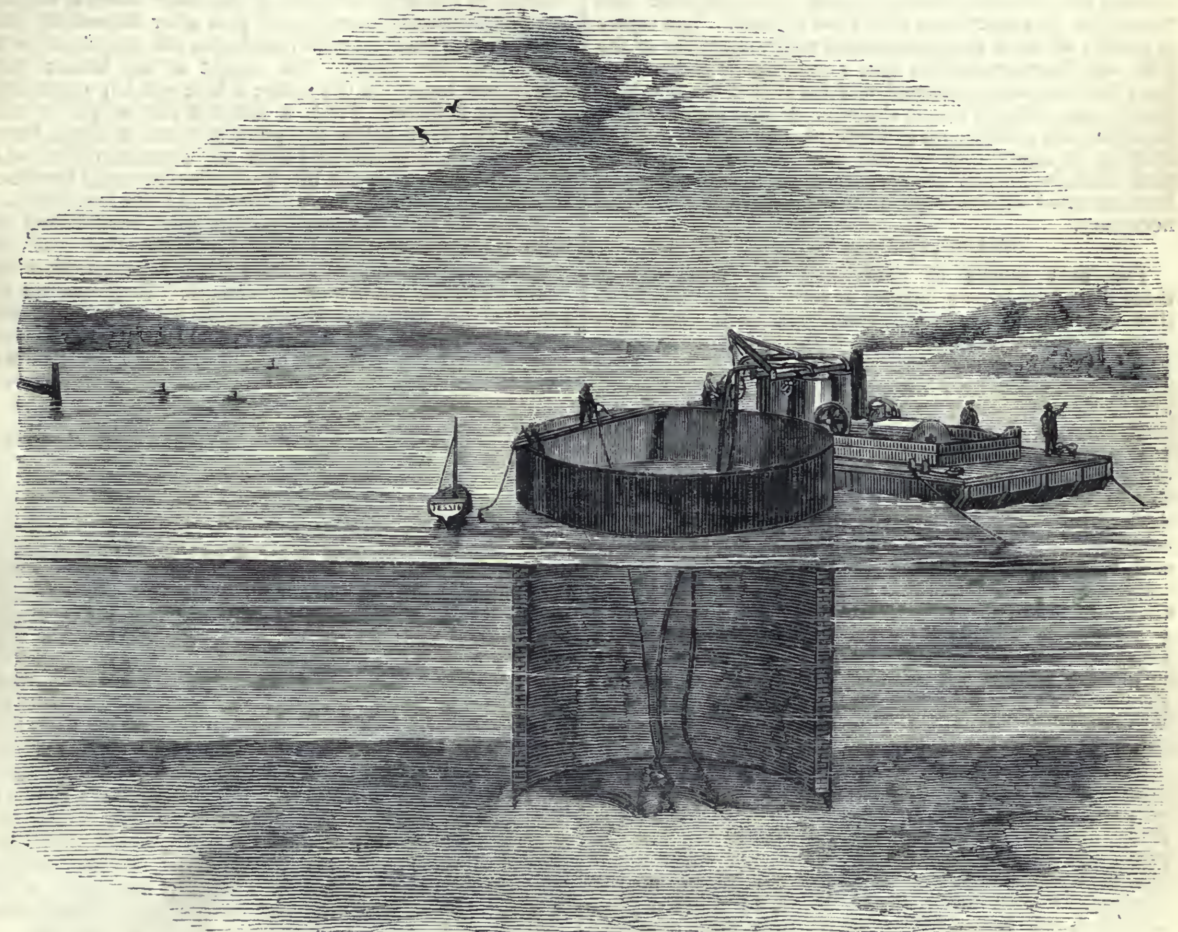


FIG. 4.—Sinking of large caissons.

At four points of the circumference T iron strips ran vertical from top to bottom, and were riveted to the caisson. In these strips or links there occurred three and a quarter inch holes at intervals of 1 foot, and by putting steel pins of slightly smaller diameter through these holes, it became possible to obtain a hold with the hydraulic rams by means of a girder with a claw-like end placed on their tops. The hydraulic rams were placed on the four corners of a square system of girders having projecting ends for the barges. On the under-side of these girders a claw similar to that found on the top of the rams made it possible to take the weight of the pier either on the four lower claws, or the four top claws, which were connected with the hydraulic rams.

Two pontoons, each 70 feet long, 20 feet wide, and 7 feet deep, lifted the weight of girders and the piers standing in between them off the ground by means of the lower claw. It was then floated out; and so powerful was the resistance which the heavy mass, weighing about 200 tons, offered to the current, that two strong tug steamers had to be employed for this purpose. Arrived at its destination, it was securely moored. The first one floated in this manner had to withstand the fury of a three days' gale, during which it had to be left out in the river, the waves washing right over the barges, the hatchways having been nailed and caulked to keep the water out.

To lower the caisson the rams were pumped up 12 inches and the steel pin which connected the upper claw

with the caisson inserted. By pumping up again the weight was taken off the lower claw, the pin which had held it there withdrawn and inserted in the hole a foot higher up. The water was then allowed to flow out of the rams and the weight to come down on the lower pin again, during which operation the caisson had necessarily been 12 inches lowered. The work of adjusting it in its right place was accomplished in the same manner as described for the smaller piers. The first of these caissons was lowered down on August 28, 1875, and the same afternoon the floating girders and barges were withdrawn, and the pier left standing on the sandy bottom. The tide was particularly strong, and from the resistance which this mass offered to its flow washed away the sand round about it, thereby causing the pier to heel over to such an extent that next morning it was more than 6 feet out of level. The centre of gravity of these piers, compared to the diameter of the base was, however, so low that no serious apprehensions were felt, and when the sand was excavated on the high side it soon came to the level again. It was always found possible with a little care to keep the pier nearly level in sinking by excavating on one side or another, and no such artificial means of keeping them upright, as had been adopted in the case of the smaller piers, became necessary.

Sinking the Large Caissons.

The best means of excavating the sand out of these piers had been a subject of very careful consideration, and numerous experiments had been tried with varying results, in order to find the best machinery for effecting that purpose. Finally, however, an apparatus constructed upon very simple principles by Mr. Reeves, one of the contractor's assistant engineers, was found the most efficient. The conditions which it had to fulfil were of this kind. It had to elevate the sand from the inside of the cylinder and drop it outside at a small cost, and it had to be constructed in such a manner as to be easily detached from the pier in case of a sudden gale. These conditions were fulfilled by placing the whole apparatus on a barge which could be moored alongside the pier and withdrawn with little trouble and at a moment's notice. On this barge were placed four air-tight tanks each having a circular hole in the bottom, closed by a door on which an india-rubber ring was fastened so as to obtain an air-tight closing. A steam-engine worked two exhaust air-pumps, and each of these pumps was so connected with two of the tanks that by means of a three-way cock either the one or the other could be put in communication with it. From the top of the tanks flexible pipes of 4 inches diameter ran down to the bottom of the pier, and their ends were in charge of the diver, who could direct them. Either of these tanks being put in communication with the pump would be exhausted, and a current of water mixed with sand would rush up the flexible hose, filling up the vacancy. A float indicated the height to which a tank was filled, and before any water or sand could enter the air-pipe and reach the valves, the attendant had to turn the cock which brought the other tank into communication with the exhaust-pump and admitted air into the full one. It then discharged its contents through a hole in the bottom of the barge into the river. The joints of the cylinder not being water-tight, the level of the water inside was always as high as that outside, and the only purpose of the temporary caisson was to protect the divers from the strong current, and give those in charge of the operation an opportunity of observing the position of the pier during its descent. Fig. 4 shows this apparatus at work by one of the large piers.

Building the Brick part of the Pier.

It will be understood that by the sinking of this caisson a hole was formed in the sand, the sides of which were kept up vertically by the caisson while the bottom was formed by the hard gravel layer. This pit was now filled with concrete to a couple of feet above the river bottom, and all the iron above that point removed. The concrete hardening became like a rock with a level surface of 31 feet diameter. The remaining part of the pier up to high water consisted of brickwork only. The lower portion was built on a level part of the foreshore in bricks and Portland cement, which became so hard that the block could be lifted from four points by means of barges and apparatus similar in all but the dimensions to that used for the floating of the 9′ 6″ piers. Previous to the building a layer of paper was spread on the temporary foundation in order to prevent the brickwork from adhering to it. The block was hexagonal in shape, measuring 27 feet in one direction and 16 feet in the other. Its height would depend on the depth of water at the place where it was to be used, and was so regulated that after depositing the block on the artificial rock foundation the top would be a little above low water. The weight of the blocks was about 200 tons. In the centre a hole was left answering a double purpose. First, it allowed the weight of the block to be kept below the point at which it would have become unmanageable and than the concrete which was put in this hole after the block was *in situ* would increase the adhesion between the brickwork and the foundation.

From low to high water line the pier was finished in solid brickwork set in Portland cement, the work of course being interrupted at every rising tide and taken up again when the water was low enough. Four courses of ashlar of an aggregate thickness of 5 feet brought the pier to the height at which the cast iron columns commenced and the bolts to hold down their bases were fixed in the stone.

Superstructure.

In the erection of the iron superstructure the same principle of not employing staging in the river was adhered to. The iron-work as it arrived, in pieces of from 20 to 35 feet long, from the contractor's works at Middlesborough, was landed on a jetty near the shore about 260 feet long, over which a strong travelling crane could be moved from end to end. Here the parts were put together and riveted up. All the girders with the exception of the 170-feet span and the 27-feet spans, are parallel lattice girders having diagonal struts and ties. The section of the booms is trough-shaped, that of the compression diagonals H-shaped, while the tension bars are flat, varying in thickness from three-eighths to five-eighths of an inch. Those girders which have the roadway on top have half struts running up from the intersections of the diagonals to the top boom, while those where the load is at the bottom have tie-bars for the same purpose. The depth of the 245 feet spans is 27 feet, their width 15 feet, and their weight about 190 tons. In them the rails are carried on longitudinal sleepers resting on iron cross-sleepers 5 feet apart, while in all the other spans wooden sleepers 3 feet apart, and having a section of 12 by 9 inches, carry the roadway.

Floating the Girders.

When a girder was completely erected and riveted up, parts of the jetty on which it rested were removed at each end, and barges were introduced at a low state of the rising tide in the gaps. As the height of the water increased these barges would touch the under-side of the girders, and a further rise would lift them off the bearings on which they had been erected, and keep them floating on the two pontoons. It will be seen that in order to get the ends of the girder free for depositing them on the piers, it was necessary to keep the barges at some distance from these ends and to introduce temporary vertical struts between those parts of top and bottom boom which rested on the barges. In addition to these struts some others were used to steady the comparatively high and narrow girders on their floating supports in case rough weather should

occur during the operation. For the different sizes of girders, different jettys and pontoons were used, but apart from this the operation was carried on in the same manner in all cases. But here again it soon became clear that while to carry out the smaller ones in doubtful weather was a dangerous operation, the larger ones were far less liable to disturbance by the weather, and some of them in fact were subjected to a severe trial in this respect, the two tugs on one occasion being unable to tow the structure in the ordinary manner against the strong westerly wind, and breaking all the tow ropes. As soon, however, as additional towing power had been procured, the operation was successfully carried through. Intricate as it seemed to build these heavy girders on shore and tow them to their destination—often more than a mile distant—by the use of plant specially designed for the purpose, the execution became very easy, it seldom taking more than twenty minutes to convey them from the one place to the other.

Lifting the Girders to their Permanent Resting Places.

The general arrangement of the six columns which have to support the larger spans follows that of the brick piers on which they rest. Four centre columns placed in a square and connected by strong vertical and diagonal bracing, are 15 inches in diameter, and from these centre columns the lifting of the span takes place. Each of the columns is about 10 feet long, varying slightly according to the height of the pier, and every second length has brackets cast on to which transverse girders are bolted. The outside columns placed on the point of the pier are 18 inches diameter, and as well as the inside ones 1 inch thick. Between these outside and inside columns rest the ends of the main girders, the end cross girder and end horizontal bracing having been left out so as not to come in the way of the four central columns and their bracings during the process of lifting. To each vertical end post a T strip is bolted from top to bottom in the same manner as was described for the large caissons. In common with these it has $3\frac{1}{2}$ inch holes at 12 inches distance. A transverse trough girder of slightly shorter length than the distance between these end posts is placed on the temporary girders connecting the columns, and in this two hydraulic rams, carrying another girder are fastened. By inserting steel pins in the holes nearest to this top girder, and then pumping up, the main span can be raised to such a height as the stroke of the rams will permit, and if at that point pins are put in the holes then nearest to the fixed or lower ram girder and the weight allowed to come down upon these, the rams can be lowered again without letting the main girders down, and by a shifting of the pins to a lower hole, the operation can be repeated, each stroke of the rams causing a rise of the girders of 1 foot. The motive power is derived from a pump worked by manual labour, the space at disposal not permitting the introduction of steam-power for this purpose. The diameter of the pump-plungers must therefore be small, and the movement of the rams consequently a slow one. It was, however, possible to lift 20 feet a day, and during the long days of summer a 40-feet lift has often been reached in 24 hours. Another set of apparatus was kept in readiness on the next lifting girder, and with it operations were continued, when the girder had become too high for the lower ones, which then in their turn were shifted to a higher point. As the girder rose, the bracings connecting the outer columns with the four central ones were put in, and after it had reached its final height a system of girders capable of carrying the bed plates and superstructure were fastened on the top of the columns, and the girder which had been elevated to a few inches above its proper height was lowered down on them. Before this could, however, be done, they had to be connected to the next set of girders, as they were calculated as continuous girders and joined together in sets of four. To make these junctions, one of the ends had to be lifted up from 5 to $6\frac{1}{2}$ inches, while the junction plates making the top and bottom booms continuous were riveted on. The lowering of the girder at the other end would then produce an initial strain in these junctions and fulfil the requirements of continuity. A good deal of time was required for making these junctions, the junction plates having all to be carefully marked and drilled while the girders were still hanging in the apparatus, and this circumstance in one case led to a disaster which caused great loss and delay. On February 3, 1877, while the work of joining the two southmost of the 245 feet spans was in preparation, a gale of unprecedented severity came down the valley of the Tay with such a fierceness and suddenness that it was even impossible to get near enough to the piers to take off the men who had been at work on them, and they were obliged to seek shelter on one of the adjacent girders which had not been raised. The wind continuing to blow in strong gusts produced violent vibrations in the unbraced ends of the girders and gradually shifted them to the edge of the lifting girders, causing one side to fall when the limit of stability was reached, breaking down the pier and precipitating the other girder in consequence. The fallen girders had to be lifted, cut up, and replaced by new ones, and the work was thereby materially delayed.

Lifting of the 145 *Spans.*

The lifting of the 145 spans was accomplished in a somewhat different manner. The hydraulic lifting apparatus was placed on the top of a temporary structure of timber fixed on the cast-iron piers, a few feet higher than the top boom of the girder would be after being raised, and long links with holes a foot apart reached to where the girder stood 5 feet above high water. These girders being so much lighter could be raised to the top in one day, but as the arrangement of the columns was somewhat different from that of the 245 feet piers (the girders resting on the four central columns), they were hoisted while fixed together by temporary transverse bracings, which kept them at a greater distance apart than they would finally be, and hoisting them up outside the columns altogether. It was therefore necessary to modify the apparatus to this extent that the girders, after disconnecting the temporary bracing, could be slung in and permanently braced.

The work in connection with this bridge was begun in the summer of 1871, the first stone of the land pier on the south side being laid on July 22 of that year. During the first three years, however, little progress was made, and the operations during that time must be considered as being more of an experimental nature. From August, 1875, however, the progress was very great, and as the managers and men gained experience, the erection of the structure was proceeded with at a vastly accelerated rate. New workshops, jetties, and appliances of various kinds were added, a foundry erected for the casting of columns, and in September, 1876, it was found necessary, in order to keep pace with the building of piers and erecting of girders on shore, to work night and day, and the contractor introduced for the first time in Scotland electric light for out-door work. Two lamps, each of 1,000 candle-power, the current for which was generated by Gramme machines, did excellent service. The lamps were placed at right angles to each other, and in this manner they lit up an area of 100 by 500 yards in such a way that every kind of work could be carried on uninterruptedly. This was of great importance for the floating out of piers and girders which had to be done at high water, the preparations commencing three or four hours beforehand, and therefore having sometimes to be made in the early morning. The last pier was in this manner floated out at seven o'clock on the morning of December 26, 1876.

The bridge was severely tested by Gen. Hutchinson, the Government Inspector of Railways, in February of this year. Five locomotive engines of 72 tons weight each were placed on the large spans and run over them at considerable speed. This weight of 360 tons for each of the 245′ spans will never be reached in the working of the railway, 162 tons being the greatest load resulting on each span from the heaviest goods train. Under the test load the deflection of any of the spans did not exceed the calculated limit, and the lateral movement during the passage of trains was but trifling.

On June 1 the bridge was opened for ordinary traffic, and it is now daily crossed by numerous and heavy trains. For the North British Railway Company and the travelling public its completion is of very great importance. But the work must have a still greater importance in the eyes of engineers and those interested in the practical application of scientific principles, as many new methods to overcome formidable difficulties were successfully carried out, some of which might with advantage be used in similar structures. A. GROTHE

THE NORWEGIAN NORTH ATLANTIC EXPEDITION

THE expedition left Bergen on June 15, and proceeded without interruption to the Westfjord, in Nordland, where we had our first station. A temperature-series was here taken with Negretti and Zambra's new deep-sea thermometer, which showed 10°·7 C. on the surface, a minimum of 4°·4 in a depth of 40 fathoms, and 6°·5 at the bottom in 340 fathoms. The *Vöringen* stopped some hours at Tromsö to take on board a pilot, and proceeded to the Altenfjord, where we found 7°·3 at the surface, a minimum of 2°·7 in a depth of 100 fathoms, and 3°·9 in 220 fathoms at bottom. From Alten we went to Hammerfest, where we stayed two days. Our next stations were in the Porsangerfjord and in the Tanafjord. On all these stations we dredged and trawled with good success. On June 26 we were lying at Wardö, where Capt. Wille made magnetical observations of force and inclination. The declination was determined the day before off the coast by going round with the ship, and taking bearings of the sun on different courses. On June 27 we put to sea on an eastward course. The barometer was falling rapidly, and at midnight the wind and sea got so heavy, that the ship was put with the stem against the wind, and we were lying almost still. This situation lasted till the next night, when we again proceeded on our course, but very slowly, the ship pitching heavily, and the wind being constantly ahead. In this manner we found 0° C. at the sea bottom, in about 71° 30′ N. and 36° 30′ E. from Greenwich. We then sailed northwards and westwards, and passed the said bottom temperature several times, so that I am now able to give its situation pretty accurately in the chart. The northern part of this zero line forms a bay east of Bear (Cherry) Island, where the warmer water reaches a higher latitude, and runs close to the east side of that island, where we for the first time observed the polar ice. The temperature-series showed the accuracy of Weyprecht's observations, that the colder Polar water edges itself along the bottom from the east and north, and the warm Atlantic water runs out in a similar edge in the contrary direction towards Novaya Zemlya and Franz-Joseph Land. On July 4 we were at Bear Island, on the south-east side. We were happy to find the sky almost perfectly clear, a rather rare occurrence on this island. The ship was anchored outside the open coast, and we went on shore at the mouth of a little river, in the vicinity of which there stands a hut, which has been the abode of wintering parties. Here we deposited the mail which we brought for the Dutch polar expedition in the *Wilhem Barentz* schooner. The place was marked with a flag, and the letters, &c., dug down, inclosed in an outer wooden and an inner sheet-iron soldered box. I took a tour upon the nearest hills, collected some rock specimens, and measured the altitude of the highest peak on Bear Island, Mount Misery. The calculation gave me a height of 1,787 English feet, a result which I regard as very accurate. After dinner we weighed and proceeded to the south-west; crossed, the following day, the zero line of bottom temperature on the bank between Bear Island and Norway, sounded in 1,024 fathoms on July 6 in lat. 73° 6′ N., long. 11° 56′ E.; we went then east-south-eastwards, crossed again the zero temperature line, and shaped our course for Hammerfest, where we arrived on the 8th. The ship is now cleaned; we take in coal, and expect to be ready to sail on a westward cruise in three days. Our first cruise has yielded thirty-eight soundings, seventeen temperature series, ten dredgings, and seven trawlings, all successful. New species of animals have been found by our zoologists. Negretti and Zambra's newest reversible deep-sea thermometer has done us great service; the instrument has, almost without exception, worked very well. I have constantly compared its readings from the bottom with the reading of the Casella, Buchanan's improved form, and found a very close agreement. As I supposed, the wooden box which carries the thermometer gets water-soaked after a few experiments in a few hundred fathoms, so that it no longer floats, but this is no drawback, as the lead rushes down so fast that the thermometer always keeps its upright situation till it reaches the bottom, and it never requires more than three minutes for a perfect accommodiation to 0·1 of a degree. I therefore regard this instrument as a very important improvement, and feel much obliged to the inventors and makers.

Hammerfest, July 10 H. MOHN

THE TASIMETER

MR. EDISON has applied the principle of his carbon telephone to a new instrument which is said to be a measurer of infinitesimal pressure. The principle is the variation of the electric resistance of a carbon button due to variation of pressure, and the instrument is said to be an extremely delicate thermoscope. We have not yet, however, received any authentic account of its performance or of its accuracy, but its ingenuity certainly deserves a description, for which we are indebted to the *Scientific American*.

It is the outcome of Mr. Edison's carbon telephone. Having experimented with diaphragms of various thicknesses, he ascertained that the best results were secured by using the thicker diaphragms. At this stage, however, he experienced a new difficulty. So sensitive was the carbon button to changes of condition, that the expansion of the rubber telephone handle rendered the instrument inarticulate, and finally inoperative. Iron handles were substituted with a similar result, but with the additional feature of musical and creaky tones distinctly audible in the receiving instrument. These sounds Mr. Edison attributed to the movement of the molecules of iron among themselves during expansion. He calls them "molecular music." To avoid these disturbances in the telephone, the handle was dispensed with; but it had done a great service in revealing the extreme sensitiveness of the carbon button, and this discovery opened the way for the invention of the new and wonderful instrument.

The micro-tasimeter is represented in perspective in Figs. 1 and 2, in section in Fig. 3, and the plan upon which it is arranged in the electric circuit is shown in Fig. 4.

The instrument consists essentially of a rigid iron frame for holding the carbon button, which is placed between

two platinum surfaces, one of which is fixed and the other movable, and in a device for holding the object to be tested, so that the pressure resulting from the expansion of the object acts upon the carbon button.

Two stout posts A, B, project from the rigid base piece, C. A vulcanite disc, D, is secured to the post, A, by the platinum-headed screw, E, the head of which rests in the bottom of a shallow circular cavity in the centre of the disc. In this cavity, and in contact with the head of the screw, E, the carbon button, F, is placed. Upon the outer face of the button there is a disc of platinum foil, which is in electrical communication with the battery. A metallic

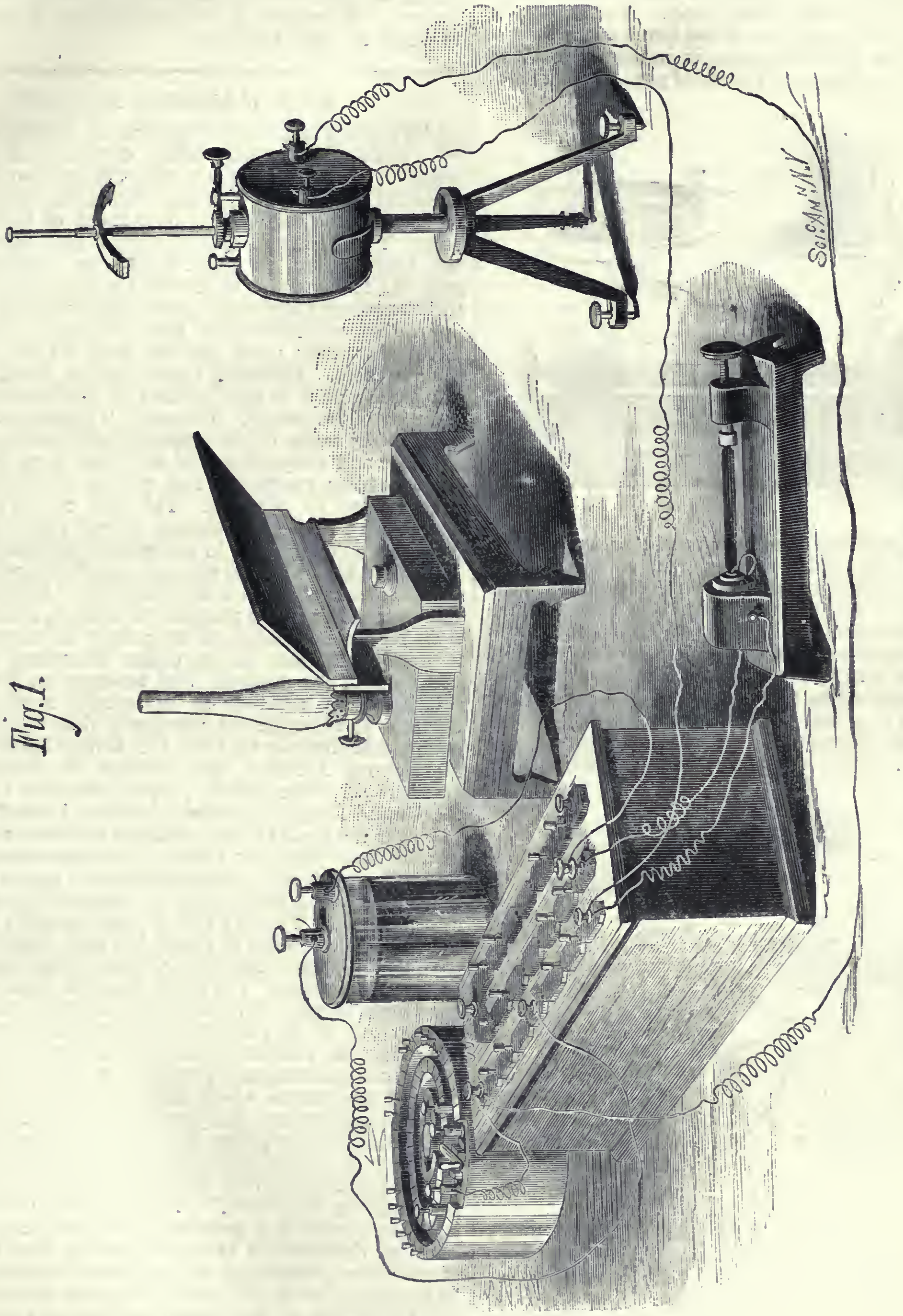


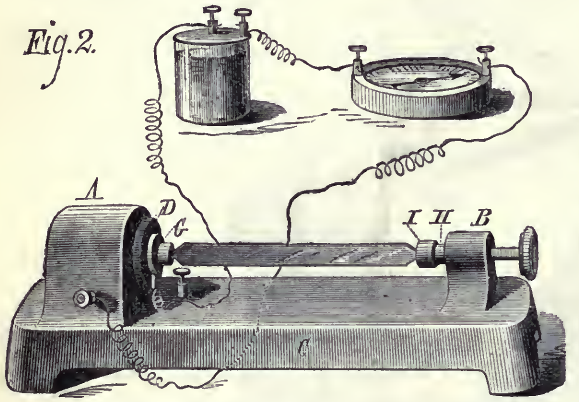
Fig. 1.

EDISON'S MICRO-TASIMETER.

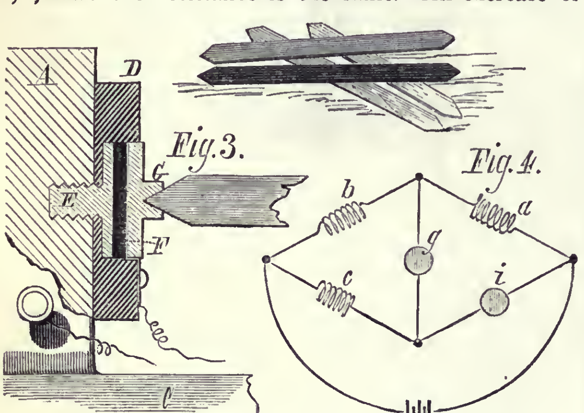
cup, G, is placed in contact with the platinum disc to receive one end of the strip of whatever material is employed to operate the instrument.

The post, B, is about four inches from the post, A, and contains a screw-acted follower, H, that carries a cup, I, between which and the cup, G, is placed a strip of any substance whose expansibility it is desired to exhibit. The post, A, is in electrical communication with a galvanometer, and the galvanometer is connected with the battery. The strip of the substance to be tested is put under a small initial pressure, which deflects the galvanometer needle a few degrees from the neutral point. When the needle comes to rest its position is noted. The slightest subsequent expansion or contraction of the strip will be

indicated by the movement of the galvanometer needle. A thin strip of hard rubber, placed in the instrument, exhibits extreme sensitiveness, being expanded by heat from the hand, so as to move through several degrees the needle of a very ordinary galvanometer, which is not affected in the slightest degree by a thermopile facing and near a red-hot iron. The hand, in this experiment, is held a few inches from the rubber strip. A strip of mica is sensibly affected by the heat of the hand, and a strip of gelatin, placed in the instrument, is instantly expanded by moisture from a dampened piece of paper held two or three inches away.



For these experiments the instrument is arranged as in Fig. 2, but for more delicate operations it is connected with a Thomson's reflecting galvanometer, and the current is regulated by a Wheatstone's bridge and a rheostat, so that the resistance on both sides of the galvanometer is equal, and the light-pencil from the reflector falls on 0° of the scale. This arrangement is shown in Fig. 1, and the principle is illustrated by the diagram, Fig. 4. Here the galvanometer is at *g*, and the instrument which is at *i* is adjusted, say, for example, to ten ohms resistance. At *a*, *b*, and *c* the resistance is the same. An increase or



diminution of the pressure on the carbon button by an infinitesimal expansion or contraction of the substance under test is indicated on the scale of the galvanometer.

The carbon button may be compared to a valve, for when it is compressed in the slightest degree its electrical conductivity is increased, and when it is allowed to expand it partly loses its conducting power.

The heat from the hand held six or eight inches from a strip of vulcanite placed in the instrument—when arranged as last described—is sufficient to deflect the galvanometer mirror so as to throw the light-beam completely off the scale. A cold body placed near the vulcanite strip will carry the light-beam in the opposite direction.

Pressure that is inappreciable and undiscoverable by other means is distinctly indicated by this instrument.

Mr. Edison proposes to make application of the principle of this instrument to numberless purposes, among which are delicate thermometers, barometers, and hygrometers. He expects to indicate the heat of the stars and to weigh the light of the sun.

KEW GARDENS REPORT

SIR JOSEPH HOOKER'S Annual Report for 1877 is more than usually bulky, extending to fifty-three pages. The report opens with the number of visitors admitted to the gardens during the year, which amounted to 687,972, a great excess over those of the preceding year. The points of public interest first treated of by the Director are, as might have been supposed, those affecting the agitation to remove the boundary wall along the Richmond Road and to open the gardens at an earlier hour. These points have been so freely discussed of late in the public press that we need do no more than refer to them. Referring to his visit to North America, Sir Joseph pays a high tribute to the intelligence and courtesy of the people. He says: "I cannot adequately express my sense of the liberality with which travelling facilities and hospitalities of all kinds were accorded to me by public companies and private individuals wherever I went in America. The fact of my being connected with this establishment [Kew] was a recognised passport, and this even in the remote settlements of the Far West, for I found a reading people everywhere, few of whom had not heard of Kew Gardens. In the Northern States of America the progress of science, and of institutions for the instruction of the people in science, occupy a prominent place in the cheap illustrated periodical literature of the masses; and nowhere on the globe is this literature better or so universally read as in the States. It is hence not wonderful that the progress of such establishments as Kew, the British Museum, South Kensington Museum, &c., should be better known amongst all classes of the people there than they are in the United Kingdom generally, and so I found it."

Under the head of "Interchange of Plants and Seeds," as also under that of "Official Correspondence," a vast deal of information is gathered together on the acclimatisation, extended cultivation, or further development of useful plants. Thus we find the ipecacuanha (*Cephaëlis ipecacuanha*), a native of Brazil, has been distributed from the Calcutta Botanic Garden to Ceylon, Singapore, Burmah, and the Andamans. Dr. King, however, does not take a very hopeful view of its ultimate success in India, partly on account of its peculiarly slow growth, which tends to prevent its cultivation being taken up with spirit by European planters, and partly on account of its insignificant appearance, which does not even excite interest among the planters. Sir Joseph suggests it as "worthy of consideration whether the Indian government would not do well to establish a nursery in some part of our Indian possessions, with the tropical climatic conditions necessary for its growth." In connection with this subject, considering the great value of the ipecacuanha in cases of dysentery, it is satisfactory to note that there seems some prospect of its cultivation being attempted in the native states of Perak, where the soil and climate are considered to be well adapted to its requirements. Regarding the prophylactic virtues of *Eucalyptus globulus*—a subject which has been to some extent ventilated in our own columns—the experiences of the Kew authorities do not throw any further light on it. As valuable timber-trees, however, there can be no doubt that many of the species of *Eucalyptus* will prove most valuable. Sir Joseph says:—"The merits of the numerous species of

this genus are beginning to be understood, and there is no doubt that, where the climate is suitable, few timber-trees can compare with them for rapidity of growth and excellence of wood. Popular misconceptions still, however, prevail on the subject, to some extent. Attempts to grow in tropical climates the species of temperate ones such as *Eucalyptus globulus* can only result in disappointment."

Two points of interest are recorded in connection with the botanical origin of Shiraz and Havana tobaccos. Hitherto the former has been stated, even upon the best authorities, to have been furnished by *Nicotiana persica*. It has, however, been proved from plants raised at Kew from seed of the finest Shiraz tobacco procured from Persia, to be merely a form of *Nicotiana tabacum*, the plant so largely cultivated in North America, and from which the bulk of the commercial tobacco is procured. To the same plant *N. tabacum*, var. *macrophyllum*, is attributed the Cuba and Havana kinds, which have always been described as the produce of *N. repanda*, no such species, so far as Dr. Vidal, the director of the Botanical Gardens, Manila, is aware, being found now in Cuba, either wild or cultivated.

An interesting subject is that connected with vegetable poisons, more particularly the arrow poisons. It is, as is stated, a subject "well deserving of investigation, although it has been doubted whether the ingredients employed have any really poisonous qualities." The evidence on these points is conflicting. Some of the plants from which these poisons are obtained have been promised to Kew, so that before long we shall no doubt know something more definite about them.

Altogether the Kew report is, this year, one worthy not only of perusal, but to be retained for future reference.

NOTES

WITHIN a short period Vienna has lost two of her leading scientific celebrities, von Littrow and von Ettingshausen. We regret to add to the list the name of Baron Karl von Rokitansky, the President of the Vienna Academy of Sciences, who died on July 23 at the age of seventy-four. He was born at Königgrätz, in Bohemia, February 19, 1804. After completing his medical studies at the Universities of Prague and Vienna, he became, in 1828, assistant in the Pathological Anatomical Institute at Vienna. In 1834 he accepted the chair of pathological anatomy in the University, fulfilling, in addition, the duties of prosector in the Vienna hospital, and of legal anatomist for the city. In these varied functions an enormous mass of observations was accumulated, which served as a basis for his "Lehrbuch der pathologischen Anatomie," which appeared in five volumes, 1842-1846, and has survived three editions. At this period von Rokitansky commenced a remarkable series of investigations with the microscope, which Johannes Müller, a few years previously, had introduced into physiological research. From the results of these and other lines of investigation, he rapidly won for pathological anatomy an importance which had been hitherto wanting in German schools of medicine, and caused its recognition as the foundation for all research, not only in pathological physiology, but in the whole province of medicine. The old symptomatic system of classification was replaced by a careful discriminating study of the changes brought about in individual organs by the varied forms of disease. The delicate appliances of modern science enabled him to detect a large number of new diseases, which had hitherto been classed with other diseases on account of the apparent similarity in symptoms. To the classical researches of von Rokitansky, probably more than to any other source, modern diagnosis owes its perfection. The impulse given by him was ably seconded by the contemporary medical authorities of Austria, and on the basis of the principles formulated by von Rokitansky, Hebra in dermatology, Engel in general anatomy, Oppolzer in therapeutics, and Dittrich, Schuh, and Skoda in other special departments of medicine have grounded the famous so-called Vienna-Prague school. Von Rokitansky retired from his professorship three years ago, and published at the same time his last work, "Die Defekte der Scheidewände des Herzens." His merits won for him numerous marks of distinction, and for some time past he has presided over the Imperial Academy of Sciences. He leaves behind him a son, Professor of Music at the Vienna Conservatorium.

WE have received, from the Ethnological Museum of Leipzig, a circular, signed by Drs. Magnus and Pechuël-Loesche, of great importance in reference to the much-debated question as to the development of the sense of colour in men. The object of this circular is to obtain data as to what degree uncivilised peoples perceive colours and distinguish them by names, after the manner of civilised nations. The circular contains a series of inquiries in German and English, along with a scale of colours, and a schedule in which to record the replies to the various questions. The instructions are carefully drawn up, and Drs. Magnus and Loesche intend to distribute the circular by thousands; if the instructions are strictly adhered to the result must be of great value. We believe this circular is only the first of a long series which Dr. Pechuël-Loesche is preparing with the intention of enlarging, in a systematic way, our ethnological knowledge, and especially to solve interesting psychological problems regarding uncivilised tribes. The Ethnological Museum of Leipzig has agreed to receive and take care of all the material collected, which will be at the command of any scientific inquirers who may care to make use of it. We need say nothing in support of this enterprise of Drs. Magnus and Loesche; we would simply urge upon all our readers who are in a position to lend a helping hand, either directly or through friends living abroad, to obtain a supply of the circular referred to, by applying to Dr. Pechuël-Loesche, Museum für Völkerkunde, Leipzig, Germany.

IN the year 1851 the Vienna Academy of Sciences offered a prize for determination of the "Crystalline forms of products obtained in chemical laboratories," which proved fruitful, to a remarkable extent, in crystallographic researches of much importance. More recently new problems have come up with reference to crystallisation, partly in consequence of the labours just referred to, and partly because of the new discoveries of chemistry with regard to the structure of chemical molecules. Wishing to promote research in this direction, the Academy offer a prize of 1,000fl. for "Investigation of the crystalline form of chemical substances, with special reference to homologous series and isomeric groups. A determination of the specific gravity is also desired. The carrying out of optical researches on the crystals measured is deferred for measurement by the winner of the prize." The limit of time is December 31, 1879, and the prize will be awarded in 1880. Papers to be sent in with sealed envelopes and mottoes.

PROF. TAIT is, we hear, engaged in developing for fog-signalling purposes a form of apparatus producing intense sounds with great economy of driving power, which he some time ago exhibited at the Royal Society of Edinburgh. His mechanism is, we believe, devised to produce an effect analogous to that of the drum, which is one of the most economic of noise-producing instruments.

THE commission appointed by the French Government to test the rope used by M. Giffard in the construction of his captive balloon have made their experiments. We have already said that the rope is conical, the heaviest end being uppermost, so that, if any breakage should take place, it will not be in the vicinity of the car, but close to the

earth. The resistance of the smaller end has been found equal to 24,000 kilos of traction exerted by hydraulic pressure, and is smaller than anticipated. It had been suggested by Mr. Newall to employ a wire rope of his own make, which would have had a much greater resistance with a smaller weight; but the suggestion was lost, M. Giffard fearing some electric discharge might ignite the gas. The commission has given its authorisation to admit the public, but under the condition that the pressure should be limited to a quarter of the breaking-strain, viz., 8,000 kilos. The ascending-power is generally 5,000 kilos (about 12,000 lbs.). The difference left to bear the pressure of the wind will be about 5,000 lbs. for a balloon whose surface is $4 \times 1,170$ square yards. The breaking of the rope answers to a resistance of 50,000 lbs., or about ten lbs. per square foot of a plane; it can bear very high wind, and need fear only a tempest. Some observations have already been made by M. Tissandier, but in a somewhat rough manner. An anemometer will be constructed in the car, and its readings will be compared with the readings at the steelyard, to which the rope is attached.

ON July 26, the Council of the Paris Observatory held its quarterly meeting under the presidency of M. Dumas. Admiral Mouchez read a memoir on the most urgent reforms required for the better working of the institution. The new director insists upon the necessity of having a watch kept all night irrespective of the state of weather, for observations to be taken with the meridian circle. Such is the practice at the naval observatory of Montsouris, of which Admiral Mouchez continues to be the director. A large augmentation is required in the *personnel*, a supplementary sum of 54,000 fr. having been asked for that purpose. The Chinese ambassador visited the observatory on July 24, when a large mirror was almost instantly silvered by the Foucault process.

THE French *Journal Officiel* has published the names of those appointed by the Minister to act during three years as members of the Central Bureau of Meteorology. M. Hervé Mangon, and Admiral Mouchez have been appointed to represent the Academy of Sciences, M. Vicomte d'Arlot, sub-director of the Oriental and Chinese Department as delegate of the Foreign Office, Dr. Du Mesnil, Physician of the National Asylum of Vincennes, delegate of the Home Office; Commander Perrier, Member of the Bureau des Longitudes, to represent the War Office; M. Vice-Admiral Cloué, Director of the French Survey, representing the Ministry of Marine; M. Leon Lalanne, Engineer of the Ponts-et-Chaussées, representing the Department of Public Works; M. Cyrien Girard, Member of the Chamber of Deputies, representing the Ministry of Agriculture; M. Berthelot, Member of the Institute, representing the Ministry of Public Instruction; with General Farre, President of the Committee of Fortifications, M. Blavier, Telegraphic Engineer of the Government, to represent the French Postal Telegraphic Administration. The appointments of the officials to fill the several departments of the Central Bureau will soon be made public. It was decided that the Meteorological Bureau will leave the observatory at any cost. An hotel will be obtained for its use in the Rue de Grenelle, Saint Germain, in a part of Paris, which although not central, is at least thickly populated.

A COMMISSION, appointed by the U.S. Congress to select a new site for the Naval Observatory at Washington, held its first meeting on July 15. According to the *New York Tribune*, no money has been voted for expenses, and the Commissioners must therefore be in rather an awkward predicament. The old observatory, in which so much excellent work has been done, is in a very dilapidated condition, and we cannot believe that the U.S. Government will be so parsimonious as to allow the Washington Observatory to lose its place as one of the first institutions of the kind in the world.

AMONGST a large number of designs for the Spinoza monument at the Hague, only two were deemed suitable by the Committee; they were furnished by the sculptors, M. Fr. Hexamer, of Paris, and Herr Joseph Tüshaus, of Düsseldorf. The monument will be erected after the design of the former sculptor.

ON June 1 last, the hundredth anniversary of the foundation of the Society of Arts and Sciences at Batavia was celebrated by a large assembly of members and friends. This Society is the first learned European association which was formed upon Asiatic soil.

AT the Jardin d'Acclimatation at Paris, three enormous tortoises have recently arrived from the Seychelles. The largest of the three weighs no less than 187 kilogrammes (nearly 4 cwt.), and measures 1·17 metres in diameter (about 46 inches).

DR F. MOOK, who for some time past has been busily engaged in making excavations in different parts of Egypt, has just returned to Freiburg (Baden), with a large collection of antiquities. There is no doubt that his collection is the most complete in its special direction which has ever been brought to Europe. It contains no less than 340 skulls from the tombs of Thebes, Dendera, Abydos, and the Pyramid fields, all in the most perfect preservation. Besides these there are some eighty animal mummies, a large quantity of flint implements from Nubia and Egypt, vases, amulets, ornaments, &c. The collection is now exhibited in the University buildings at Freiburg.

WE have received from Messrs. Eberstein, of Dresden, a specimen of an interesting "walking-stick for naturalists or tourists." The stick is a perfect *multum in parvo*, and contains quite a museum of scientific instruments. The handle alone contains a compass, a double magnifying glass, or pocket microscope, and a whistle. Below it there is a thermometer on one side of the stick and a sand-glass on the other. The body of the stick is partly hollow, and in its interior holds a small bottle, which is intended to contain chloroform or ether for killing insects. Along the outside of the body there is a half-metre measure, showing decimetres and centimetres. Near the end of the stick a knife-blade may be opened, which serves for cutting off objects which cannot be reached by hand, such as aquatic plants, &c. At the extreme end a screw may hold in turn a spade (for botanists), a hammer (for geologists or mineralogists), a hatchet, or a strong spike, which would be of great use on glaciers. The whole is neatly finished in black polished wood.

THE following is the title of the essay to which the Howard Medal of the Statistical Society will be awarded in November, 1879:—"On the Improvements that have taken place in the Education of Children and Young Persons during the Eighteenth and Nineteenth Centuries." The essays to be sent in on or before June 30, 1879.

NEXT Sunday evening, August 4, from 6 to 8 P.M., is the last Sunday on which the Grosvenor Gallery will be open to the public this season. Tickets may be obtained by forwarding stamped envelope to the Hon. Sec., Sunday Society, 19, Charing Cross, S.W. More than 3,000 persons visited the Gallery on Sunday evening, July 21.

IN a paper in the last number of the *Journal* of the Statistical Society, on "Failures in England and Wales," it is pointed out, by reference to the tables in Prof. B. Stewart's papers in NATURE, vol. xvi. pp. 9, 26, and 45, that there seems to be some relation between the number of failures and sun-spot periods, just as there is between famines and the periodicity of the same solar phenomenon. Indeed, it is easy to see that this phenomenon must have a widespread influence, and in the interest of the most material commercial interests of our own and

other countries, effective measures ought to be taken for its thorough investigation.

THE British Medical Association meets at Bath from August 6 to 9.

NEAR Pombonne (France) the incisor of a mastodon has just been discovered in a sand-pit at a depth of about a metre. It measures 2·95 metres in length, its base is 45 centimetres in diameter, and the whole weighs some 250 kilogrammes. The ivory at the point is particularly well preserved.

THE General Meeting of the German Anthropological Society will take place at Kiel on August 12–14. Prof. Fraas, of Stuttgart, will speak on the drawing-up of a prehistoric map of Germany; Prof. Virchow, of Berlin, on the statistics of the shapes of skulls in Germany; and Prof. Schaaffhausen, of Bonn, on the compilation of a general catalogue of all the anthropological material in Germany.

THE Geologists' Association have arranged for an excursion to the Boulonnais on August 5, and five following days, which, judging from the admirable programme, promises both pleasure and profit to those who join it.

DR. SCHLIEMANN is at Constantinople, and intends resuming his excavations in the Troad if he can obtain from the Porte fifty soldiers as a guard against robbers. From Berlin it is stated that a summary account of the German excavations at Olympia says that the number of marble objects found during the last three winters is 904; of bronzes, 3,734; of terra cottas, 904; of inscriptions, 429; and of coins, 1,270. All the more important ruins have been photographed, and the third volume of the official account is about to appear. An exhibition of all the casts taken will shortly be opened at Berlin.

THE Jardin d'Acclimatation of Paris offers, during the present year, numerous opportunities for ethnological study. The latest arrival is a party of Guachos from the Pampas of South America, consisting of six men, three women, and a child. They are accompanied by a complete collection of the animals of the Argentine Republic, and by seventeen wild horses. The capture of the horses at full gallop with the lasso forms their chief exhibition.

A SECOND edition of Hooker's standard "Student's Flora of the British Islands," has just been published by Macmillan and Co.; several emendations have been introduced.

WE alluded recently to the remarkable record of earthquakes preserved through so many centuries in Japan. Mr. Hattori, of Tokio, has lately described an ingenious seismograph, which was invented by one Choko, 1,750 years ago. It consisted of a cylinder 8 feet in height, ornamented by various characters and designs. The upper part was encircled by a series of eight dragon's heads, in the open mouth of each of which, a copper ball was lightly balanced. The interior of the cylinder was occupied by a system of rods and springs, so delicately joined that the slightest trembling of the earth would serve to push a ball from a dragon's mouth. Immediately below each dragon's head was a frog looking upward, with his mouth likewise wide open, to receive the balls. The sound of the falling ball would call attention to the phenomenon, and the direction of the earthquake would be revealed by the particular ball dropped. This seismograph correctly recorded earthquakes, which were felt strongly at a distance, but were too feeble to be noticed by the senses in the immediate vicinity.

THE African traveller, Hildebrandt, recommends strongly, in the *Korrespondenzblatt der afrik. Gesellschaft*, the use of petroleum for those travelling in the tropics, as a protection against insects. Occasional applications to the face and hands ensured entire freedom from mosquitoes, and the same method sufficed to preserve horses and cattle against the deadly attack of the Dondorobo gadfly, which so often cripples the movements of the explorer. Petroleum, likewise, protected the natural history collections of the traveller from ants, moths, &c.

THE contributors to the *Zeitschrift für wissenschaftliche Zoologie* have just completed the issue of a supplemental volume of 634 pages, following on their thirtieth volume, as a testimonial offering to Carl Theodor von Siebold on the fiftieth anniversary of his doctorate, April 22, 1878. Prof. von Siebold was for long the chief conductor of this most important journal, having now associated with him as active editor Prof. Ehlers. The festival volume is remarkable for the number and eminence of the contributors, and the importance of their contributions. There are more than twenty authors, including Haeckel, R. Leuckart, Ehlers, Oscar Schmidt, von Thering, Forel, Stiela, Weismann, Simroth, F. Leydig, Salensky, Carl Vogt, Möbius, Repiachoff, and L. Graff. Leydig on the Amphipods and Isopods, Möbius on the movements of flying fish through the air, Haeckel on the phylogeny of the Echinoderms, Flögel on the brains of insects, may be mentioned as memoirs of the most valuable kind.

MESSRS. LONGMANS have sent us vol. iv. of "Ure's Dictionary of Arts, Manufactures, and Mines," edited by Mr. Robert Hunt, F.R.S. This volume is supplementary to the preceding three, and it is apparent that an earnest attempt has been made to record all additions, improvements, and new applications of value.

WE have received from Messrs. Maclure and Macdonald the first four parts of a beautifully and faithfully executed series of Portraits of Distinguished Men, among which are the portraits of several men of science—Sir Joseph Hooker, Prof. Owen, Dr. Allen Thomson. The future parts will contain other portraits of men well known in the scientific world. Each portrait is accompanied by a suitable notice, and the work as a whole deserves hearty encouragement.

THE additions to the Zoological Society's Gardens during the past week include two Macaque Monkeys (*Macacus cynomolgus*) from India, presented by Dr. Adcock; a Burrowing Owl (*Speohyto cunicularia*) from America, presented by Dr. Geo. E. P. Nixon; a Green-winged Dove (*Chalcophaps indica*) from India, presented by Capt. Otho N. Shaw; six Common Guillemots (*Uria troile*), British Isles, presented by Sir Hew. Dalrymple, Bart.; a Common Nightingale (*Daulias luscinia*), European, presented by Mr. Gee; five Great Bustards (*Obis tarda*), a European Bearded Vulture (*Gypaëtus barbatus*), a Spanish Imperial Eagle (*Aquila adalberti*), a Bonelli's Eagle (*Nisaëtus fasciatus*), a Booted Eagle (*Nisaetus pennatus*), two Lanner Falcons (*Falco lanarius*) from Southern Spain, a Red and Blue Macaw (*Ara macao*), a Blue and Yellow Macaw (*Ara ararauna*) from South America, deposited; an Orang-outang (*Simia satyrus*) from Borneo, a Coati (*Nasua nasica*) from South America, two American Flying Squirrels (*Sciuropterus volucella*) from North America, received in exchange.

THE MOVEMENTS OF FLYING FISH THROUGH THE AIR

THESE movements form the subject of an interesting paper recently contributed by Prof. Möbius to the *Zeitschrift für wissenschaftliche Zoologie* (Band xxx., suppl., p. 343; see *Naturforscher*, June 8, 1878). From his own observations (made during a voyage to Mauritius, *viâ* Suez, and back by the Seychelles) and the observations of others, he describes the principal features of the phenomenon thus:—

The exocœti dart with great velocity out of the water without regard to the direction of the wind and the course of the waves. They do not, during their flight, make any regular fluttering

movements with their pectoral and ventral fins, but hold them spread out. In the outspread fins there may occur very rapid vibrations.

The hinder part of the body remains somewhat lower than the fore part during flight.

Directly against the wind they commonly fly further than with the wind, or when their course and the direction of the wind form an angle together.

Most exocœti which fly against the wind or with the wind continue during their whole course of flight in the direction in which they come out of the water. Winds coming laterally upon the original course of the exocœti deflect these into their direction.

All exocœti which withdraw from ships fly during their whole course through the air, near the surface of the water.

When with strong winds they fly against the course of the waves, they commonly rise somewhat over each wave; sometimes their tail dips slightly in the top of the wave.

Only those exocœti whose air-course is crossed by a ship rise to considerable heights (at the most about five metres above the surface of the sea).

By day flying fish seldom light on the ship; they mostly do so at night, and never in calm, but only when the wind is blowing. They mostly fall upon ships which lie not higher than two to three metres above water, and when these are sailing on a wind (the wind coming obliquely from beyond) or with half a wind (the wind coming at right angles against the ship), and are sailing rapidly. Flying fish never come on board from the lee side, but always and only from the weather side.

Not uncommonly when their tail has dipped in the water they describe in the horizontal part of their course, a bow to the right or to the left side.

During wind and a rough sea they appear above the water more frequently than in calm weather.

Before ships, which come upon them in swimming, the exocœti escape into the air, just as they do before fishes of prey and cetacea.

Many authors have affirmed, in explaining the flying of fish, that the pectoral fins operate like the wings of birds, bats, and of insects. Prof. Möbius, however, shows that both the anatomical structures of the pectoral fins and their muscles, and the physiological relations of the position and size of the fins to the volume and weight of the whole body, are against flight-like movements of the pectoral fins.

The movements occasionally observed in these organs during flight are merely a vibration.

The true cause of these movements of fishes through the air are the spring-movements which they impart to their body by means of their very strong side muscles, just as other fish propel themselves powerfully through water. They spring out of the water with great velocity, because the air presents less resistance than the water, and when after some time, they fall back into the water, their outspread fins act like a parachute.

It is easy to understand how the action of the wind combines favourably or otherwise with their flight. By day the direction of their spring is so chosen that the disturbing ship is avoided. By night this orientation by the sense of sight is wanting, and the animals fall into the ship. As any air in strong motion, when it impinges against obstacles (a ship's side or waves), rises, it raises also the fish, so that this flies over the wave, or may come on board the ship. In short, as Prof. Möbius proves in detail, all the phenomena observed may be fully explained by the combined action of the oblique projection forwards and the wind. It may further be mentioned that the flying fish has a peculiar arrangement of the mouth, so that in this a portion of water may be carried during flight for the process of respiration.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

The Kingdom of Portugal has for its 4,700,000 inhabitants but a single university—that of Coimbra, which was originally founded at Lisbon in 1290. The university has a corps of instructors numbering 70, is attended by 1,100 students, possesses a library of 42,000 volumes, and is equipped with astronomical and meteorological observatories, as well as natural history collections. Advanced education is likewise provided for by polytechnics, medical academies, and industrial institutes in Lisbon and Oporto, and an agricultural school. The elementary schools of the land number 2,450, and parents lose their political rights if their children cannot read and write at the age of 15.

The University of Strasburg is attended at present by the largest number of students recorded since its establishment, viz., 710, consisting of 45 in theology, 195 in law, 150 in medicine, 177 in philosophy, and 143 in science.

The new regulations for medical study in France require a term of four years, five examinations, including one in physics, chemistry, and natural history, practical work in the laboratories and anatomical theatre, and two years visiting of the hospitals. The sum requisite for this course of study is 1,360 francs—520 for lectures, and the remainder for examination fees, thesis, and diploma.

An examination will begin at Merton College, Oxford, on Tuesday, October 15, for the purpose of electing to a Physical Science Postmastership of the annual value of 80*l.*, and tenable for five years from election. After two years of residence the College will raise, by a sum not exceeding 20*l.* per annum, the postmasterships of such postmasters as shall be recommended by the tutors for their character, industry, and ability. Candidates, if members of the University, must not have exceeded six terms of University standing. Information may be obtained from the tutor in physical science.

Mr. Samuel Sharpe has promised to give 5,000*l.* towards the building of the north wing of University College, London, so soon as the Council are prepared to begin the work. It is expected that this liberal donation, together with others which have been received, will enable the building to be very shortly commenced. A sum of 50,000*l.* in all will, however, be required to complete the extensions which are immediately contemplated.

SOCIETIES AND ACADEMIES

London

Anthropological Institute, June 25.—Mr. John Evans, D.C.L., F.R.S., president, in the chair.—Dr. Paul Topinard, of Paris, was elected an Honorary Member, and the election of the following gentlemen as Ordinary Members was announced:—The Rev. H. W. Watkins, Warden of St. Augustine's College, Canterbury; Hy. Wm. Jackson, M.R.C.S., F.R.A.S., F.G.S., of Lewisham, and Dr. Dunkley, of New Zealand.—A paper was read on the ethnology of the islands of the Pacific, by the Rev. S. J. Whitmee. This paper was chiefly intended to explain an ethnographic chart of the Pacific, coloured according to the author's own observations, and which, in the main, followed the divisions of races in previous charts. In speaking of the people, he said the Melanesians, or black race, might be regarded as the aboriginal people, and that they had affinities, more or less remote, with the blacks found in the various parts of the southern hemisphere. Probably these Melanesians once extended further across the Pacific than they now do. The brown Malayo-Polynesian race had, doubtless, entered Polynesia from the west. The difficulties of such a migration were not insuperable. An example was given of a comparatively recent arrival of a vessel thought to be Chinese or Japanese, at Fotuna, or Home Island, containing forty people. There is a third people in Polynesia differing considerably from both of the others. These are the Micronesians. They probably are primarily from the Philippines, or some other portion of the Indian Archipelago, but are mixed with Melanesian and Malayo-Polynesian blood. There is also reason to believe they have had an admixture of Chinese or Japanese blood derived from the occupants of junks which have been driven by adverse winds to this region.—Mr. Worthington G. Smith read a paper descriptive of palæolithic implements from the gravels of N.E. London, and a paper was communicated by Mr. G. M. Atkinson on a new method of finding the cephalic index.

Entomological Society, July 3.—H. W. Bates, F.L.S., F.Z.S., president, in the chair.—Mr. Basil G. Nevinson was elected an Ordinary Member and Mr. John A. Finzi a Subscriber.—M. Pascoe exhibited a number of insects he had collected during a recent tour through Algeria and the south of Spain; with these there was a remarkable myriopod having the cylindrical body of the Julidæ, but with only one pair of legs to each somite.—Mr. Boyd drew attention to the food plant of *Eluchista cerusella*. This insect had always been considered to feed on the leaves of *Arundo phragmites*, which Mr. Boyd doubted, as he had lately found the larva feeding on *Phalaris arundinacea*,

a grass which somewhat resembled the other plant before the flowers appear.—Mr. Distant exhibited some specimens of the homopteron *Ricania australis*, Walk., which had been sent him for identification through Dr. Sharp, from Mr. Lawson, of Auckland, New Zealand, where the species had been observed last year on the dahlia for the first time. These New Zealand forms were, however, much darker in colour than Australian specimens, and hence had probably been introduced for some time. Mr. Jenner Weir exhibited two specimens of *Leucania turca* with several pollinia of *Habenaria bifolia* attached to the trunk of each, and which was only observed in these two instances out of fifty specimens examined. Mr. Weir also exhibited an interesting variety of *Hipparchia hyperanthus*.—Prof. Westwood remarked on a recent note in NATURE, vol. xviii. p. 226, referring to observations made by Dr. A. S. Packard on the manner in which lepidoptera escape from their cocoons, and stated that similar observations had been previously made and recorded by Capt. Hutton (*Trans.* Ent. Soc., 1st. ser. vol. v. p. 85). Prof. Westwood also stated that he had recently heard of injuries done to potato crops by *Cetonia aurata*, which had been found stripping the leaves, and a lepidopterous larva (probably a species of *Botys*), which bored into the stem.—Mr. Dunning read a note on spiders resembling flowers.—The Secretary read a note from Mr. J. Haselden relating to the habits of the honey bee (*Apis fasciata?*) in Egypt.—Mr. Waterhouse communicated a paper on new coleoptera from Australia and Tasmania in the collection of the British Museum.

CAMBRIDGE

Philosophical Society, May 20.—A communication was made by the Rev. E. Hill, on some points connected with the influence of geological changes on the earth's axis of rotation.—The author proved by elementary methods the following results recently obtained by Mr. G. H. Darwin :—(1) That small deformations of the earth cannot alter the position in space of the earth's axis of rotation. For if external forces be neglected this follows immediately from the conservation of angular moments. If we introduce the attractions of the sun and moon, the investigation of precession and nutation will in general still apply to the deformed earth, and the mean obliquity be unchanged. 2. That these deformations cannot sensibly separate the axis of figure from that of rotation. It was shown that as soon as a separation began, the rotation-pole would appear to trace out on the surface a cyclone with its base in the direction along which the figure-pole might be shifting; that the two would coincide about every 300 days, and the greatest divergence thus be infinitesimal. 3. That should the earth yield to strain, these poles would describe a spiral on the surface. This was only shown in a single case. 4. Expansion and contraction will be less effective in shifting the principal axes than transference of surface-matter. For expansion and contraction involve transference of matter from within outwards, or *vice versâ*, and the effect of increase of matter at one point of a radius would be more or less counterbalanced by its subtraction from another point. But transference of matter on the surface may be so arranged that the gain at one point is reinforced by the loss at the other.

MANCHESTER

Literary and Philosophical Society, December 4, 1877.—Transit of the shadow of Titan across the disc of Saturn, November 23, 1877, by Joseph Baxendell, F.R.A.S.

February 11, 1878.—Mr. Binney, F.R.S., sent a marine alga from the Isle of Man for identification. It was not in fruit, but was undoubtedly an unusually narrow fronded form of *Chondrus crispus* (Lamx.).—Mr. C. Bailey, in the absence of Mr. Hurst, read a paper by the latter on the best method of collecting and preserving plants for herbarium purposes, when gathered in tropical or subtropical countries.—Mr. J. Boyd exhibited slides of *Spongilla fluviatilis*, the fresh-water sponge, showing spicules.

February 25.—Results and deductions of rain-gauge observations made at Eccles, near Manchester, during the year 1877, by Thomas Mackereth, F.R.A.S., F.M.S.

March 5.—On the decomposition of ultramarine by carbonic acid, by Mr. S. Sugiura (Student in the Chemical Laboratory of the Owens College). Communicated by Prof. Roscoe, F.R.S.—On siliceous fossilisation, by J. B. Hannay, F.R.S.E., Assistant Lecturer on Chemistry in the Owens College. Communicated by Prof. Roscoe, F.R.S.

March 11.—On bryozoa, by Arthur Wm. Waters, F.G.S.

March 19.—On a remarkable flash of lightning, by B. St. J. B. Joule.—On a barometer, by Dr. J. P. Joule, F.R.S.—A comparison of the standard barometer of the Owens College Physical Laboratory with the working barometer, by Mr. Morisabro Hiraoka, Student of Owens College. Communicated by Prof. B. Stewart, LL.D., F.R.S.—On a new calorimeter, by J. B. Hannay, F.R.S.E., Assistant Lecturer on Chemistry, Owens College.

VIENNA

Imperial Academy of Sciences, March 14.—On processes of degeneration and regeneration in normal peripheric nerves, by Herr Mayer.—Embryology of ferns, by Herr Leitgeb.—New experiments in proof of Döppler's theory of tone and colour variation through motion, by Dr. Mach.—Researches on the origins and the functions of the accelerating nerves, by Dr. Stricker.—On a fluorescein-carbon acid, by Dr. Schreder.—On phenomena in the circulatory apparatus after temporary closure of the aorta (a contribution to physiology of the spinal cord), by Dr. Mayer.—On the salivary glands of *Eledone moschata*, by Dr. Dietl.—On a new geological inclosure in the region of the Carlsbad springs, by Prof. Hochstetter.—On the magnetic declination and inclination at Vienna, by Herr Liznar.—The daily and yearly course of temperature at Port Saïd and Suez, by Herr Kostlivz.

March 21.—On peculiar openings in the upper surface flower leaves of *Francisea macrantha*, Pohl, by Dr. Waldner.—On the electromotive force of metals in aqueous solutions of their sulphates, nitrates, and chlorides, by Dr. Streintz.—On the diffusion of carbonic acid through water and alcohol, by Dr. Stefan.

April 4.—The following, among other papers, were read:—On determination of electric resistance by the electrostatic method, by Herren Gruss and Biermann.—On the heat capacity of mixtures of methylic alcohol and water, by Herr Lecher.—Main features of the actinic theory of heat, by Herr Reschl.—The basaltic lava of the Eifel, by Herr Hussak.—On the organisation of the brain of invertebrates, by Dr. Dietl.—On the arrangement of the more recent tertiary formations of Upper Italy, by Dr. Fuchs.—On Canides from the diluvium, by Dr. Woldrich.

April 11.—New and rare fish of the Vienna museum, by Dr. Steindachner.—Two problems of the dynamical theory of gases, by Lieut. Schlemüller.—The products of the volcano Monte Ferru, by Prof. Doelter.—The geological formation of Attica, Bœotia, Locris, and Parnassus, by Dr. Bittner.—On great subterranean watercourses and reservoirs, and the purity and transparency of certain lakes, by Dr. Boué.—On peculiar properties of some astronomical instruments, by Herr Sterneck.

May 9.—The dolomite ridges of Southern Tyrol and Venetia, by Dr. v. Mojsisovics.—The reptiles and fishes of the Bohemian chalk formation, by Prof. Fric.—On the results of the meteorology of the present, by Herr Hann.—Fish fauna of the Magdalene stream, by Dr. Steindachner.—Nostocolonies in the thallus of the Anthoceroteæ, by Prof. Leitgeb.—On continuous acoustic rotations and their relation to the principle of surfaces, by Herr Haberditzl.—Comparative anatomy of the seeds of *Vicia* and *Ervum*, by Dr. Beck.—Experimental pathology of œdema of the lungs, by Dr. Mayer.—On the electrolysis of water, by Dr. Exner.—On the relative volumes of atoms, by Herr Wächter.—Development of Chætopoda, by Prof. Stossich.—Chemical composition of diastase and grape jelly, by Prof. Zulkowsky.—Interpolar electrotonus, by Dr. Fleischl.—On the internal friction of glycerine, by Herr Schöttner.

May 16.—On the colours which follow each other in Newton's ring system, by Prof. Rollett.—On azophenols, by Prof. Weselsky and Dr. Benedikt.—On the existence of man at the time of the loess formation, by Count Wurmbrand.—On the apparently secular variations of dry land, by Prof. Suess.

May 23.—On the course of spark-waves in the plane and in space, by Prof. Mach.—On the path of the Comet II. of 1873, by Herr Becka.—Influence of pressure and temperature on the spectra of vapours and gases, by Herr Ciamician.—Theory and application of electro-magnetic rotations, by Dr. Margules.—The laws of the individuality of planets of our solar system; attempt to found a general theory, by Herr Lehmann.—Stones

from the peninsula Chalcidice, by Herr Becke. On Berberin, by Dr. Weidel.

June 6.—The following, among other papers, were read:—Contributions to a knowledge of the colour-change of Cephalopoda, by Dr. Klemensiewicz.—On some problems of the mechanical theory of heat (continued), by Prof. Boltzmann.—On the cold-mixture of chloride of calcium and snow, by Dr. Hammerl.—On the gases arising from action of barium-oxide hydrate on albuminous substances, by Dr. Liebermann.—On the mica group (second part), by Herr Tschermak.

June 21.—The protoplasm of the pea (second part), by Prof. Tangl.—On development of hydrogen in the liver, and a method of production of butyric acid of fermentation, by Prof. Pribram.—On the specific viscosity of liquids, by Profs. Pribram and Handl.—Relations between electromotive force and chemical heat-tone, by Prof. Sekulie.—On the best method of showing details of the ethnography of a country with adequate accuracy and completeness in maps, by Prof. Boué.—On motion of electricity in space and Nobili's rings, by Prof. Ditscheiner.

July 4.—Map of the mountains of the moon, from personal observations in the years 1840–1874, by Dr. Schmidt, of Athens Observatory.—Fourth report from the Adria Commission, giving results of meteorological observations for 1871–73, and maritime observations for 1873.—Yearly periods of the insect fauna of Austria-Hungary, by Herr Fritsch.—Determination of the orbit of Comet V., 1874, by Dr. Gruss.—On the molecular size of indigo, by Prof. Lieben.—On heliotropic phenomena in the plant kingdom, by Prof. Wiesner.—On the friction of vapours, by Dr. Pulnj.

Rome

R. Accademia dei Lincei, May 5.—The following, among other papers, were read:—On fossil bones in the environs of Rome, by Sig. Ponzi.—On personal errors in observation of the duration of meridian passages of the solar diameter, by Sig. Respighi.—Catalogue of the mean declination of stars of the first to the sixth magnitudes, comprised between the parallels 20° and 64° N. lat. (first part), by the same.—Objections to the induction of Messrs. Humphrey and Abbot, and representation by means of a parabolic curve of the subaqueous velocity, by Sig. Fambri.—On the minute structure of the skin of reptiles, by Sig. Todaro.—Histological researches on the pigmental epithelium of the retina, by Dr. Angelucci.—On the serpentine of Verrayes in the Valle d'Aosta, by Sig. Cossa.—On the serpentine formation of the Pavian Apennines; report on memoir, by Sig. Taramelli.—Theory of the boraciferous soffioni of Tuscany, by Sig. Bechi.—Astronomical and physical observations on the axis of rotation and on the topography of the planet Mars, made at the Royal Observatory of Brera, in Milan, with the equatorial of Merx during the opposition of 1877, by Sig. Schiaparelli.

Paris

Academy of Sciences, July 22.—M. Fizeau in the chair.—The following among other papers were read:—On the theory of fermentation, by M. Pasteur. He takes objection to the unauthorised posthumous publication by M. Berthelot of some laboratory notes of Claude Bernard, written in October last, and which seem to be opposed to M. Pasteur's views. The notes were those of experiments made in order to test to the utmost those views, not a manifesto against them. M. Berthelot replied.—On the electro-chemical deposit of cobalt and nickel, by M. Becquerel. He points out the priority of his father's and his own experiments on the subject in 1862.—On the variation of the intensity of currents transmitted through mediocre contacts according to the pressure exerted on them, by M. Du Moncel. He has made various observations on this subject since 1856, and in 1875 noted in metallic filings, &c., properties on which Prof. Hughes' thermoscope microphone is based.—Velocity of propagation of excitations in the motor nerves of muscles of animal life in mammalian animals, by M. Chauveau. The average velocity in frogs was first measured and found about twenty-one metres per second. In the pneumogastric nerve of solipedes great differences were observed both in different parts of the nerve and in different animals. In one ass the velocity in the recurrent branch of the pneumogastric was 51 m., in the pneumogastric 68 m., and in the intermediate section 66·5 m. *The activity of conduction decreases from the origin to the termination of nerves.* In *post-mortem* experiments this law is reversed. If the pneumogastric is cut the conduction is retarded somewhat without reaching the figure for the terminal portion of the recurrent nerve. Operating on the middle portion of nerves, the velocity is about the same in animals placed in the same physiological conditions. It is about 65 m. per second, and may rise to 75 m. in strong animals of high breed, or go below 40 m. in common weak animals.—Currents observed in the Suez Canal and consequences resulting from them, by M. de Lesseps. Lake Timsah and the Bitter Lakes act as regulators. The prevalent north and north-west winds from May to October raise the mean level of water at Port Said and depress it at Suez; hence in summer a current, interrupted by the tides, from the Mediterranean to the Red Sea, and finally driving a good deal of water southwards. In winter the reverse occurs. It is estimated that 400,000,000 cubic metres of water are thus annually driven to and fro. This, with the tides, tends to annihilate the effects of evaporation, and aid the dissolution of the salt banks of the Bitter Lakes.—Note on a new earth of the cerium group, and remarks on a method of analysis of columbates, by Mr. Lawrence Smith (sealed packet deposited September 22, 1877).—On mosandrum, a new element, by the same. The new earth was obtained from samarskite; and he established that it differed from that of the yttria group, from oxide of cerium, from lanthanum, and from didymium. M. Soret has, with the spectroscope, confirmed the existence of the metal constituting the base of this new earth.—Discovery of the periodic comet Tempel at Florence, by M. Tempel.—On an apparatus for demonstrating simultaneously the law of recoil of a gun and the law of motion of a projectile, by M. Sebert. The instrument is called a velocimeter.—On the tension of vapour and the freezing point of saline solutions, by M. Raoult. With regard to power of diminishing the tension of vapour, or retarding the freezing point, the different anhydrous salts rank in nearly the same order. The power of producing the one or the other effect is generally greater the smaller the atomic weight.—On the presence of lead in sub-nitrate of bismuth, by MM. Chapuis and Linossier. A method of detection is described.—On a new hexavalent non-saturated hydrocarbon diallylene, C_6H_8, by M. Henry.—On the presence of lithium in the earths and thermal waters of the Solfatara of Pozzuoli, by M. Luca. It is there found in very small proportion in the state of sulphate.—On the peripheric temperature in febrile maladies, by M. Couty. In febrile affections developed normally the temperature increases in the peripheric parts more than in the central, and there is equalisation, or a tendency to this, in all parts of the body.—Relation between manifestations of ozone and turning movements of the atmosphere; observations in 1877, by M. Gully. The coloration of the paper seems to be always greatest to the north of a centre of depression.

CONTENTS

THURSDAY, AUGUST 8, 1878

THE JOURNAL OF PHYSIOLOGY

Journal of Physiology. Edited, with the co-operation in England of Prof. A. Gamgee, F.R.S., of Manchester, Prof. W. Rutherford, F.R.S., of Edinburgh, Prof. J. Burdon-Sanderson, F.R.S., of London; and in America of Prof. H. P. Bowditch, of Boston, Prof. H. M. Martin, of Baltimore, and Prof. H. C. Wood, of Philadelphia, by Michael Foster, M.D., F.R.S. (London: Macmillan and Co.)

THERE is perhaps no science which is making more rapid advances than that of physiology, and which is at the same time so interesting to general readers, as well as to those engaged in its special prosecution. A knowledge of the processes of life has such a close relation to individual health and happiness that it ought to be more or less taught to every child at school, and all thinking men must note its advances with interest. So swift is the progress of physiological science, that it has necessitated for this journal a mode of publication, now becoming common in Germany, but of which this is perhaps the first example in this country. Instead of appearing at regular intervals, the *Journal of Physiology* is published in numbers, which are issued at periods varying from two to three months, according to the supply of material sent in to the editors. From four to six numbers will form a volume of about 500 pages. The advantage of this mode of publication is that it prevents a discovery made by one man from being forestalled by another whose observations, although really made later in point of time, might sometimes obtain priority under the ordinary method of publication.

The title-page of the *Journal* shows that it is to some extent an international work, three American co-operating with three English professors, under the able editorship of Dr. Michael Foster. The first numbers contain contributions from the Continent of Europe as well as from Great Britain and America, one of the most interesting articles in them being contributed by a German, Prof. Kühne, of Heidelberg. The range of subjects is very wide, and includes papers on almost every function of the body—innervation, motion, circulation, respiration, and secretion. Some time ago an account was given in NATURE of Kühne's interesting discoveries regarding visual purple, that pigment in the eye which is so susceptible to the action of light. In his present paper he takes up the other pigments of the retina, which are either not affected at all, or only to a slight extent, by exposure to light. He has succeeded in discovering and isolating from a bird's retina no less than three distinct pigments of great stability, and he gives in one paper the mode of preparation, properties, and spectroscopic appearances of these substances. In the same paper he simply mentions the black pigment of the retina, which he regards as exceedingly stable, and little altered by light; but while the number of the *Journal* in which his paper is contained was still passing through the press he made the discovery that this pigment does not resist the action of light so perfectly as he at first supposed, and is slowly altered by exposure. This leads him to remark that "if one considers the extremely widespread occurrence in the animal kingdom of the black pigment of the eye and other similarly stable pigments, it is scarcely possible to repress the idea that these, in addition to visual purple, also represent visual excitants, or so-called visual substances, and are intended to be decomposed by light during life, and to yield those substances which stimulate chemically the terminal apparatus of the visual organ." He also calls attention to the remarkable circumstance that the pigments of a bird's retina he has discovered are so mixed with oil globules that the colours in the cones of the retina represent exactly half the spectral colours, viz., from red to yellowish green, so that with their complementary colours they yield all the colours of the spectrum. He also observed that the three pigments are most readily decomposed by blue light, less by green, and not at all by red. Comment is unnecessary on the importance of this paper in reference to vision.

In a preliminary note Mr. Gaskell contributes some interesting observations on the vaso-motor nerves of striated muscles. He had previously found that irritation of the motor nerve of a muscle dilated its vessels, and increased the flow of blood through it, at the same time that contraction was produced so that fresh supplies of nutriment and oxygen were supplied to the muscle by the blood at the same moment that it was stimulated to work. He has now shown that the same phenomena may be produced reflexly by irritating a sensory nerve, and that the dilatation of the vessels will occur, and the blood will flow more freely through the muscle even when it is prevented from moving by paralysing the motor nerves with curare. On then irritating a sensory nerve, the current of blood is increased as usual in the paralysed muscle, which would have contracted under ordinary circumstances, and thus proof is afforded that the vaso-dilating nerves are distinct from the motor nerves of the muscle.

Mr. Priestley gives a full account of the literature regarding the pulsations of the lymph-hearts in the frog, and he details a number of experiments which demonstrate several new facts, as well as confirm the observations of other physiologists.

In a joint paper, Dr. Gamgee and Mr. Priestley criticise Tarchanoff's statement that each vagus nerve can set in action the whole inhibitory apparatus contained in the heart, and that when this apparatus, whose function is to lessen or stop the cardiac beats, has been exhausted by irritation of one vagus no stimulation of the other can stop the cardiac pulsations. Their own experiments show that even when one vagus has been exhausted, irritation of the other will still stop the heart, and even when both are exhausted the inhibitory apparatus is still active, so that the pulsations of the heart may even then be arrested by galvanism applied to the venous sinus. They therefore conclude that the inhibitory apparatus in the heart is much less easily exhausted by stimulation than the vagi, and that it may still retain its power over the heart although both vagi are so exhausted that they will no longer convey to it a stimulus applied to them.

The question as to whether the apex of the frog's heart contains within itself ganglia which will keep up its rhythmical motion has lately been the subject of lively debate, but Dr. Bowditch brings forward a number of

experiments which seem to point strongly towards a negative answer.

An interesting paper on the respiration of the frog is contributed by Prof. Martin, whose observations strongly suggest a close relationship between the nervous centre which regulates respiration and that which regulates general reflex action, even if the two should not be identical. He discusses the question whether there be two independent, though closely related, nervous centres, one for inspiration and the other for expiration, or whether, as supposed by Budge, there is a single centre from which the muscles of inspiration and those of expiration may receive their innervation according to circumstances. Dr. Martin shows that this latter hypothesis does not hold good for the frog, and that in it there are really two distinct centres, one for inspiration, and one for expiration, each having its own stimulus, and generating its own nervous impulse, which can travel in them only to its own set of muscles quite independently of the resistance opposed to discharge from the other centre.

Those who are interested in the electro-motive properties of muscle will find in this journal an admirable report on this subject by Prof. Burdon Sanderson, in which he gives an account of Hermann's recent work in this department of animal electricity, along with such information both regarding modes of investigation and experimental results as greatly facilitate comprehension of the subject.

A most laborious and fatiguing series of experiments has been made by Mr. North on the effects of starvation with and without severe labour, and on the elimination of urea from the body. These experiments were made upon himself, and, in addition to the personal discomfort produced by a complete abstinence from food, he voluntarily underwent severe exercise upon the treadmill, for the purpose of ascertaining exactly the effect of labour upon the excretion of urea. Flint had found that in the case of Weston, the pedestrian, the excretion of urea was considerably increased during a long walk, and Mr. North's observations go to show that severe exercise does increase the elimination of urea, but the increase is very small, both when the person is fed upon ordinary diet, and when nitrogenous food is entirely withheld. The quantity of urea passed, however, depends largely on the condition of the body at the time, varying according to the greater or smaller reserve of nitrogenous material contained in it, and he thinks that Weston, before entering upon his walk, had accumulated a large reserve, from which the urea he excreted was derived.

The paralysis produced by potash salts when injected into the circulation, is usually ascribed to a special action upon the muscles and heart. Dr. Ringer and Mr. Murrell, however, from a number of experiments on the subject, have come to the conclusion that potash has no special affinity for muscle, but is a protoplasmic poison, having an equal affinity for all protoplasm, and destroying the tissues in the order of their vital endowments.

Mr. Langley has made a number of observations upon the salivary glands, and finds that Nussbaum's supposition that the disappearance of the black colouration produced by osmic acid from the sub-maxillary (?) gland after treatment with glycerine is not due to the removal of ferment from the gland, but to some other cause, and that furthermore an amylolytic ferment does not exist at all in the sub-maxillary gland of the rabbit. He finds that there is a marked difference between the cat and the dog in regard to the salivary secretion, the sympathetic secreting nerves having a different connection with the gland cells in the two animals, a difference which favours the paralysing action of atropia in the cat.

The secretion of sweat is now known to be, like that of saliva, directly under the control of the nervous system, and to be excited by secreting nerves, independently of alterations in the vessels which supply secreting glands. Dr. Ott and Mr. Field show that the nerve centres in connection with the sweat glands can be stimulated by the poison muscarine, and that a greater amount of carbonic acid than usual in the circulating blood will also excite functional activity, a fact which would tend to explain the greater tendency to sweat which people observe when they are shut up in a close room, a tendency which appears to be greater than can be readily accounted for by the warmth of the room alone.

These brief observations will give some idea of the variety of physiological subjects discussed in the *Journal of Physiology*, and we heartily congratulate the able editor and his co-operators on the importance and interest of the results set before us in the numbers which have already appeared. We have no doubt that in such competent hands this journal will continue to maintain its high character, and, while absolutely indispensable to all who desire to follow the progress of physiology, it will, we think, do much to diffuse a knowledge of that science amongst general readers.

A UNIVERSAL GEOGRAPHY

Stanford's Compendium of Geography and Travel, based on Hellwald's "Die Erde und ihre Völker." Africa: Edited and extended by Keith Johnston. *Central and South America:* Edited and extended by H. W. Bates. With Ethnological Appendices by A. H. Keane, B.A. Maps and Illustrations. (London: Stanford, 1878.)

HELLWALD'S "Die Erde und ihre Völker" is well known in Germany, and has achieved a great popularity. We doubt, however, if a simple translation of Hellwald's work would have been either fair or wise; for though it is written more brilliantly than German works usually are, and although Hellwald himself is a competent geographer, it has several drawbacks which we should have regarded as serious defects had they been permitted to stand in this English edition. For one thing, Hellwald is a violent Anglophobist, and he takes every opportunity of depreciating English travellers or ignoring them altogether. We therefore think it wise in the publisher of the English edition to take the German work simply as a basis on which to found an English work that shall fairly represent the present state of geographical knowledge. The method adopted by the publisher appears to us well adapted to attain the end in view. He has succeeded in obtaining the services of geographers having a special knowledge of the various divisions of the earth of which the several sections of the work treat. These editors, taking the translation of Hellwald as their raw material, go over it, correcting and extending as far as they deem necessary in order to

produce a work which comes up to their standard. Thus Mr. Keith Johnston has dealt with Africa, and Mr. Bates with Central and South America; of the future volumes, Europe will be edited by Prof. Ramsay, North America by Dr. Hayden, the chief of the U. S. Geological Survey, Asia by Col. Yule, and Australasia by Mr. A. R. Wallace. It must be admitted that no more competent men could be found for the parts allotted to them, and judging from the two volumes before us, the "Compendium of Geography and Travel" ought to take its place as a standard authority on geographical knowledge and geographical exploration.

The volume on Africa by Mr. Keith Johnston, who himself will shortly lead an expedition to that much-explored continent, contains a complete account of our knowledge of the "dark continent," up to the date of publication, including the recent discoveries of Mr. Stanley. After a general introduction, each of the principal regions of the continent, from the region of the Atlas southwards, is treated separately, in all its aspects —physical, geological, topographical, ethnological, and biological. It is evident that Mr. Johnston has added largely to the German original; indeed, his volume is two or three times the size of the section of Hellwald's work devoted to Africa. The result is a work which gives a full and satisfactory summary of our present knowledge of perhaps the most interesting continent of the globe. It would, however, be a mistake to imagine that the work is a dry geographical treatise; it reads more like a well-written narrative of travel, and besides its value to all interested in geography as a standard work of reference, it will be found genuinely interesting reading. Mr. Johnston's Notes on the distribution of rain in Africa, illustrated by a series of fourteen rain-charts, are of distinct scientific value. Mr. Keane's Appendix on the African Races is evidently the result of long and conscientious research; and while he possibly makes too much of language *per se* as a test of race, he is evidently master of his subject, and has gathered together in a clear and well-arranged form a mass of information of great ethnological value.

To many, perhaps, the second volume, on Central and South America, edited and to a considerable extent re-cast by Mr. Bates, will contain more of novelty than the first, treating, as it does, of a region less familiar to the public than Africa. Under the title of Central America the second volume includes not only the smaller states of the isthmus—Guatemala, Honduras, San Salvador, Nicaragua, Costa Rica, and British Honduras—but also Mexico proper, the whole forming a region probably upheaved by volcanic agency, and which seems to taper away gradually from north to south. The area of this large district of country exceeds more than five times that of Spain, and would seem to be sufficiently distinct both in a geographical and geological point of view from those broad continental expanses known as North and South America. The highlands of this district form a series of wonderful lofty table-lands, intersected by detached hilly portions and flanked by commanding volcanic peaks. In some places these table-lands rise in terraces one over the other. In others these will be suddenly interrupted by deep intervening valleys of very various forms, sometimes mere chinks, at other times fissures of variable breadth and upwards of a thousand feet in depth between whose steep rocky walls flow little streamlets. The great mountain-chains culminate in such giant volcanic peaks as Popocatepetl, which is nearly 18,000 feet in height.

In addition to the chapters describing the physical and natural features of this area, and a brief account of its former wondrous greatness, there are chapters on the present inhabitants, and copious information is given as to each of the States. Especially would we note the chapters relating to the population and government of Mexico.

The second division of Mr. Bates's volume is devoted to the West Indian Islands. This large group of islands lying east of Central and north of South America, includes Cuba, Jamaica, Hayti, and the Lesser Antilles. The condensation of this part is carried too far. These islands awaken many memories of the past, not, indeed, of a prehistoric past, like those that cling round Mexico, but as it were of a modern past, with which some of ou own island glory is connected, and it would have been well had the editor not only edited, but extended, from the English point of view, Hellwald's notices of Jamaica, Cuba, and Hayti. In an appendix it is true there is a most useful tabulated survey of the principal islands in this group, which gives details of their population, a list of their chief towns, and a short account of the products and industries of each, but what we would have liked would have been to have had all this incorporated in the text, with a short account of the past greatness if any of each of the larger islands.

The third division treats of South America, a well-defined continent, over some portions of which our editor has often wandered, a continent, the greatest in the world for some of its natural wonders, a continent conspicuous for its mighty mountain ranges, for the peculiar way in which these run, which fact in combination with their great height and their vast woody slopes, accounts for their giving birth to so many gushing streamlets which, in their turn uniting, form so many mighty rivers, by which the future greatness of this part of the world will be achieved. The carefully edited chapters of this section read—though not exaggerated in tone—like so many pages from some tale of fairyland. Passes over mountains upon the snow—just on the very line of eternal whiteness with bright flowers and brighter humming-birds, views from these lofty eminences that no words can describe, views of nature in its vastness and its greatness that seem to pain the human soul because it has to confess its inability to take them wholly in. Then the vast steppes or llanos, then those rivers, such as the Amazon and her tributaries, and lastly the volcanoes. Amid all this nature the great towns and the varied peoples of South America are, however, not overlooked, and there are some good woodcuts illustrating the chief features of both scattered through this portion of the volume.

The chapters also on the natural products and resources of the various tribes and people are most interesting, and the statistics seem to prove that the leaven of civilisation is at last beginning to work in the huge human mass.

The chapter on the ethnography and philology of the American continent, by Mr. Keane, covers 100 pages, and seems all but exhaustive; it is accompanied by several maps, and, as in the case of Africa, by a long list in

alphabetical order of all the known American tribes and their languages. Each volume has a good useful index, a most important item in a work of this nature.

The abundant equipment of maps adds greatly to the value of the volumes, as the numerous illustrations do to their interest.

OUR BOOK SHELF

Geometry in Modern Life, being the Substance of Two Lectures on Useful Geometry, given before the Literary Society at Eton. By J. Scott Russell, F.R.S. (Eton: Williams and Son, 1878.)

IN a recent number (NATURE, vol. xviii. p. 263) we took occasion to suggest that the usefulness of a school scientific society might still further be increased by calling in the assistance of scientific men to deliver lectures which should be open not merely to the members, but also to a wider circle. The literary Society at Eton has, we believe, adopted this plan on very many occasions; recently it will be remembered that Mr. Gladstone addressed the society on Homer. Mr. Russell's lecture is a full one, and on the lines which it follows, a useful one. "Geometry is a pure science, gives logical training, is a discipline of thought, is an instrument of human culture, and has high educational value. But geometry is equally the development of a method pervading nature; its mastery gives man a power to govern matter. The training which enables him to comprehend the mechanism of the universe, enables him also to make creations of his own in harmony with those greater designs of which his own are but a small portion. These two uses of geometric education the one purely gymnastic, the other practical and technic, may be so combined that each shall aid and not impede the other. The order, number, and measure which pervade the universe can be easily brought within the scope of elementary education, and so form the fit preparation for scientific observation and experiment in later life, by means of which the standard of application of abstract truths to matter and events in human life are determined and made familiar. But the one learning cannot be too soon begun, nor the other too long continued, and each is a material aid to the other." This extract shows the author's views, which he has worked out in some detail. Starting from the Greek geometry, he passes on to useful geometry: its applications to land-measuring, trigonometry, navigation. He touches also on numbers, goes on to symmetry, harmony, melody, then to light, shape, and shadow. He closes with a chapter on matter, force, and motion. To sum up, the whole furnishes a quantity of illustration from an eminent practical man, which is likely to be profitable to teachers in search of such illustration—to allure the "what's the use of it?" boys who form a part of every mathematical master's geometrical classes.

Die Geologie der Gegenwart. Dargestellt und beleuchtet von Bernhard von Cotta. Fünfte umgearbeitete Auflage. (Leipzic: J. J. Weber, 1878.)

THE appearance of a fifth edition of von Cotta's well-known work is a sufficient proof of its popularity—a popularity which, in spite of some unfortunate drawbacks to its usefulness, we cannot but regard as being well deserved. Since the first appearance of the volume in 1866 it has been steadily growing in bulk, and in the present edition the author has brought his work up to date by noticing the principal contributions which have recently been made to geological science. Among such additions we may point to his notices of the method of study of rocks by the means of the microscope, of the new classification and nomenclature of the stratified rocks suggested by Carl Mayer, of the results of the *Challenger* expedition, of the latest speculations on the causes of volcanic activity and the nature of meteorites, and of Croll's theory of the recurrence of glacial periods. The coloured frontispiece now added to the work, we can scarcely regard as an improvement, seeing that it tends to perpetuate those views of the restriction of certain classes of volcanic products to distinct geological periods, which, though so frequently insisted upon by German petrographers, do not appear to be sustained by extended observation in the field.

Ocean and Her Rulers. By Alfred Elwes. New and Revised Edition. (London: Griffith and Farran, 1878.)

Under the Red Ensign. By Thomas Gray. (London: Simpkin, Marshall, and Co., 1878).

THESE are two good books, each in its way. The former is a narrative of the nations which have from the earliest ages had dominion over the sea, comprising a brief history of navigation down to the present time. It is evidently intended for boys and is likely to interest the more thoughtful of them and send them to works which will give a more detailed account of the peoples whose exploits by sea are told, and lead them to take an interest in geographical discovery. The reading is rather miscellaneous and unconnected, and the information sometimes undigested, but as a whole the book is useful and interesting.

Mr. Gray's booklet is one that will prove thoroughly useful to parents intending to send their boys to sea, as well as to the boys themselves. Mr. Gray knows well what he writes about, and the information and advice he gives as to the choice of a sea-life as a calling, how to get a boy launched into it, what kind of ship to choose, how the boy should conduct himself, what books he should read, and a multitude of other points are admirable. We are glad to see that among the books he recommends a large proportion are standard scientific works.

Memoir of the late Alfred Smee, F.R.S., by his Daughter. With a Selection from his Miscellaneous Writings. (London: George Bell and Sons, 1878.)

MR. SMEE was in many respects a remarkable man, and this readable memoir by his daughter will, we doubt not, be acceptable to those who knew him personally or through his works. An Appendix contains about forty papers, letters, pamphlets, &c.; these occupy quite two-thirds of the volume.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

An Intra-Mercurial Planet

WITH reference to the important announcement, by telegram, of the discovery by Mr. Watson of an intra-Mercurial planet during the late eclipse of the sun, it may be worth remarking that the position of θ Cancri agrees very well with that given in the telegram published in NATURE, and that there may be a possibility that the object observed is in reality this star. The position of the suspected planet recorded by Mr. Watson is R.A. 8h. 26m., and N.P.D. 72°; the apparent place of the star, computed from the mean place given in the new Nine-Year Catalogue for 1872, January 1, is, for July 29, R.A. 8h. 24m. 40s., and N.P.D. 71° 29′ 40″. The magnitude of this star is, however, smaller than that given by Mr. Watson, that in the British Association Catalogue being 5½, and that in Argelander's

Uranometria Nova 6. This discrepancy may very easily occur in the hurry of such a sensational observation, as on these occasions the time at the disposal of the observer is so limited.

Royal Observatory, Greenwich, August 3 G. B. AIRY

Floating Magnets

I HAVE no intention of discussing the beautiful experiments of Prof. A. M. Meyer on floating magnets; but as a privately-expressed opinion of mine has appeared in NATURE, vol. xviii. p. 260, I feel bound to defend it. The mutual repulsion of the vertical floating magnets varies nearly inversely as the fourth power of the distance at great distances, and nearly inversely as the square at small distances. The horizontal attraction of the magnet, held vertically over the water, varies nearly inversely as the fourth power at very great distances. At a certain moderate distance it reaches a maximum, and close to the centre it varies directly as the distance. It is easy to see that variations of the magnetisation of the lengths of the magnets, and of the distance of the large magnet from the surface of the water, may render configurations stable which would, under different conditions, be forms of unstable equilibrium. Prof. Meyer thinks that the configuration can never be stable. It is easy to see that it is a form of equilibrium, and in fact that any given size of hexagon will be brought into equilibrium by placing the large magnet at a suitable distance. It may, therefore, be in equilibrium when the floating magnets are on the circle of maximum attraction of the fixed magnet. But, in this case, the equilibrium is stable; for work would be expended in altering in any way the position of any one of the floating magnets. If this one is carried away from the others they repel it less, and it will be brought back; if it is carried nearer to the others they repel it more, and again it will be brought back.

The nature of equilibrium where there are several degrees of freedom may be illustrated by considering a tract of country upon which water can run. The hollows are positions of stable equilibrium; the summits and passes (saddles) are positions of unstable equilibrium. Then, if any one speaks of the former as more or less stable (as Prof. Meyer does of stable configurations), he may be understood as having reference to the curvature of the hollow, or to its level, or to some vague and mixed characters. It is very easy to understand why the form should be difficult to produce or maintain. It is because the floating magnets are in this case at much greater distances from the centre than when they assume the form Hence, the potential energy of the former configuration is much greater than that of the latter. The reverse is the case with and , and still more so with and , and so with greater numbers of magnets.

C. S. PIERCE

Mons. A. Cavaillé-Coll on Musical Pitch, the French Diapason Normal, Scheibler's Tuning-Forks, &c.

IN the course of my researches on musical pitch, with the view of discovering the source of the discrepancy between Appunn's and Lissajous's measurement of the French diapason normal, I have had the good fortune to enter into correspondence with M. Aristide Cavaillé-Coll, the celebrated Parisian organ-builder, and in his long and obliging answers to my inquiries he has communicated some facts which I have thought it important, with his permission, to lay before the readers of NATURE, as far as possible in his own language.

Scheibler, and the Persistency of the Pitch of Tuning-forks.—M. Cavaillé-Coll had the advantage of personally knowing Heinrich Scheibler, silk manufacturer, of Crefeld, near Düsseldorf, who died November 20, 1837. Scheibler's experiments on tuning, with which I had long been acquainted, are the most important hitherto made; but I had feared that his wonderfully accurate tuning-fork tonometer was irrecoverably lost. I find that M. Cavaillé-Coll is fortunate enough to possess one, and Herr Amels, of Crefeld, another, that is, a series of fifty-six forks, proceeding by degrees of four beats in a second, from A 220 to A 440 double vibrations in a second, which last was adopted by the Stuttgart Conference in 1836 as the best normal pitch. This was chosen by Scheibler as his standard, because it was the mean of the Viennese grand pianos in his day. Of him M. Cavaillé-Coll says:—

"M. Scheibler n'était pas un savant, mais, en s'appuyant sur les expériences faites par Sauveur en 1701 pour la détermination d'un son fixe, il était arrivé par ses patientes recherches à créer, en 1834, un tonomètre différentiel de la plus rigoureuse exactitude et qui n'avait pas été fait avant lui."

Of the exactness with which Scheibler worked M. Cavaillé-Coll gives the following remarkable proof, which is at the same time a proof that tuning-forks will preserve their pitch for at least twenty-eight years; so that there is no reason to suppose that, when properly protected, they will not form a lasting record. This was a point on which I dwelt much in my letter to M. Cavaillé-Coll, because it has been often thought that they might vary considerably. See Zantedeschi (*Sitzb.* Vienna Acad. vol. xxv., year 1857, p. 172), whose conclusions I believe to be erroneously based. M. Cavaillé-Coll says, in his first letter (January 24, 1878):—

"En 1862, j'ai eu l'avantage d'assister aux expériences faites par M. Léon Foucault pour la détermination expérimentale de la vitesse de la lumière. Ce savant expérimentateur, que la mort a enlevé à la science en 1868, se servait, pour mouvoir son miroir tournant, d'un petit tambour mis en mouvement par une soufflerie et un régulateur de pression que je lui avais établis; laquelle turbine devait faire 400 tours à la seconde. Or avec cette vitesse, la turbine faisait entendre un son d'axe dont le nombre de vibrations correspondait au nombre de tours." In a subsequent letter (February 8, 1878) M. Cavaillé-Coll adds:—"M. Léon Foucault, bien qu'il fit construire ses instruments par les premiers constructeurs, était toujours obligé de les vérifier et de les rectifier lui-même pour arriver à la régularité de marche qu'il avait en vue d'obtenir."

"Pour mesurer la vitesse de la turbine, M. Léon Foucault avait imaginé un moyen nouveau que je vais essayer de décrire. D'abord une pendule de précision, construite par l'habile constructeur Froment, mettait en évidence une roue dentée de 400 dents, laquelle faisait un tour entier par seconde. Ensuite, la turbine était disposée de manière à réfléchir un rayon lumineux du miroir tournant sur les dents de la roue. Or la coincidence des rayons lumineux avec le passage des dents de la roue de la pendule permettait de reconnaître, à l'immobilité apparente des dents de cette roue, que la vitesse de la turbine était alors exactement de 400 tours par seconde." This description is necessary to understand the extreme delicacy of the test of Scheibler's work, which follows. "Un jour que j'assistais à une de ses observations, M. Léon Foucault me dit: 'Si nous avions un diapason exactement accordé de 400 vib. par seconde il devrait se trouver d'accord avec le son d'axe de la turbine? Sans rien dire à M. L. Foucault, je cherchai dans mon tonomètre de Scheibler un diapason de 400 vib., et l'ayant comparé avec le son d'axe de la turbine, je le trouvai si exact que je fus émerveillé de constater que par des moyens différents et à plus d'un quart de siècle de distance ces deux savants expérimentateurs avaient atteint avec la même perfection la détermination d'un son fixe donnant exactement 400 vib. par seconde. Cette circonstance est venue confirmer dans mon opinion que le tonomètre de H. Scheibler pouvait être regardé comme un instrument de la plus haute précision." M. Cavaillé-Coll concludes:—"Dans mon opinion le diapason conserve le même ton à la même température. Il n'y a que l'altération du métal lui-même qui puisse faire changer le ton; mais si l'on prend les soins nécessaires pour préserver les diapasons des influences climatériques, comme le faisait H. Scheibler, on peut être à peu près certain qu'ils conservent le même ton."

Improvements in the Siren, Bellows of Precision, Double-Action Counter.—M. Cavaillé-Coll was also personally acquainted with M. le Baron Cagniard de Latour, and was "initié à ses travaux." He calls him "un des plus savants acousticiens français du siècle présent," and says he is "sans contredit le véritable inventeur de la syrène;" adding, "la date de la création de ce merveilleux instrument, qui se trouve aujourd'hui dans tous les cabinets de physique d'Europe, remonte à l'année 1819;" and he complains that Helmholtz should have mentioned Seebeck's first, even on the score of simplicity of construction, as it was invented so long afterwards.

The difficulty of using the siren for the exact determination of pitch is ordinarily very great, so that observations made by it without proper precautions are, as a general rule, defective. The causes of error (besides imperfect workmanship) are—1. The difficulty of estimating with precision at what time the continually rising pitch of the siren note reaches the height of the continuous tone with which it is compared, precise equality of pitch (as in the example just given) being always extremely difficult to attain, and also to verify, except under the most favourable circumstances, and with the siren the circumstances are most unfavourable; 2. The difficulty of obtaining a blast under constant pressure to make the tone of the siren continuous; and 3. The difficulty of comparing the counter of the rotations of the siren's disk with the seconds counter. Now M. Cavaillé-Coll, as an experienced, ingenious, and scientific organ-builder, turned his attention in the first place to the second difficulty, which when overcome would obviate the first. It is clear that if the tone of the siren could be indefinitely sustained at the same precise pitch, it could be completely compared with another tone either by unison or by beats. In 1863 (*Comptes Rendus*, vol. lvi. pp.309-443) M. Cavaillé-Coll invented a "soufflerie de précision" for giving a constant blast, applicable not only to the siren but to many other scientific instruments. The complete bellows, such as he furnished for the physical laboratory of the Sorbonne, is expensive (about 80*l*.), but he has arranged "un petit modèle de soufflerie de précision pour des expériences d'acoustique, et que j'estime à 500 fr.," 20*l*. (not including the siren), inclosed in an oak case about 27½ inches long, 17¾ inches wide, and 32½ inches high, and therefore of most convenient dimensions for an experiment. "Cette soufflerie," he says in his letter of February 8, 1878, in answer to my inquiries, for the small model is not described in the *Comptes Rendus*, "est mise en jeu par une pédale en fer à la portée de l'opérateur. Au-dessus de ce bâti est un grand régulateur de pression communiquant avec un sommier de 13 notes sur lequel on peut monter toute espèce de tuyaux; de chaque côté du grand régulateur et communiquant avec lui, j'ai disposé deux petits régulateurs angulaires à poids curseurs, avec leurs sommiers sur lesquels on peut monter soit la syrène, soit deux tuyaux pour l'étude des battements. Sur le sommier du grand régulateur de 13 notes j'ai placé une série harmonique de tuyaux à bouche du ton de 8 pieds à partir du 3ème (*Ut* de 2 pieds), et composé de 13 tuyaux d'étain exactement accordés, donnant les sons 4, 5, 6, 7, 8, 9, 10, 11, 12, 13, 14, 15 et 16. Bien que les trois premiers tuyaux de la basse manquent, cette série des sons harmoniques naturels permet néanmoins de faire de bonnes expériences sur le *timbre*, et sur les *sons résultants*." I have given the description of this instrument at length, because it is evidently precisely what is wanted for acoustical experiments. The ordinary laboratory blowing apparatus I have found quite useless for experiments on pitch and beats. By means of these constant action bellows it is possible to maintain any tone on the siren for many minutes, and hence the two first difficulties are overcome. The third difficulty (of counting), not to mention insufficient accuracy of workmanship in the siren, still remained.

"En général," remarks M. Cavaillé-Coll in his letter of February 8, "l'exécution de ces appareils [les syrènes de commerce] laisse beaucoup à désirer. Quand j'ai voulu faire des observations exactes, j'ai dû faire retoucher l'appareil avec beaucoup de soin par un de mes employés; mais après avoir obtenu la régularité du mouvement de la syrène, j'ai rencontré une nouvelle difficulté pour marquer exactement la durée de l'observation au moyen d'une pendule à secondes. C'est alors que j'eus l'idée de compléter la syrène par un compteur à pointage, que me permet d'embrayer du même coup le compteur de la syrène et le compteur à secondes, de manière à bien préciser le point de départ et le point d'arrivée de l'observation. Je construisis à cet effet un petit appareil en bois que je conserve dans mon cabinet comme souvenir historique de mes essais, et qui fonctionnait fort régulièrement. Plus tard j'ai composé le dessin d'un appareil plus élégant, pouvant se fixer sur l'armature de la syrène, et j'en ai confié l'exécution à l'opticien qui avait établi ma syréne, espérant que cela pourrait lui donner l'idée d'en construire de semblables pour le commerce; mais ce petit travail fut mal construit, et ce n'est que longtemps après que j'ai pu faire exécuter sous mes yeux, dans mes ateliers, l'instrument que j'ai montré, et qui est encore unique en son genre."

By means of this complete regeneration of the siren, M. Cavaillé-Coll says, in his memoir in the *Comptes Rendus*, that he has been able to "faire des expériences qui ont duré plus de dix minutes, avec une telle exactitude, qu'en répétant plusieurs expériences les résultats n'ont jamais varié que de quelques vibrations sur 50,000 environ." In his letter he says:—"J'ai fait à cette époque (1858–9) avec mon appareil, plusieurs observations dont la moyenne concordait avec les nombres constatés au tonomètre de Scheibler."

Pitch of the French Diapason Normal, and how Lissajous determined it.—As regards its pitch, M. Cavaillé-Coll has carefully compared the French diapason normal with Scheibler's 440 double or 880 single vibrations, the accuracy of which, after the striking proof already given of Scheibler's exactness, admits of no question. He says, on January 24:—"J'ai trouvé alors, que notre diapason normal de 870 vib. simples, donnait exactement 871·75 vib. par seconde, d'où il résulte que notre diapason, au lieu de 435 vib. doubles, donnent 435·875 vib., soit près d'une vibration sonore en plus que le chiffre de 435 vib. assigné par le rapport de la commission."

This is a most important piece of information. Copies of the French normal are easily procured, but they almost all vary by some vibrations in ten seconds, even when costing 20 to 35 francs. I have given means of making forks of 440, 256, 512 vibrations, according to Scheibler, and hence also of the exact pitch of the French normal, to Messrs. Valentine and Carr, 76, Milton Street, Sheffield, successors to, and long workers with, Mr. Greaves, from whom physicists can be pretty sure of getting any pitch they like, within a few tenths of a vibration, for 3*s*. a fork, small size, but sounding 20 to 30 seconds. I mention the names of these workers because people do not generally know where to go for such work, and it is not safe to give orders second-hand through the music-sellers. For larger forks and greater accuracy, and of course much greater cost, perhaps Mr. Ladd, of Beak Street, would be the best person to consult, but he uses Koenig's pitch. Messrs. Valentine and Carr can also make large forks if required. Time must also be given. To make a fork with perfect accuracy is often two or three weeks' work, for after filing, the pitch rises, and the fork has to rest three days at least before it can be tried again. This was Scheibler's experience, fully confirmed by my own.

Now, my observations on Appunn's instrument, just finished, but not reduced, show that his numbers are in excess about one per cent., a little more or less. His tonometer, when in perfect condition, gave the pitch of Broadwood's copy of the French normal, presented by the French Commission in 1859, as 439 exactly. As Koenig's forks showed perfect intervals when measured by this tonometer, we may take it as almost, if not quite, exactly correct that the acceleration of the beats in that instrument is uniform throughout. This would show that Appunn's numbers should be reduced in the proportion of 435·875 to 439, in order to obtain Scheibler's pitch, which is probably as accurate as we can hope any measurement to go. But I have since found that Broadwood's copy was not quite accurate, and that the best approximate rule is to throw out 1 in 123 vibrations. Thus the fork of the Liceo Musicale of Bologna, sent officially to the Society of Arts in 1869, and measured "graphically" at Bologna as 443·89, but measured by me with Appunn's tonometer as 447·2, would be 443·6 by Scheibler's pitch, and this agrees with actual measurement by Scheibler's 440. Again, by Appunn's tonometer, Koenig's Ut_3 was 258·4, which, corrected as above, gives 256·3. Now the measurements of Koenig's Ut_3, by Prof. Alfred Mayer and Prof. McLeod, with their own special instruments, give the pitch nearly as 256·3, and this agrees with actual measurement by one of Scheibler's own forks given me by M. Cavaillé-Coll.

For some time I had vainly endeavoured to learn the method employed by M. Lissajous to determine the pitch of the diapason normal, which I regarded as of great importance in the history of practical music. I am indebted to M. Cavaillé-Coll for the following information (on February 8):—

"M. Lissajous s'est servi de la syrène de M. Cagniard de Latour, mise en jeu par ma soufflerie de précision, munie d'un régulateur de pression, pour déterminer le ton du diapason normal. Quant au compteur à secondes dont j'ai armé ma syrène, je ne pense pas que M. Lissajous en ait eu connaissance lors de la détermination du diapason normal, et c'est peut-être à cela qu'est dûe la petite erreur que j'ai constatée par mes expériences à la syrène et par comparaison avec le tonomètre de Scheibler." "Je n'ai pas assisté," he had written, on January 24; "aux expériences de M. Lissajous pour la détermination du ton normal, vu qu'à cette époque nous n'étions pas d'accord sur

l'abaissement du quart de ton qui a été fixé par la commission. Je voulais, avec quelque raison, je crois, fixer le ton du diapason à 888 vib. qui avait pour base l'*ut* de 32 pieds égal à 33 vib. par seconde, le *la* géometrique = à 880, et le *la* tempéré 888, ainsi que je l'ai expliqué dans la petite brochure, 'De la Détermination du Ton Normal ou du Diapason pour l'Accord des Instruments de Musique,'" published originally in *L'Ami de la Religion*, February 6, 1859, before the normal La was fixed. At the close of this paper M. Cavaillé-Coll says, in favour of 888 v. s., besides his present remarks, "Ce nombre, qui se trouve de 8 vibrations plus élévé que le *la* normal du congrès de Stuttgard et de 8 vibrations plus bas que le diapason de l'Opéra de Paris [en 1857] aurait, suivant nous, le mérite, s'il était adopté, de concilier les exigences de la science physique et les besoins de l'art musical." The peculiarity that C 264 gives a just A 440 = $\frac{5}{3}$ × 264, and a tempered A 444, has been productive of some confusion. The committee called together by the Society of Arts in 1859 recommended the Stuttgart pitch A 440, which they considered would give C 528, whereas on equal temperament it would give C 523¼. But they made C 528 their standard, which would give the tempered A 444, and the Society of Arts commissioned the late Mr. J. H. Griesbach to make them such a fork, for which he employed the instrument now in room Q of the South Kensington Museum, and to this he endeavoured to make an equally tempered A. His results in place of C 528, A 444, were, when reduced from Appunn's to Scheibler's standard, C 535 and A 446, which do not even agree with each other, for his C requires an A 450, and his A requires a C 530, both being rather sharper than was intended. In the organ of the cathedral of St. Denis M. Cavaillé-Coll measured the pitch as A 444·25, by means of the siren, but before the application of his bellows of precision. The Bolognese fork, already mentioned as being nearly A 444, was also measured at Bologna by the siren, but the result is not stated in the report preserved by the Society of Arts.

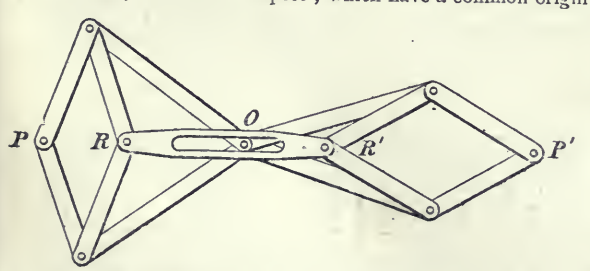
As regards the introduction of equal temperament into France, M. Cavaillé-Coll informs me that up to 1834 their house tuned on the old mean-tone principle, but that subsequently to 1834 he has consistently laboured to carry out the equal temperament. He thinks, however, that equal temperament was used for pianos before that date. I may mention that the change was made at Broadwood's, in London, between 1841 and 1846. That at the first Great Exhibition of 1851 in London, only one organ (by Schulze) used equal temperament, and that at least three organs had not adopted it a year ago (St. George's, Windsor, Turvey Abbey, and Norwich Cathedral).

Kensington, W., July 13 ALEXANDER J. ELLIS

Peaucellier Cell

THE following application of the Peaucellier Cell may possibly interest some of your readers. The object of this arrangement is to make two points—one on each side of a lens—move in such a way as always to remain at conjugate foci.

In the accompanying wood-cut P, R, and P′, R′, are the poles of two cells, alike in all respects, which have a common origin



at O; and the poles R and R′ are connected together by a bar with a slot in it, through which the pin which forms the pivot at O passes. Then if P, R, P′, R′, are constrained to keep in a straight line, P and P′ can approach or recede from O, only in such a way that, if there is a lens of proper focal length at O, P and P′ will always be conjugate foci.

This is easily proved thus:—

Suppose P O $= p$, P′ O $= r'$,
R O $= r$, R′ O $= p'$,
and the bar R R′ $= l$.

From the property of the cell,

$$r = \frac{k}{p} \quad r' = \frac{k}{p'}, \text{ where } k \text{ is a constant},$$

$$l = r + r' = k\left(\frac{1}{p} + \frac{1}{p'}\right)$$

$$\frac{1}{p} + \frac{1}{p'} = \frac{l}{k}.$$

Hence, if $\frac{k}{l}$ is the focal length of the lens, P and P′ are conjugate foci.

Mr. Francis Galton wanted to use the above arrangement, but found he could not get sufficient range unless the cells were made of unwieldy size. HORACE DARWIN

The Microphone

IN reproducing the experiments first made by Prof. Hughes with the microphone, I interposed in the circuit a galvanometer, and first found with the battery used (made with three small glass cones, as used by Prof. Hughes), when the microphone was not in the circuit, the current was sufficiently strong to deflect the needle to 40°. Now interposing the microphone, made of mercurised carbon peas in a small glass tube, it acted well as a transmitter only when the pressure on the carbon peas was so adjusted that the needle of the galvanometer stood about 15°.

When the pressure was very slight and the resistance to the current so great that the needle swung only to 5° or 8°, then the "continuous distant waterfall roar" of the telephone was plainly audible. The slightest sound of the voice in the room would produce the painful *pat*, *pat*, indicating an intermittent current and not a continuous one of varying intensity.

This "distant waterfall roar" emitted by the telephone, not unlike the "murmur of the sea-shell," was in all respects similar to the sound familiar to those who have attempted to use a telephone whose line was greatly affected by the induced currents of a number of proximate telegraph lines in active use. When the pressure of the carbon peas was so slight and consequent resistance great, the vibrations of the air in the room, when most quiet, so increased and diminished the resistance to the electric current as to cause the incessant tremor of the tympanic plate of the telephone, and thus rendered audible the constant murmur.

Among many other methods I tried a torsion pendulum, made by suspending, with a small cotton cord, a double cone of mercurised carbon an inch long, between two pieces of carbon less than an inch apart, to which the connecting wires were attached. The pressure was regulated by the torsion of the cord. In this simple manner any required delicacy was easily attainable.

Vanderbilt University, Nashville, Tenn., July 1 WM. LEROY BROUN

OF the many ingenious forms the microphone has taken—and I believe I am acquainted with most of them—none is, I think, more efficacious than the one I offer for your inspection. The jarring sound in the principal instruments in use, which, by vibration, may emanate from passing vehicles, &c., is entirely obviated, and the sound of a piece of fine silken thread, or the now well-known tramp of a fly, is heard with double the distinctness of any microphone I have listened to.

It consists simply of a cup and ball of carbon, the cup being fastened to a small piece of board, and one of the insulated wires attached to it in the usual manner, while the other is carried through the bottom of the cup sufficiently far to touch the ball without disturbing it in its socket.

From this little instrument I have obtained the most satisfactory results, and have heard distinctly that which I had to strain my hearing to catch before. Unless my "idea" is already anticipated, might I ask you to make it known to your numerous readers? GERALD B. FRANCIS

23, Bessboro' Gardens, S.W., July 24

A Simpler Form of the Phoneidoscope

MOST of your readers will be familiar with Messrs. Tisley and Spiller's beautiful instrument, known as the phoneidoscope. In using it, however, I have found certain defects, which my improvement on it is intended to obviate.

These are :—

1. That it is sometimes difficult to adjust the angle of the film so as to get the best light on it.

2. It is impossible to vary the distance of the film from the mouth so as to use both loud and faint sounds.

3. There is no means of adjusting the tension of the film.

My phoneidoscope, which is free from these defects, and which I have found to work exceedingly well, simply consists of the hand and some soap-suds. The forefinger and thumb being bent so as to form a circle, a soap film is drawn across them with the other hand. By turning the wrist, the angle which the film makes with the direction of the light can be accurately adjusted.

A motion of the elbow alters the distance from the film to the mouth, and by slightly separating or bringing together the finger and thumb, the tension of the film can be exactly regulated so as to give any degree of sensitiveness that may be desired.

The extra delicacy obtained by this adjustment much more than counterbalances the absence of the tube and mouthpiece.

Pixholme, July 30 J. E. H. GORDON

Spectrum of the Electric (Jablochkoff) Light

I WOULD suggest that when your readers visit Paris they should take their pocket spectroscopes. They will find a very interesting spectrum in the electric lamps now being used for lighting some of the principal public places in the city. One might have expected to have found from the brilliant spark inclosed in a white opaline globe a continuous spectrum such as is afforded by the voltaic arc. But the contrary is the case. The Jablochkoff candle now in use in Paris, even when viewed by one of Browning's small pocket instruments, presents a very complicated and highly interesting spectrum. I had no chart with me for comparison, nor did I, as I intended subsequently, make even a rough record of the spectrum; but speaking from memory, I may say that several lines in the blue and green were very marked and distinct, and, in fact, the whole spectrum was traversed by bright and dark lines. I thought, probably, some of these dark lines might be due to absorption by the white opaline glass globe, but I have tested several specimens of this white glass, and I find it does not alter in any way (except by generally reducing its brilliancy) a continuous spectrum, nor does it change the character of the solar spectrum. We must, then, turn to the light itself and to the atmosphere surrounding it for the cause of these phenomena. I believe that in a chemical sense there is no difference between the ordinary electric arc between the carbon points and the arc of the Jablochkoff candle, except that between the carbon points of the latter is a rod of kaolin, which has, I think, a calcium base. This kaolin is intensely heated by the current, and is volatised at the same rate as the carbon rods by the alternative current which this form of candle requires. The light, therefore, is a combination of the electric and the lime light, the current taking the place of the oxy-hydrogen elements. The surrounding atmosphere will be the same in both cases, but the products of combustion will obviously be different, and partly so from the composition of the kaolin. Still, I confess that I cannot suggest the cause of this complicated spectrum, and I hope that some observers who have more accurate means and more experience will give us the *rationale* of the phenomena.

I may say that there is not at present any Jablochkoff candles to be seen in use in this country, but in the course of two or three weeks they will be introduced into a large establishment, where excellent means of observation will be afforded.

Royston House, Tottenham, July 27 E. WALKER

P.S.—Since forwarding the above I have observed the Loutin light now on view at the Gaiety Theatre. The spectrum is somewhat similar to that of the Jablochkoff light, but much less distinct. This is probably owing to the circumstance that at the Gaiety the arc is inclosed in a small opaline globe, which is itself encased in an ordinary ground glass lanthorn (the proper lamps came to grief in transit), this diffusive ground glass causing, by overlapping, the indistinctness. Still there are absorption bands and some remarkable bright lines, which, with my small pocket instrument, I will not attempt to define. Nor, as it is a matter for careful observation, will I speculate further than to suggest—seeing that the Loutin light is from the carbon points only—that the white opaline glass may exercise a selective power over the spectrum given by this high state of incandescence which it does not in ordinary cases, and may give us also, to an extent, the actual wave due to a particular element rather than its obscuration. If so the Loutin light should differ somewhat from the Jablochkoff light, being deficient of the kaolin. E. W.

The Meteor Showers of July

THE prominent shower of *Aquarids* mentioned in my letter in NATURE, vol. xviii. p. 356, had become extremely feeble on July 31 and August 1, for of 136 shooting stars seen on those nights only three or four were conformable to that radiant point which, from a careful re-examination of all the paths recorded from it, is situated exactly at 341°—13°, near δ Aquarii (from fifty-four meteors).

Between July 26 and August 2 403 shooting stars were recorded here, of which no less than sixty-three (including one perfectly stationary) belonged to a very sharply-defined radiant near χ Persei, at 32° + 53°. Fourty-four of these were noted on the three nights, July 30-31 and August 1, when the shower appeared to attain its full intensity. The meteors were very swift with short paths (of about 7°), and almost invariably left streaks of 3° or 4°. They were shorter and less bright than the August *Perseids* at 43° + 58°, and in strong contrast to the long, slow meteors of *Aquarids* seen on the few preceding nights. This very rich stream at 32° + 53° has escaped previous detection, for, being near the date and position of the August *Perseids*, there can be no doubt that its meteors have in past years been attributed to that well-known shower, and given it an undue extension of period. The two radiants are, however, quite distinct, and it is now easy to explain the statements of some observers that there are many *Perseids* visible during the latter part of July. I believe that but few of the old *Perseids* can be seen before August 6 or 7. My own observations this year show that only seven or eight were seen before August 2, though I watched that region in which the radiant lies very carefully, and noted 400 meteors amongst the constellations there!

Thus at the end of July we may expect two special meteor showers: one of *Aquarids*, at 341°—13°, the other of *Perseids*, 32° + 53°. The former comes to a maximum two or three nights earlier than the latter, which may be called the "*Perseids* II.," in order to distinguish them from the old *Perseids* of Heis.

Ashleydown, Bristol, August 3 W. F. DENNING

The Rainfall of Brazil and the Sun-Spots

AN examination of the scanty records of rainfall obtainable in Brazil proves that the relation between rainfall and sun-spots, which has been pointed out in India by Dr. Hunter and others, holds good for the inter-tropical portion of Brazil.

The only stations from which I have been able to obtain records for a series of years are the city of Fortaleza (better known in Europe as Ceará), in latitude 3° 42′ S., and Rio de Janeiro, in latitude 23′ S. The rainfall of these two stations is shown in series in the following table :—

Series of Years in the Cycle of Eleven Years.	Average relative Annual Number of Sun-Spots, 1811-1875.	Mean Annual Rainfall of Ceará, 1849-1877.	Mean Annual Rainfall of Rio de Janeiro, 1851-1877.
		March.	June.
Minimum Group. 11th Series ...	16·3 } 12·6 mean.	1429·3 } 1342·2 mean.	1134 } 1032·6
Minimum Group. 1st & 2nd Series	10·8	1298·6	986
3rd and 4th Series	48·6	1493·8	1039·2
5th ,, 6th ,, ...	88·3	1587·7	1011·2
7th ,, 8th ,, ...	65·3	1608·1	1355·3
9th ,, 10th ,, ...	38·5	1252·6	1139·3
11th ,, ...	16·3	1429·3	1134

The northern provinces of Brazil outside of the Amazon valley, and notably that of Ceará, are subject to severe and prolonged droughts, of which that of 1877 is one of the most terrible on record. The annals of Ceará make mention of thirty years of drought since 1711, many of which, however, were only partial or slight, and many of which occurred in groups of consecutive years, there being one group of five dry years, another of four, and four groups of two years each. Twelve notable floods are also recorded since 1776. The droughts and floods are distributed as follows, among the groups of the years of the sun-spot cycle, proposed by Dr. Hunter :—

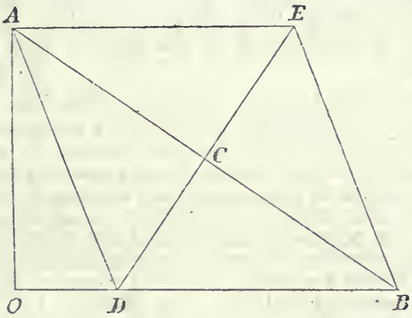
	Droughts.	Floods.
Minimum Group (11th, 1st, and 2nd Series)	13	2
Intermediate Group (3rd, 4th, 9th, and 10th Series) ...	10	4
Maximum Group (5th, 6th, 7th, and 8th Series) ...	7	6

Of the seventeen cycles of eleven years, between 1711 and 1877, only three have no record of one or more dry years. The great droughts were those of 1722, 1778, 1792, 1825, 1845, and 1877, of which four occurred in the minimum group, one commenced in that group, but culminated in the intermediate group, and one was confined to the latter group.

Rio de Janeiro, June 12 ORVILLE A. DERBY

The Cell of the Bee

THE following simple construction shows in one figure all the elements of a cell of a honey-comb. On two rectangular axes take OA and OB equal to the side and diagonal of a square. Join AB and bisect it in C. Draw CD normal to AB. Join AD, and complete the rhombus whose sides are AD, DB. Then if OA be a side of the hexagon, ADBE is one of the three equal planes forming the trihedral angle which closes the cell. The three short diagonals DE meet in the vertex of the cell, and are



normal to each other. The three long diagonals AB form an equilateral triangle. OD is the height of the vertex above the hexagonal face of the prism. AED is the angle which the axis of the prism makes with each of the diagonals DE. ADO is the angle which the axis of the prism makes with each of the edges of the trihedral angle. The diagonals DE and AB are in the ratio of the side and diagonal of a square. Such a cell contains a maximum volume with a minimum surface.

Bardsea EDWARD GEOGHEGAN

OUR ASTRONOMICAL COLUMN

THE REPORTED OBSERVATION OF "VULCAN."—In the telegram received from Mr. Lockyer relating to the solar eclipse which appeared in NATURE last week (p. 353), and which, like many other similar messages, had suffered in course of transmission, mention was made of Prof. Watson's observation of an object of $4\frac{1}{2}$ magnitude in R.A. 8h. 26m., and declination 18° 0′ north, which was neither a known planet nor a star. θ Cancri, a star of the fifth magnitude, is less than a degree from this position, but the observer would doubtless be aware of its presence. A telegram to the same purport was received by M. Mouchez, the director of the Observatory at Paris. By the formula deduced by Leverrier from the observations of suspicious objects in transit across the sun's disc, if the indeterminate k be put $= 0$, the elongation in longitude of his hypothetical body from the sun's centre at the time of totality at Prof. Watson's station would be 5°·9 eastward, and if $k = -1$, 9°·5 westward, neither of which, it will be seen, accords with the position given in the telegram. The fourth-magnitude star, δ Cancri, must have been within the limits of the coronal surroundings of the sun, and was only just beyond them during the total eclipse of July 28, 1851, when no observer, to our knowledge, remarked the star. In the instructions for observing the eclipse issued from the United States Naval Observatory, Washington, and prepared by Prof. Harkness at the instance of Admiral Rodgers, the superintendent, it is remarked: "As the truth of Leverrier's discovery of an apparently unexplained motion of the perihelion of Mercury is now established beyond all doubt, it is important to renew the search for an intra-Mercurial planet or planets." And to facilitate the work of such astronomers as might institute a search with considerable telescopic power, a chart was appended to the instructions showing every star so large as the seventh magnitude in that portion of the heavens occupied by the sun at the time. This chart extends from 7h. 32m. to 9h. 40m. in right ascension, and from 11° to 26° in declination. θ Cancri, the only star which appears near the position indicated for Prof. Watson's object, is marked on the chart as a sixth magnitude, which is the estimate of the *Uranometria* and *Durchmusterung*, but the star has been occasionally rated a fifth magnitude as in the first Radcliffe Catalogue, wherein particular attention was paid to the brightness of the stars. This is only a half-magnitude below Prof. Watson's estimate, but it remains to be seen from further intelligence whether there was any possibility of the star having been the object really noted; if it were separately remarked, or if the observed position does not admit of such change as would be necessary for identification, then it may truly be said that the American astronomer will have rendered the occasion of this eclipse a memorable one in the history of the science. Leverrier's confidence in the existence of an unexplained motion in the perihelion of Mercury and the necessity of accounting for it, by admitting the presence of matter in some form within the orbit of the planet, continued undiminished up to the time of his decease. One of his last communications to the writer of these lines was upon this subject.

THE LUNAR ECLIPSE ON AUGUST 12.—The eclipse of the moon on August 12 is the only one that will be wholly visible in this country until the year 1884; first contact with the earth's dark shadow at 10h. 42m., the middle of the eclipse at 12h. 8m., magnitude 0·59, and last contact at 13h. 34m. On October 4, 1884, there will be a total eclipse of the moon, the middle near 10 P.M., and the passage through the shadow nearly central.

THE AUGUST METEORS.—The earth will arrive at the descending node of the orbit of the comet 1862 III., in the track of which the meteors of the August period are found to move, soon after noon on Saturday next; the comet itself has now receded from the sun to a distance nearly equal to the mean distance of Neptune, to return to these parts of the system probably between 1980 and and 1985. Moonlight interferes this year with observation of the smaller meteors August 9-11, during a part of the night.

TEMPEL'S COMET.—The following places of this comet are deduced from M. Schulhof's elements, with the time of perihelion passage, corrected by the early observations at Strasburg by Prof. Winnecke:—

12h. G.M.T.	Right Ascension.	North Polar Distance.	Log. Distance from Earth.	Intensity of Light.
	h. m. s.	° ′		
August 23	16 20 30	109 3	9·9018	0·86
,, 25	— 25 55	109 52		
,, 27	— 31 28	110 40	9·9079	0·84
,, 29	— 37 12	111 27		
,, 31	— 43 6	112 13	9·9144	0·82
September 2	— 49 10	112 58		
,, 4	16 55 24	113 42	9·9213	0·80
,, 6	17 1 48	114 24		
,, 8	— 8 22	115 5	9·9286	0·77
,, 10	— 15 5	115 44		
,, 12	— 21 58	116 22	9·9365	0·74
,, 14	— 29 0	116 58		
,, 16	17 36 11	117 33	9·9448	0·71

On September 10 the comet passes very near to the orbit of Mars, but the planet is far distant. The dimen-

sions and eccentricity of the orbit of the comet are now :—

Perihelion distance ...	1·3393	Semi-axis major ...	2·9956
Aphelion ,, ...	4·6518	,, minor ...	2·4961

Eccentricity ... 0·55289

The interval between the perihelion passages in 1873 and 1878 is 1899·78 days.

BIOLOGICAL NOTES

THE PRIMARY GERM-LAYERS AND THE ORIGIN OF THE MALE AND FEMALE REPRODUCTIVE ELEMENTS.—Prof. Edouard Van Beneden, of Liége, three years ago observed that in the marine hydroid polyp Hydractinia the cells forming the testis and giving rise to spermatozoa, were derived from an ingrowth of the outer of the two primary cell-layers which form the foundation of all higher animal bodies, whilst the ova, he found, were simply cells of the inner primitive layer. This complementary function of the ectoderm and endoderm in Hydractinia led him to frame the hypothesis that throughout the animal kingdom the outer cell-layer is male in function, and the inner cell-layer female. On reviewing the facts known as to the derivation of the sexual cell-elements in various groups of animals, he was able to show a considerable amount of evidence in favour of the view that the testis is always ectodermal and the ovary always endodermal. Though accurate observations in this matter are excessively difficult, and definite knowledge as to the facts, in nearly all cases, is still wanting, yet Prof. Van Beneden's hypothesis was plausible and worthy of full consideration. It has been adopted by Gegenbaur in the last edition of his "Grundriss." The hydroid polyps consisting, as they do for the most part, of the two primary cell-layers in a very slightly differentiated condition, present the most ready field for the further testing of Van Beneden's hypothesis. The observations of Kleinenberg on Hydra were opposed to it. According to these *both* the sperm-cells and the egg-cells of Hydra develop from the ectoderm. Mr. J. Ciamician, of Vienna, has made a special study of this question in certain genera of hydroids (*Zeitsch. wiss. Zoologie*, 1878, part 4), and has published careful drawings in support of his statements. In *Tubularia mesembryanthemum*, assuming the accuracy of Mr. Ciamician's drawings, both female and male reproductive cells develop from a hollow in-growth of the ectoderm (the gonophors of the two sexes being distinct), which at first depresses the endoderm, but is afterwards itself flattened out by the up-growth of the endodermal layer of the spadix. In *Eudendrium ramosum* the ova develop from cells of the ectoderm, and the sperm-cells from cells of the endoderm, precisely the reverse of the relations detected by Van Beneden in Hydractinia. It seems hardly possible to interpret Mr. Ciamician's drawings of Eudendrium in any other sense than that which he himself adopts; the cell-layers at all stages are as clear in this species as they possibly can be. In the female gonophors of Hydractinia, Van Beneden saw an in-pushing of ectoderm at the apex developed in the same place as the in-pushing at the apex of the male gonophor, from which the sperm-cells developed. Van Beneden interpreted the rudimentary in-pushing in the female gonophor as a survival of a primitive hermaphrodite condition of the gonophors. Ciamician considers, on the contrary, that this ectodermal in-pushing is only the commencement of the formation of a medusa (in fact, the space between umbrella-margin and manubrium), which, instead of being completed, subsides into the condition of a medusoid gonophor. Hence, in place of a constant law of ectoderm being male and endoderm being female, we have in the three genera Tubularia, Hydractinia, and Eudendrium, the following variations respectively :—1, ectoderm male and female; 2, ectoderm male, endoderm female; 3, ectoderm female, endoderm male. The only possible generalisation from these facts is that of Ciamician, viz., that primitively the sexual functions are not assigned exclusively to cells of either layer: ectoderm may produce both male and female elements, and so may endoderm. With increased development and specialisation of structure, the production of reproductive elements would become limited to particular tracts of cells, and these would be necessarily *either* exclusively endoderm-cells or exclusively ectoderm-cells, but might be either one or the other indifferently even in closely-allied genera; and might be the same or complementary for the ovary and testis respectively. Nevertheless it must be admitted that though such indefiniteness in the relation of the sexual glands to the primitive cell-layers might be expected in Cœlentera where the differentiation of the two layers is at its commencement (both layers, for instance, developing nematocysts), yet in the higher groups of the animal kingdom we should be justified in looking for absolute constancy in the derivation of ovary and testis respectively from one or other (the same or diverse) of the two cell-layers—and we have not ground for supposing that this derivation would be the same in each of the great phyla until we can show that it is more constant in the Vermes than in the Cœlentera. E. R. L.

THE TOILET HABITS OF ANTS.—The Rev. H. C. McCook, of Philadelphia, eulogises the neatness of the agricultural ant, as observed in confinement at any rate. The most minute particles of dirt are carefully removed, and the whole body is frequently and thoroughly cleansed, especially after eating and sleeping. They assist each other in the general cleansing, and the attitude of the ant under operation is one of intense satisfaction, like that of a family dog being scratched, a perfect picture of muscular surrender and ease. Mr. McCook has seen an ant kneel down before another, and thrust forward the head under the face of the other, and lie motionless, expressing quite plainly the desire to be cleansed; the other ant understood this, and went to work. Sometimes this is combined with acrobatic feats, in which these ants excel, jumping about and clinging to blades of grass in a remarkable fashion. Sometimes the cleansing ant hangs downward from the grass, and to her the ant operated upon clings, reaching over and up with great agility to submit to her friend's offices. Evidently moisture from the mouth is used for washing. Mr. McCook has observed most minutely the whole of these processes, which are recorded in the Philadelphia Academy's *Proceedings* for this year. He suggests that with ants as with the human kind an artificial condition induces greater attention to personal appearance.

THE MODE OF RECOGNITION AMONG ANTS.—The combats and communications of ants are among the most interesting and mysterious phenomena. The Rev. H. C. McCook has given an account to the Academy of Natural Sciences at Philadelphia of some experiments he has made to determine what is the mode of recognition among ants. He has studied the pavement ants (*Tetramorium cæspitum*), which he has observed engaged in continued combat for over a fortnight, the warriors being only the workers or neuters. There is no distinguishable difference between the ants of the fighting parties, yet they recognise each other infallibly as friend or foe. They challenge all comers with their antennæ; if they are friends, they pass on; if foes, they straightway interlock and "fall to." Sometimes many ants are congregated against one, which is being torn limb from limb. Mr. McCook surmised that recognition was based upon a certain odour emitted by the respective factions. He found that if they were enveloped in an odour of eau-de-Cologne, while not at all deprived of activity, all became harmonious; those who were previously engaged in

battle unclasped one another, and they went on for several days amicably feeding, burrowing, and building. The same experiment was tried on the carpenter ants, which behead their enemies; their hostile proceedings were not stopped by eau-de-Cologne.

THE SNARE OF THE BASILICA SPIDER.—Science is under obligations to Mr. McCook of Philadelphia, for his study of the marvellous constructions of the Basilica spider (genus *Epeira*), near the Colorado River, Texas. It was first found about two feet from the ground upon a bush. The general form is pyramidal, the upper part of it a mass of straight lines knotted and looped, and crossing in all directions. Within this is suspended an open silk dome, of a vast number of radii crossed by regular concentrics. The dome was suspended from the upper erection so as to be perfectly steadied and kept in form. Beneath the dome was a light sheet of irregular cobweb. The spider itself is very beautifully coloured. This form appears to be a capital specimen of transition between the orb-weavers and the line-weavers. It has the characteristics of the line-weavers, namely, right lines and sheet-web in exact detail, and dome-shaped web in outline; it also has the geometric web of the orb-weavers, or radiating lines regularly crossed by concentrics. An allied species (*Epeira globosa*) is an orb-weaver, adding to the simple orb an open but distinct tube reaching almost to the centre of the web, with a free ray running along the floor of the tube, kept taut by the fore feet of the spider. An insect struggling in the web communicates the motion directly to the spider, which rushes along the covered gangway to its prey. Sometimes the gangway is imperfect, or even wholly omitted. The orb in the basilica spider appears to be the chief means of capture, the dome the dwelling-place, and the upper pyramid a suspension for this, and a protection against enemies.

SEXUAL CONDITIONS IN THE RED MAPLE.—It is commonly stated that maples bear hermaphrodite, male, and female flowers, but Mr. Thomas Meehan, of Philadelphia, asserts that the red maple is, according to his observations, really diœcious, having only flowers of one sex on the same tree. But the male and female flowers are similar in outward appearance at first opening, except that the small pistil is not developed in the male flowers. The female flowers have anthers of full size, and are supposed to be of both sexes, but the fact is that the anthers do not develop after the flower has opened, and shed no pollen. This is a very remarkable survival from a condition when the flowers were perfect. It is not uncommon to find trees, originally female, sending forth male branches, but Mr. Meehan has not found male trees produce female branches. The male flowers were found fragrant, the female not so.

GEOGRAPHICAL NOTES

UNDER the title of "Naturalists in the North West," a Sydney paper has recently published some interesting articles, the last of which relates to Mr. Miclucho-Maclay's account of the manners and customs of what he calls the Papuans of the Maclay coast. Their food consists mainly of vegetable products, but they have also some animal food, though it is somewhat scarce. Many of their domestic implements are of a very primitive nature; a flat splint of kangaroo bone forms a knife, of which a large kind is made from a smooth shell; axes are made sometimes of agate, and a few large ones, 3 inches wide, are kept as public property in each village. The dress of these natives is the *mal*, a piece of cloth prepared like the tapas of the Polynesians, from the bark of trees. The men all carry the *jambi* and the *gun*, to supply the want of pockets, the former being a bag suspended from the neck, and containing tobacco, &c., and the latter one woven of different coloured threads, and ornamented with shells. The *gun* is slung over the left shoulder, and contains the box of lime, betel-nut, knives, bamboo boxes of red and black dyes, &c. The natives also wear bracelets of bark or grass above the elbows, into which the *dougan* is thrust, and implements or weapons are also placed in the bangles on their legs. Wild boar's tusks are highly prized as manly ornaments to be worn on the chest, and ear-rings of tortoise-shell, bamboo, stones, or flowers, are all considered the proper adornment of the men. The women do not decorate themselves to the same extent, but they have cords from the upper part of one ear passing over the forehead to the other, and also bunches of dogs' teeth hanging from the lobes of the ears; they carry two bags, in one of which they place provisions and in the other their young infants or some pet pigs or puppies. Their huts and villages are situated in groups round clearings in the forest, and the plantations are usually at some distance. They have three sorts of houses—for the single people, the families, and a common house, principally used by the bachelors. These habitations do not resemble the pile-dwellings of the Western Papuans, and are only slightly raised above the ground. In each cluster of huts is a gong, like a boat raised on trestles, which, when struck in the right place, emits so great a volume of sound that it can be heard at a distance of six miles. It may be mentioned that these people have no means of obtaining fire, and frequently have to go to the hill tribes, who are acquainted with a cumbersome mode of friction by which they obtain a light.

GREAT exertions are being made by the Marquis de Croizier, and others, to ensure the success of the coming International Congress of Commercial Geography, which will be opened at Paris on September 23, under the presidency of M. Meurand, of the French Foreign Office. Numerous foreign societies have been invited to send representatives to the Congress, and we believe that the Royal Geographical Society will be represented by some members of their Council. The programme of the Congress is an extensive one, the numerous subjects proposed for discussion being arranged under the following five heads: Explorations et Voies Commerciales; Produits Naturels et Manufacturés; Emigration et Colonisation; Enseignement; and Questions Générales. A detailed programme and a *résumé* of the proceedings of the Congress will be published each day and forwarded to all the members. The meeting will be brought to a close on September 28.

AT a recent meeting of delegates of the German African Society at Berlin, it was resolved to grant the sum of 10,000 marks (500*l.*) to the International Association for the Exploration of Africa, and also to support Dr. Buchner, who is about to start for a tour through the districts lying south of the Congo River. The reports recently received from Herr Schütte, the engineer of the society, who is now at the Congo, continue to be favourable, and are accompanied by excellent maps of the districts he visits.

DR. OTTO FINSCH, the Director of the Natural History Museum of Bremen, will start for a scientific expedition to Australia at the end of this year; he is sent out by the Royal Academy of Sciences of Berlin, who will defray his expenses from their Humboldt fund.

THE ELASMOTHERIUM

AMONG the extinct animals of the diluvial age, few have left such scanty remains as the elasmotherium. At the beginning of the present century Fischer von Weldheim, when examining the palæontological collections of the University of Moscow, came across the half of the under-jawbone of an unknown animal, to which he assigned a place between the rhinoceros and elephant. The name elasmotherium was given to the new species,

on account of the peculiar appearance of the teeth, which seemed to consist of plates of enamel longitudinally folded. Later scattered teeth of this animal were found in Hungary, in Sicily, and in various Russian provinces. A few years since a complete under-jawbone was discovered at Petrowski; a fragment of the back part of a skull in the Museum of the Jardin des Plantes, at Paris, which was discovered on the banks of the Rhine in the last century, has likewise lately been identified as

FIG. 1.—Side view of the skull of the Elasmotherium

belonging to the elasmotherium. These remains were altogether too limited, to offer the zoologist any satisfactory clue to the general character of this animal. While the form and size of the jaw showed a strong resemblance to that of the rhinoceros, a close relationship was forbidden by the peculiar characteristics of the teeth.

Interesting as were the questions arising with regard to the nature and habits of this extinct resident of Europe, they have hitherto remained unanswered, until a fortunate discovery at the beginning of the present year, placed the zoologist in possession of a well-preserved skull of the elasmotherium. This object, certainly the most valuable of late palæontological discoveries, was found attached to the net of some fishermen in the river Volga, not far from its mouth, a district which has furnished many valuable remains of the extinct fauna of Russia. The St. Petersburg Academy of Sciences has become the fortunate recipient of the newly-found treasure, and to one of its members, Dr. Alexander Brandt, the scientific world owes the first complete summary of the deductions drawn from the study of this skull, as well as its detailed description. Besides his communication on the subject to the Academy, he has published a longer article in the Russian periodical *Niwa*, and translated it likewise into German.

The skull (Fig. 1) itself has the following dimensions. Length 33 inches, height, including the under-jaw, 21½ inches, breadth 16½ inches. Its most striking feature is an enormous bony protuberance on the brow. This is hemispherical in shape, possessing a circumference of over 3 feet, and projecting forward about 5 inches, and is hollow, forming a portion of the frontal cavity. Unusual developments of this cavity are noticeable in the skull of the ordinary cow, and more especially in those of the elephant and rhinoceros. As in the case of the latter animal, the protuberance of the skull of the elasmotherium presents a rough, uneven surface, traversed by deep furrows once occupied by blood vessels. The whole analogy with the rhinoceros points with the greatest certainty to the previous existence of a horn, which, to judge from the size of the blood-vessels once encircling the base, must have possessed enormous dimensions, and easily exceeded the length of the skull itself. The presence of a similar, rough protuberance of much smaller dimensions lower down towards the nostrils, would incline to the supposition that a second smaller horn was likewise present on the elasmotherium.

The front view of the skull bears a general resemblance to that of a horse or a ruminating animal. The rear portion of the skull, however, shows the relationship with the rhinoceros, and this relationship, at least, to the extinct rhinoceros, is still more strongly evidenced by the bony partition dividing the nasal cavity, a most peculiar and characteristic anatomical formation; for with the exception of these two animals, all other mammals known to us possess simply a cartilaginous division in this cavity. The structure of the teeth (Fig. 2) presents, on the contrary, no points of similarity with that of the rhinoceros. They are composed of winding folds of plates of enamel, extending the whole length of the tooth, and presenting, on the upper surface, an odd foliated appearance.

To judge from the skull in question the elasmotherium was most closely allied to the rhinoceros family, standing between it and the horse. Its proportions surpassed, however, those of any of its congeners, thus far known, existing or extinct. The proportions of the skull would point to a length of body ranging between 14 and 16 feet. With regard to the form of the body and limbs, nothing definite can be said. The nose was much narrower than that of the rhinoceros, while the eyes were larger, and the powers of vision of the elasmotherium, therefore, probably greater than those of the rhinoceros. Analogy with the contemporary rhinoceros and mammoth of

FIG. 2.—Grinding surface of tooth (natural size).

Siberia would warrant the supposition of the presence of a shaggy coat of hair.

The absolute, as well as the comparative size of the cranial cavity would assign to the elasmotherium a low degree of intelligence. Dr. Brandt pictures it as an

enormous animal, of great strength, but clumsy, awkward, and slow in its movements. It wallowed in the mud, lazily chewed its food of grass and tender twigs, and stretched itself to rest among the reeds or grass on the river's edge. From this apathetic condition it would be roused only by the attack of a rival, of a mammoth, of a rhinoceros, or of some one of the great carnivorous animals of that distant epoch. It would then rush enraged on its adversary, and endeavour to lay it low with its formidable horn.

The discoveries of remains of the elasmotherium show that it once wandered over the greater portion of Europe, from the Ural to the Rhine, and as far south as Sicily. It is also highly probable that later palæontological discoveries will show its existence in Asia, in company with the other large mammals, whose remains have been so well preserved in Siberia.

It was also in all probability contemporary with the men of the stone age, its remains occurring in the same deposits, in which the anthropologist finds the flints, collections of bones, and other evidences of prehistoric man. From their weapons the elasmotherium had probably little to fear.

In connection with this interesting discovery Dr. Brandt recounts a tradition of a tribe of Tartars in South Siberia, which describes the death of an enormous black ox. It possessed, however, but a single horn, and that of such size that it could be transported on sledges only. Possibly a reference to the elasmotherium.

UNDERGROUND MONSTERS

In a former number (vol. xvii. p. 325) we gave some account of a curious underground monster, the Minhocao, supposed to exist in Brazil. Dr. Spencer Baird, of the Smithsonian Institution, sends us an interesting document, which shows that the belief in such a monster is not confined to Brazil, but is shared in by the people of Nicaragua. In the *Gaceta de Nicaragua* for March 10, 1866, is a long letter signed "Paulino Montenegro," containing a circumstantial account of an object possessing very much the same attributes as the Minhocao. The letter is dated Jinotega, Nicaragua, February 21, 1866. The writer states that he went to Concordia on private business, when he heard on the 17th of a serpent having taken up its abode at a place called La Cuchilla, within the jurisdiction of the village. Along with some friends, M. Montenegro set out on the 18th to examine into the foundation of the report. A tradition concerning such a monster has existed from "time immemorial." After having travelled on that day about two leagues (1 league = 2·6 English miles) north-east from the village, they reached the spot where the inhabitants of the neighbourhood had traced signs and tracks, which, M. Montenegro states, positively prove the existence of such an animal.

The most detailed accounts stated that here, some five years before, a sort of platform of about fifty varas diameter had been formed at the foot of a large rock cropping out from a hillside. One of the neighbours had established there an orchard, though no one had been able to account for this new formation. Three years before, however, people began to observe that this little piece of level ground was gradually deepening, and that in the month of November the base of the rock adjoining it became exposed and worn from some agency, notwithstanding that there was not sufficient water to cause the phenomenon. At the same time mighty trees (*robles*[1]) were observed to become uprooted and to fall in great disorder, while immense rocks were moved and shifted their foundations so much, that in the following month of December, during one night, the road from Chichiguas and Cuchilla to San Rafael del Norte was destroyed by a multitude of cracks and clefts, which had suddenly opened. At that time the ground was observed to be undermined, falling in at intervals. These occurrences were observed some three days before M. Montenegro and his friends visited the place, which they saw all to be in accordance with the statements. Immediately on examining the locality for themselves they came to the conclusion that there were signs not of one but of two animals, probably of the shape of huge fishes.

In commencing their work these animals seemed to pursue a kind of an upheaving movement. As the bottom of their hiding-place was loose, shifting ground, the surface of this was seen to give way, while trees were shaken out and came down crashing. The noise of this seemed to scare the animals away. One of them—believed to have been the male on account of its larger size and greater strength—took to the left in descending, but always in a parallel direction with and along the slope of another hill, which here terminated. As it broke through the banks of a ravine, which measured about twenty varas in width and nine feet in depth at its greatest opening, he passed with his head underground. The thrown-up soil showed the tracks of the head, which left its marks both in the soil and on the roots of the trees, which were broken, the broken pieces being four inches thick. The main part of the body, which certainly must have passed here uncovered, left its traces at the bottom of the ravine. Passing out from this the animal entered upon ground more level and friable, which it went through at a depth of five quarters (1·25 varas), forming a furrow and leaving behind a ridge more than one vara high. Following the ravine for a distance of about sixty varas it encountered two deep ditches, when it turned and traced its way back, and, approaching the aforesaid ravine, took to the bed of a pond and disappeared perpendicularly.

The other animal, which left behind a smaller track, and therefore was believed to have been the female, went at once to the right, to the outlet of the pond of water before referred to, leaving behind it everywhere the same marks as the other. When it reached the two deep ditches it turned back also, and undoubtedly encountered its companion afterwards.

The whole ground had become irregularly disturbed and broken up, and the power of these animals is shown by their being able not only to throw up huge masses of soil but even to move rocks weighing more than thirty quintals.

The animals seem to be covered with a skin clad with scales or plates, the markings of which, imprinted on the soft clay or loam, bear much resemblance to those of the garrobo[1] in the mud. It appears that the shape of these animals must be like that of the guapote.[2] The length of the body is at least twelve varas, the height three, and its thickness 1·5 varas.

A tradition about such an animal as this has been kept up unaltered, without contradiction, for more than a hundred years. It is described in general as a large snake, and called "sierpe," on account of its extraordinary size, and living in chaquites.[3] One is said to have been once killed by lightning the moment it had left its hiding-place in the river "Sebaco viejo."

[1] "Roble," in Spanish, means an oak. The same name is found through various parts of Spanish America, applied to trees belonging to very different botanical families, having no natural affinities among each other, and also none to the "Cupuliferæ." On the River Atralo, in New Granada, South America, a huge tree, a species of *Tecoma*, is called "roble." The name "roble," no doubt, is given to these different forms merely on account of some external resemblance to an original Spanish species of oak.

[1] [2] "Garrobo" and "guapote" are names of which the exact application cannot be ascertained.

[3] "Chaquites" seems to be an Indo-Hispanic expression and a provincialism; probably pools or ponds is meant in the present case.

TWO AUSTRALIAN GEOLOGISTS

Australia has recently lost two of its most eminent geologists, the Rev. W. B. Clarke and Mr. Richard Daintree. The death of Mr. Clarke we noted some weeks

ago, and now by the kindness of a correspondent we are able to give a few details as to his career.

The Rev. W. B. Clarke was born on June 2, 1798, at East Bergholt, in Suffolk. In October, 1817, he went to Cambridge and entered into residence at Jesus College. In January, 1821, he took his B.A. degree, and in July, 1824, he was made M.A. and a member of the Senate. From May, 1821, until November, 1824, he officiated as curate of Ramsholt, Nedging, Whatfield, Chellesworth, and Brantham; after this he became curate for a time in his native parish. The rector seems to have had a proper appreciation of his talents, for he not only allowed but enjoined him to travel during a portion of each year, a privilege of which Mr. Clarke availed himself in order to pursue and complete the geological and mineralogical studies that he had commenced at the University under the teaching of Prof. Sedgwick and Dr. Clarke, the celebrated traveller in the Holy Land. During this period he made some fifteen distinct journeys of exploration either on the Continent or in different parts of his native land, enlarging his experience and acquiring fresh stores of information. In 1823 he was presented by Lord de Manley to a small vicarage in the county of Dorset, with a written promise of the succession to a desirable rectory in Gloucestershire. Having, however, rheumatic affection, and seeing no immediate prospect of succeeding to the Gloucestershire living, he determined to take a voyage to Australia, and arrived in Sydney in the year 1839. He appears to have found in Australia a fitting field, both for his labours as a clergyman and for the prosecution of his favourite studies in geology and mineralogy, and he at once applied himself to both. He had been considerably influenced in deciding to make Australia his home by correspondence he had had with the late Sir Thomas Mitchell, formerly Surveyor-General of the colony, and with the late Bishop Broughton, who had been his fellow-undergraduate at Cambridge. The first charge to which he was appointed was that of King's School, Parramatta, a position which he continued to fill until the beginning of the year 1841, after which he retired from the school, and attended only to his clerical duties. In 1844 he took charge of the parish of Willoughby, North Shore, with which he remained connected till 1870.

Mr. Clarke filled many positions of honour and distinction in connection with his own communion, and with learned and scientific bodies in various parts of the world. He was Fellow of St. Paul's College, vice-president of the Royal Society of New South Wales, trustee of the Australian Museum, trustee of the Public Free Library, Fellow of the Geological Society of England since the year 1826, member of the Geological Society of France, member of the Royal Geological Institute of Austria, member of the Royal Geological Society of London, &c., &c. In the department of science to which he had more particularly devoted his attention, he was regarded as an authority by all geologists. Perhaps his largest and best-known work is that entitled "The Southern Gold-fields," which contains an exhaustive description of the auriferous deposits throughout Australia. This work was written in consequence of Mr. Clarke having been commissioned by the Government to visit and report upon the principal gold-producing localities in the colony. A number of his papers were at different times read before the Royal Society of New South Wales, and are preserved among its records. They are almost entirely confined to subjects relating to geology, mineralogy, and meteorology.

Some two or three years ago Mr. Clarke was elected a member of the Royal Society of England—an honour which has been so rarely conferred upon colonial *savans* that Mr. Clarke valued it perhaps more than all the other distinctions he had won, especially as the honour was conferred upon him by the unanimous and spontaneous action of the Society. Mr. Clarke died on June 16.

The announcement of the death of Mr. R. Daintree, C.M.G., F.G.S., will be read with deep regret alike in this country and the Australian Colonies, more especially Victoria and Queensland. Next perhaps to that of Mr. Clarke, his name has been more intimately associated with geological science at the Antipodes than that of any other observer, more particularly in connection with the former province.

On his first arrival in Victoria Mr. Daintree became connected with Mr. A R. C. Selwyn, F.R.S. (now Director of the Geological Survey of Canada), in the geological survey of that colony, and did good work in the exploration of the Bass River, the survey of the Cape Patterson Coal-field, and other difficult explorations. He was also engaged in the survey of a large tract of country to the south-west of Melbourne, around the now flourishing town of Geelong, comprising the Barrabool Hills, the Anakil, and other minor ranges, and to the north the districts of Ballan and Bacchus Marsh. An accomplished photographer, his knowledge of this science was turned to good account in the preparation of a large number of photographs illustrative of the general geology of Victoria and Queensland, and more especially of the physical structure of the gold fields, and the methods in operation for the mining and extraction of gold. The formation and mode of occurrence of the precious metal attracted a good deal of his attention, more especially with regard to its presence in rocks associated with dioritic dykes in Queensland.

Mr. Daintree's connection with the Geological Survey of Victoria commenced in 1854, and, with the exception of a short interruption in 1857-8, continued down to 1864. In that year he left Victoria for North Queensland, and settled there as a "squatter," continuing at the same time to interest himself in matters geological. In 1869 the Queensland government appointed him government geologist for the northern half of the colony —a choice amply justified by the remarkably good work he performed whilst acting in that capacity. Queensland owes to Richard Daintree no ordinary debt of gratitude for the important part he took in the development of her mineral resources, the discovery of several important gold fields having followed quickly upon his prognostications.

Daintree's geological map of Queensland will give some idea of the vast tracts of country traversed and reported on during the course of his residence in that province, and on which the geological boundaries of the various formations were laid down with great care and precision. Whilst on this subject we would call attention more particularly to the definite delineation of the boundaries of the great secondary formation of north-eastern Australia about which little, comparatively speaking, was known up to that time, except the occurrence of typical fossils at a few isolated localities. The great tertiary series, aptly designated by Daintree the "Desert Sandstone," was shown to have extended over the greater part of Australia at one time.

In 1871 Mr. Daintree was appointed Special Commissioner for Queensland to the International Exhibition of 1872, and organised the admirably-arranged Queensland annexe, which was, in that and succeeding exhibitions, so universally admired. An experienced and determined bushman, he had, during his Australian career, paid too little attention to the preservation of his health, and in all probability laid the seeds of the complaint which afterwards terminated fatally. Soon after his appointment in March, 1872, as Agent-General for the Queensland Colony in London, his health began to give way, and his condition caused much anxiety to his numerous friends. Notwithstanding his removal to a warmer climate during our winters, signs of pulmonary disease

rapidly manifested themselves. He died last month, at the early age of forty-seven years. By his death there has passed from amongst us a true, most unselfish, and large-hearted man, a true friend, and a most agreeable companion. He was an accomplished geologist, a close and accurate zoological observer, a thoroughly practical chemist, and a photographer of no mean order. It will be long ere the vacancy in Australian science will be filled.

THE BRITISH ASSOCIATION

TWENTY-ONE years have passed since the British Association met in Dublin. It was then under the presidency of Dr. Lloyd, the venerable, but still hale, Provost of Trinity College. On Wednesday next, as our readers are aware, the forty-eighth annual gathering of this congress of science once more meets in the metropolis of Ireland, the President-Elect again being an eminent mathematical physicist, Dr. Spottiswoode.

Notwithstanding the fact that Dublin is now as easy of access from London as Edinburgh, and as near in point of time, yet the average Englishman knows far more of Paris or Switzerland than he does of the sister-isle. We trust the forthcoming meeting in Dublin will help to remove much of the prejudice with which Ireland is regarded, prejudice which proceeds from ignorance. For instance, not long ago an eminent scientific Englishman having been asked to lecture in Dublin, seriously inquired whether it would be advisable to be armed with a revolver, exhibiting a fear less reasonable than that of the man who, on a visit to Christiania, took precautions against being attacked by bears. Dublin no longer deserves the second adjective in the epithet of "dear dirty Dublin," in fact, its well kept streets, its splendid buildings and squares, the activity of its commercial and intellectual life, delight and surprise the stranger. In point of situation it is, perhaps (at least next to Edinburgh, our Scotch friends will think), the most beautiful capital in the world, backed by the Dublin and Wicklow Mountains, flanked on one side by the Hill of Howth, and on the other by Bray Head, the Bay of Dublin, with its clear blue water, is even comparable in beauty to that of Naples, if corresponding atmospheric conditions be granted.

Unusual facilities have been offered by the railway and steam-boat companies for the conveyance of visitors to Dublin. To some of our readers it may be convenient if we summarise the ways of reaching Dublin from London. The quickest route is of course by the Irish day or night mail from Euston Square: for example, leaving London at 8.25 P.M., one is landed in Dublin before seven the next morning. The splendid and perfect arrangements of the mail steamers from Holyhead to Kingstown are too well known for us to describe them. Recently the London and North Western Company have built two magnificent steamers, which run during the day from Holyhead to Dublin, and *vice versâ*. The fare is less than by the mail, and the boats are quite as large and sumptuous as the mail-boats, though not quite so fast. To those who intend travelling second class (there is no third by the mail) we should recommend their selecting the North Western boats from Holyhead to North Wall, Dublin; second class passengers being allowed to use the first class saloon without extra charge. The night boat, which meets the 5 P.M. train from Euston, is not quite so fine as the day boat, but the visitor has the advantage of entering the Bay of Dublin by daylight, reaching North Wall about 7 A.M. At the present season of the year no alarm need be felt on the score of sea-sickness by those who travel in the mail-boats from Holyhead to Kingstown. The boats are so large and steady that even with a considerable wind little motion is felt; and the passage is very short, about four and a half hours being the average. Another route is *viâ* Liverpool to Dublin, these are good boats. Lastly, to those who prefer a long sea-trip and can spare the time, nothing is more pleasant than going from London to Dublin direct by steamer. So much for transit. As regards accommodation in Dublin, the Executive Committee have provided a very complete list of lodgings; and the hotels, we understand, have not raised their usual tariffs.

The arrangements of the meeting we have already announced. The sections will meet in Trinity College, the addresses will be delivered in the Exhibition Palace, and lectures will be given by Mr. Romanes and Prof. Dewar an August 16 and 19. On August 15 a *soirée* will be given by the Royal Dublin Society to the Association. This promises to be a very brilliant affair. The Department of Science and Art has allowed a liberal selection of objects from the South Kensington Museum and the Science Collection to be lent for the occasion. Electrotype reproductions of many of the most interesting relics exhibited in the Loan Collection of Scientific Apparatus will be shown at this *soirée*, together with other curiosities from South Kensington. Dr. Spottiswoode has also kindly lent one of his new leviathan condensers which, used in conjunction with an enormous induction coil lent by Mr. Horatio Yeates, will be sure to attract much attention during the evening, and further, the Stereoscopic Company have promised to exhibit the phonograph at this *soirée*. A *conversazione* will also be given by the Royal Irish Academy, when its unrivalled museum of Irish archæology and antiquities will be seen to advantage and with interest by the members of the Association.

No neighbourhood lends itself so easily to beautiful excursions as that around Dublin, and the excursion programme this year is most varied and complete. Here is the list for Saturday, August 17:—

High Park and Artane Reformatories, to be entertained at the Artane Reformatory; St. Doulough's Church, Malahide Castle, and antiquities of Swords, to be entertained by the Right Hon. Lord Talbot de Malahide; Bray Head, Kilruddery Demesne, Hollybrook, Charleville, the Dargle and the Scalp, to be entertained by the Right Hon the Earl of Meath; Maynooth R. C. College, Carton, Lord Annaly's and Phœnix Park, to be entertained by his Grace the Duke of Leinster; Howth and Ireland's Eye (walking excursion), to be entertained to afternoon tea by residents of Howth; Lucan and Leixlip, Valley of the Liffey, Woodlands and Phœnix Park, déjeuner will be provided by committee at Lucan; Irish Lights Board, Dublin Bay—steamer *Alexandra*, to be entertained on board the steamer by Irish Lights Board; London and North Western Railway—steamer *Rose*, to be entertained on board the steamer by Committee; Glencree Reformatory, Killakee Demesne, Waterfall, Dargle and Enniskerry, to be entertained at Glencree Reformatory by the Managers.

For Thursday, August 22 this is the list:—

Glendalough and Seven Churches, to be entertained at Newrath Bridge; Vartry Waterworks, déjeuner at Vartry Lodge; Vale of Avoca, déjeuner at Glenart Castle, given by the Earl of Carysfort, and at Skelton Abbey, by the Earl of Wicklow; Boyne, déjeuner at Drogheda; Cashel, déjeuner at Limerick Junction; Parsonstown, déjeuner at Birr Castle, given by the Right Hon. the Earl of Rosse; Powerscourt, déjeuner at Powerscourt Castle given by Viscount Powerscourt; Curragh Camp, déjeuner at Stand House, Curragh; Kilkenny, déjeuner at Kilkenny Castle, by the Most Noble the Marquis of Ormonde; and on Friday, August 23, an excursion is arranged to Belfast, luncheon being provided at Glanmore, Lisburne, by Messrs. Richardson and Sons, with dinner at Belfast.

Dublin is famous for its hospitalities, and, amid other festivities, the following have been arranged:—On the morning of Monday, the 19th, members will be enter-

tained at breakfast in the Zoological Gardens by the Royal Zoological Society, in the afternoon at a dinner by the College of Physicians, and in the evening at a *conversazione* by the Royal College of Surgeons. Their Graces the Duke and Duchess of Marlborough will also hold a reception in the Viceregal Lodge and entertain a number of distinguished visitors at dinner.

Several eminent visitors have already announced their intention of being present at the meeting, among others Messrs. Cornu, Chevalier, Brown-Sequard, Emile de Laveleye, Perier, Feil, Bertrand, Ranvier, Maas, Zirkel, Vogel, Salensky, Kanitz, Wittmael, Stricker, Cope, Sylvester, Draper, Sterry Hunt, H. M. Stanley, and Capt. Burnaby.

Through the unceasing labours of Dr. Ball the Royal Astronomer for Ireland, Dr. Norwood, and their co-secretaries, Dr. Sigerson and Mr. Goff, the meeting promises to be an unusually good one.

NOTES

MR. CHARLES DARWIN has been elected Corresponding Member of the Paris Academy of Sciences in the section of Zoology by 26 votes against 14. This success is all the more notable that Mr. Darwin obtained only 5 votes in a scrutiny which took place quite recently. Prof. Asa Gray has been elected a corresponding member in the Section of Botany in succession to the late Dr. Braun of Berlin.

AT the meeting of the French Association, of which M. E. Fremy will be president, M. Janssen will give a lecture on a question of physical astronomy, Prof. V. Trélat one on the Hospital, and Prof. Marey another on graphic researches relative to animated motors. Among the sectional papers promised are the following:—In the Mathematical Sciences, Signor V. Cerruti, of Rome, on the infinitely small movements of a solid body. In Physics and Chemistry, Prof. Crova on the solar heat; M. Ducretet on the liquefaction of gases; M. Janssen on new data obtained by photography on the constitution of the sun, and on the constitution of photographic spectra of short exposure; M. Montigny on the scintillation of the stars; M. Woeikoff on climatological researches. In Natural Science, Dr. Alix on myology of mammals; Dr. Baillon on the development of the ovular teguments; Dr. Blandet on geological periods before the secular variations; Prof. Chauveaux on the rate of propagation of excitations in the vaso-motor nerves; Prof. A. Gaudry on the evolution of primitive mammals; M. A. F. Nogues on method in geology, and on the climatology of geological times; Dr. Topinard on the notion of race in anthropology. Altogether there are about 250 papers already down to be read.

WE notice that the Bavarian Academy of Sciences at Munich at its last session elected to membership the famous French chemist Prof. Adolphe Wurtz, of Paris. Prof. Wurtz is at present engaged in a careful study of the more modern chemical laboratories of the German universities, preliminary to the completion of the plans for the new laboratory in connection with the École de Médecine at Paris. This structure will face on the new Boulevard de St. Germain, and its erection will require about five years. When completed it is expected that it will rank among the model laboratories of the world.

THE Berlin Academy of Sciences has elected to its membership the astronomer, Prof. Aubers, and the archæologist, Prof. Conze.

SOME improved forms of microphone and telephone are described in the August number of *Scribner's Monthly*. One form of telephone, as devised by Mr. Phelps, gives surprisingly good results. It contains two diaphragms, and in shape somewhat resembles a double crown. Twelve permanent magnets bent into a circular form are used in place of the single magnet employed in other magneto-telephones. Six of these on each side of the instrument have their like poles joined to one of the cores which carry the helices, and radiate from it in as many different directions. The opposite poles are joined to the periphery of the diaphragm on the corresponding side of the instrument, while the helices are so connected that the currents generated in them when the diaphragms are made to vibrate mutually strengthen each other and thus contribute to the effectiveness of the apparatus. Some idea of the performance of these improved instruments will be conveyed by mentioning the results obtained at a recent exhibtion of them in the Sunday-school room of Dr. Wells's church, Brooklyn. Mr. Edison's carbon transmitter was used for sending, and Mr. Phelp's crown telephone for receiving. The sound was also reinforced at the receiving end by the use of a large paper cone, whose smaller extremity was held to the mouthpiece of the instrument. The circuit extended from the residence of Dr. Wells, near the church, to the lecture-room. Speech from the telephone was distinctly heard in all parts of the room by an audience of about three hundred persons, while the singing of a vocal quartette, solo singing, and guitar playing, were transmitted with surprising clearness and loudness. It should be observed, moreover, that the performance in this case was very different from the so-called musical telephones by means of which only the pitch and rhythm of the notes are distinguished, the tone always resembling that of a penny trumpet. In this instance the quality of the tone, which is the real life of music, was exactly reproduced; this is one of the characteristics of the magneto-telephone—everything is faithfully reproduced. Dr. Wells addressed the audience from his parlours through the telephone, and not only was he clearly understood, but his voice was also instantly recognised.

THE observatory of the University of Jena, which occupies a romantic site in the garden where Schiller wrote his "Wallenstein," has been for three years unoccupied since the death of Prof. Schrön in 1875. By a recent appointment Prof. Abbe has been assigned to the chair of astronomy, and will commence active duties in the observatory.

THE seventeenth annual meeting of the Devonshire Association for the Advancement of Science, Literature, and Art, was held on July 30 and 31, and August 1, at Paignton. Mr. W. Froude, F.R.S., was the president-elect, but in consequence of the lamented death of his wife that gentleman was unable to discharge the duties of the office, and at the last moment Sir Samuel Baker, F.R.G.S., was chosen to fill the vacant place, and delivered an address upon the chief points of progress in the past half century. The list of papers was a very full one, thirty-four in all, including the reports of the various committees through whose action much of the work of the Association is now systematised. Thus there are committees at work upon the subjects of Devonshire meteorology, folk lore, celebrities, verbal provincialisms, Dartmoor, the Devon domesday, and for the collection of scientific memoranda of a miscellaneous character; and to these two others were added at Paignton, one to collect and to record facts relating to Devonshire barrows, and the other to perform a similar duty with regard to ancient and still existing manorial customs. Scientific papers predominated, and among these geological papers occupied the foremost place. Mr. Pengelly, F.R.S., contributed a fourth instalment of his collections of the literature of Kent's Cavern prior to the investigations of the British Association; a fifth set of "Notes on recent Notices of the Geology and Palæontology of Devon;" and papers on "The Geology of the North-Eastern Coasts of Paignton;" "Cetacean Remains found in Torbay;" and a second instalment of "Notes on Slips (*i.e.*, blunders of various writers) connected with Devon-

shire." Mr. W. A. E. Ussher, F.G.S. (Geological Survey), read papers on "The Geology of Paignton," and "The Mouth of the River Exe;" Mr. R. N. Worth, F.G.S., "On the Origin of the Ossiferous Deposits at Oreston;" Mr. A.R. Hunt, F.G.S., "Notes on Torbay," the Rev. W. Downes "On the Fossils of the Culm Measures about Holcombe Regis," while Mr. E. Parfitt contributed another important instalment to his fauna of Devon —a list of the *Lepidoptera*. There were several other papers of a more general character. Some of the papers gave rise to lively discussions, and incidental to one by Mr. Ussher, Mr. Champernowne, F.G.S., entered upon a brief exposition of his views of the Devonian question. The whole of the papers read will appear in the *Transactions*. The membership of the Society continues highly satisfactory, approaching 400.

AFTER a lapse of some twenty years the Oreston quarries have yielded another ossiferous fissure, the contents of which were removed under the direction of Mr. R. N. Worth, F.G.S., and will be deposited in the museum of the Plymouth Institution. The quantity of remains is not large, and they are almost wholly of *Bos* and *Cervus*, but the find is valuable inasmuch as it adds to the Oreston cave fauna the Aurochs (*Bison priscus*) and Great Irish Elk (*Megaceros*), and thus reduces the points of difference between the Kent's Hole fauna and that of Oreston. The relics of the Aurochs include a very fine horn core in an excellent state of preservation.

IN its last session the Municipal Council of Paris voted a sufficient sum to defray the expenses of a laboratory for the detection of adulterations in articles of food, a want the necessity of which has long been felt. At the same session 6,000 francs was appropriated for the purchase of tickets to the Exhibition, to be placed at the service of the teachers of the city.

THE Royal Archæological Institute commenced its annual meeting this year at Northampton on Tuesday last week. Lord Talbot de Malahide, president of the Society, presided, and congratulatory addresses were read from the municipality, the local clergy, and Architectural Society. Lord Malahide then vacated the chair in favour of the Ven. Lord Alwyne Compton, president of the Northampton meeting, who delivered an address. The week was devoted to visits to places of archæological and historical interest and to sectional meetings.

THE British Medical Association commenced its forty-sixth annual meeting at Bath on Tuesday, when it was calculated that upwards of 1,000 members were present. The proceedings commenced in the morning with Divine Service at the Abbey. During the afternoon some business meetings were held, and in the evening Dr. Falconer, of Bath, the president for the year, delivered his opening address.

M. LEVERRIER established at the Paris Observatory a daily journal which published not only the warnings of the meteorological service, but also all the astronomical news of general interest. The separation principle having prevailed, the *Bulletin International* is now entirely devoted to meteorological purposes, and the Observatory has no means at its disposal for conveying to the public the observations it receives, except by the channel of daily papers. It was only by chance that information as to Watson's discovery of an intra-Mercurial planet was obtained by the papers from Admiral Mouchez. The Admiral is preparing to establish at the Observatory a course of lectures on astronomical observations during the winter months. During summer the pupils will be admitted to practise with instruments belonging to the establishment. The pavilion in the Paris Observatory in which has been placed the transit instrument given by M. Bischofsheim, will be called Bischofsheim Pavilion. Admiral Mouchez has decided that the public shall be admitted twice a month to the Paris Observatory instead of once as usual. Letters requesting admission are to be directed to the Secretary of the Observatory.

ON Sunday next a monument will be inaugurated at Chamounix to Jacques Balmat, who was the first to ascend Mont Blanc. The *fête* is due to the co-operation of the Geological Society of France and the French Alpine Club. The programme is a very brilliant one, comprising an ascent of Buet and various rejoicings at Chamounix.

THE Helvetic Society of Natural Sciences meets at Berne on August 12, 13, and 14.

THE monuments to Alexander and Wilhelm von Humboldt, the former by Prof. Begas, the other by Herr Otto, will soon be erected one on each side of the entrance gate to the Berlin University. Both monuments are in marble and the figures are represented in a sitting position.

SENOR RAIMONDY, one of the first scientific authorities of Peru, has just published a new work on the minerals of Peru which is specially intended for the use of those who wish to examine more closely the rich and valuable collection of Peruvian minerals sent to the Paris Exhibition.

WE recently referred to Dr. Siemens' idea that the Falls of Niagara might be utilised to supply industrial wants. This idea seems likely to be realised, as we learn that a company has been formed in America to make use of the Falls to transmit to Buffalo, twenty-two miles distant, a constant supply of compressed air, which it is expected will be used as a substitute for steam in the principal establishments at Buffalo.

AN earthquake is reported from Jenbach (Tyrol); it occurred on July 19, at 10.32 A.M., and lasted for ten seconds.

M. ELISÉE RECLUS, the eminent geographer, who had been sentenced to transportation for the part taken in the communistic troubles, but had his sentence mitigated to exile, has, by a recent decree of the President of the French Republic, been authorised to return to Paris.

THE great Trocadéro lift is in full operation. The number of persons that can be elevated at once is fifty. In a single day 1,200 persons have used it. The time required for ascending is about four minutes for an altitude of fifty-three metres. The velocity is rather great at first, but gradually diminishes, and is very slow at the end. The distance run is about $\frac{1}{10}$th of the elevation reached by the captive balloon.

THE ascent of the Giffard captive balloon was stopped on July 31, owing to the wind-pressure suddenly increasing to eighteen pounds per square yard. No attempt was made during the two following days, but the wind having diminished it ascended again on August 3, at two o'clock. The air was so humid that the balloon seen from the earth appeared almost lost in clouds. Various interesting observations have been made.

WE have received a specimen sheet of a catalogue of books and papers on Electricity and Magnetism, compiled by the late Sir Francis Ronalds, F.R.S., which will shortly be published by the Society of Telegraph Engineers. This catalogue, which contains more than 12,000 entries, and will probably extend to over 600 pages, is believed to include every important work and almost every paper which has been published upon the subject of Electricity and Magnetism up to the date of its author's death in 1873. It also forms a valuable catalogue of scientific works generally. Sir Francis Ronalds devoted the greater part of his lifetime to its compilation and in the formation of the valuable library now in the possession of this society. It is proposed, should the number of subscribers be sufficient to cover the extra cost for printing, &c., to issue a separate librarian's edition,

printed on one side of the paper only, for the use of librarians. The price to subscribers of each copy in this form will be 20*s*., delivered post free in the United Kingdom, and plus the extra postage for abroad. The price of the catalogue in the ordinary form will be 16*s*. The importance of this enterprise, not only to science but to bibliography generally, need not be insisted on. Those interested should communicate with the Librarian of the Society, 4, Broad Sanctuary, Westminster, S.W.

A LARGE coal-field, we learn from the Sheffield *Daily Telegraph*, has just been opened out near Hemsworth, about seven miles from Barnsley. The thick or Barnsley seam of coal has been reached on the estate of Mr. Allott, at a depth of 635 yards from the surface. This proves that a well-known and valuable bed exists in what may be termed the largest unworked coalfield in the West Riding. The shaft is the deepest in South Yorkshire, and the field in which the seam is worked commences at its southern extremity close to the town of Nottingham, and extending through Derbyshire, proceeding along the margin of the limestone to Barnsley and the east of it and the Ashworth Rock. The great coal-field, which includes parts of Derbyshire, Yorkshire, and Notts, is the largest in England, and only 150 square miles less in area than that of South Wales, the extreme length being sixty-six miles. The southern boundary is new red sandstone, and the strata rise and cross out westward near Bradford and Leeds, and then turning to the east, disappear under the magnesium limestone. The thickness of the bed near Hemsworth is about eight feet, which may be considered as about the average in South Yorkshire, while at Shireoaks Colliery, near Worksop, it is little more than three feet. The new colliery will be able to raise, when fully opened out, more than 1,000 tons daily, so that there will be an addition to the coal-producing power of South Yorkshire of 400,000 tons a year.

THE possibility of keeping a manatee in a healthy state in an aquarium is fully proved by the good condition of that at Westminster. It arrived on June 20, and has increased in size and done well. The only other specimen brought alive to England lived at the Zoological Gardens from August 6 to September 7 only, in 1875. At Westminster the animal is in a glass tank above the level of the ground, and accessible on both sides, so that its movements, position in floating, &c., can be well seen.

THE *Times'* notice of Bank Holiday amusements speaks of the Nubian Camp at the Alexandra Palace as an educational entertainment. In this we quite agree, and hope that every success may attend such instructive exhibitions as this.

RESPECTING the tornado, by which Canton was visited on April 11, the Rev. John Chalmers writes to the London Missionary Society that after careful inquiry and personal observation, he concludes that the whirlwind did not extend much further than three miles, the average width of its path being about three hundred yards. But on the next two days there were several distinct whirlwinds of a similar character in the neighbourhood. Over the space above indicated the tornado seems to have equalled in force and destructive effect anything of the kind heard of in the West Indies.

IN our "Meteorological Notes," vol. xviii. p. 287, the name of Mr. Eliot, who is officiating for Mr. H. F. Blanford in India, during the absence of the latter, should have been quoted instead of that of Mr. Blanford, as the authority for the forecast of the monsoon referred to.

AN astronomical correspondent suggests that in Mr. Lockyer's Eclipse telegram last week the words "Corona probably photographed in Siam. Fluorescent eye-piece worked well," should read "Corona probably photographed; it seemed fluorescent. Eye-piece worked well." With respect to the place of "Vulcan" as observed by Watson, R.A. 8° 26′ should be R.A. 8h. 26m.

THE additions to the Zoological Society's Gardens during the past week include a Slow Loris (*Nycticebus tardigradus*) from Borneo, three Chinese Cranes (*Grus longirostris*) from China, presented by Mr. Theodore Hance; a Mexican Deer (*Cervus mexicanus*) from Mexico, presented by Mr. A. Scrutton; a Common Fox (*Canis vulpes*), European, presented by Mr. Athelston Riley; three Great Bustards (*Otis tarda*) from Spain, presented by Lord Lilford, F.Z.S.; a Tiger Bittern (*Tigrisoma brasilianea*) from South America, presented by Mr. Hammond Hawbyne; a Greater Sulphur-crested Cockatoo (*Cacatua galerita*) from Australia, presented by Mrs. A. A. Hole; a Passerine Owl (*Glaucidium passerinum*), a Bacha Eagle (*Spilornis bacha*) from Borneo, presented by Mr. J. S. Jameson; two Beisa Antelopes (*Oryx beisa*) from North-East Africa, a Brown Coati (*Nasua nasica*) from America, deposited; an Ocelot (*Felis pardalis*), a Black Vulture (*Cathartes atratus*) from America; two Chilian Sea Eagles (*Geranoaëtus melanoleucus*), a Brazilian Caracara (*Polyborus brasiliensis*) from South America, two Rufous Tinamous (*Rhynchotus rufescens*) from Brazil, nine Spotted Tinamous (*Nothura maculosa*) from Buenos Ayres, a Razor-Billed Curassow (*Mitua tuberosa*) from Guiana, purchased; an Axis Deer (*Cervus axis*), born in the Gardens.

THE PHONOGRAPH AND VOWEL-SOUNDS[1]

II.

IN our last communication we confined our attention to the letter *ō*. We will now turn to the letter *ū* (corresponding to *oo* in "food"). Fig. 2 shows a series of curves obtained for

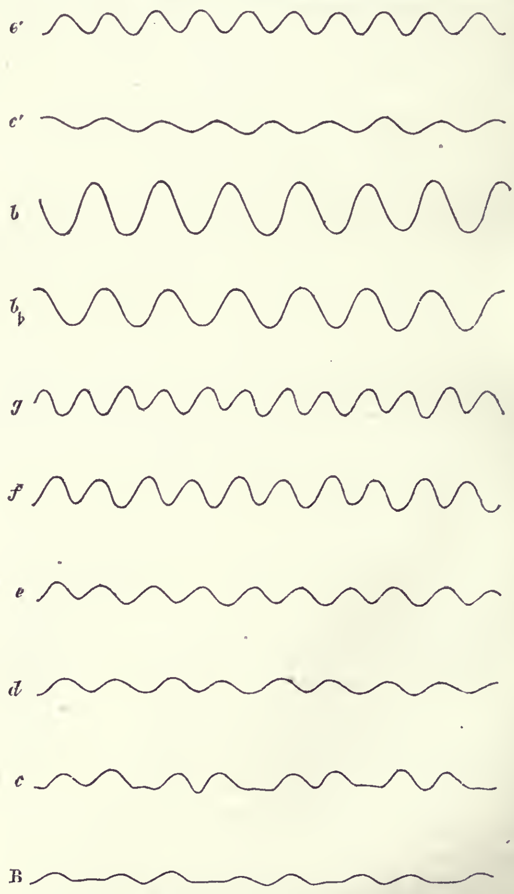


FIG. 2.—*Wave-forms of ū Sung by the same Voice at Various Pitches*

ū by the mechanical process already described. They are all

[1] Continued from p. 343.

sung by one voice: that numbered 5 in our previous communication.

Table V. gives the amplitudes of the first six harmonic partial tones for this series; and Table VI. gives the same constituents of another series of *ū*'s, sung by the voice formerly numbered 1.

Comparatively few curves have been transcribed for the sound *ū*, because the groups of constituents in this letter are much simpler than those in *ō*. Moreover, although the curves for only two voices have been analysed, we have by numerous examples satisfied ourselves that the selected examples represent with sufficient accuracy results produced by several other voices.

In examining Fig. 2 it must be borne in mind that the phonograph was turned in all cases at approximately the same speed, as accurately as this could be done by aid of a metronome. The length of the period in the curve corresponds, therefore, to the duration of the vibration which the voice produced on each note. On the upper path of the scale the wave-forms are almost exactly simple harmonic curves, but from *g* down to *d* the period is composed of two halves each closely resembling

TABLE V.—*Harmonic Constituents of ū Sung at Various Pitches by Voice No.* 5.

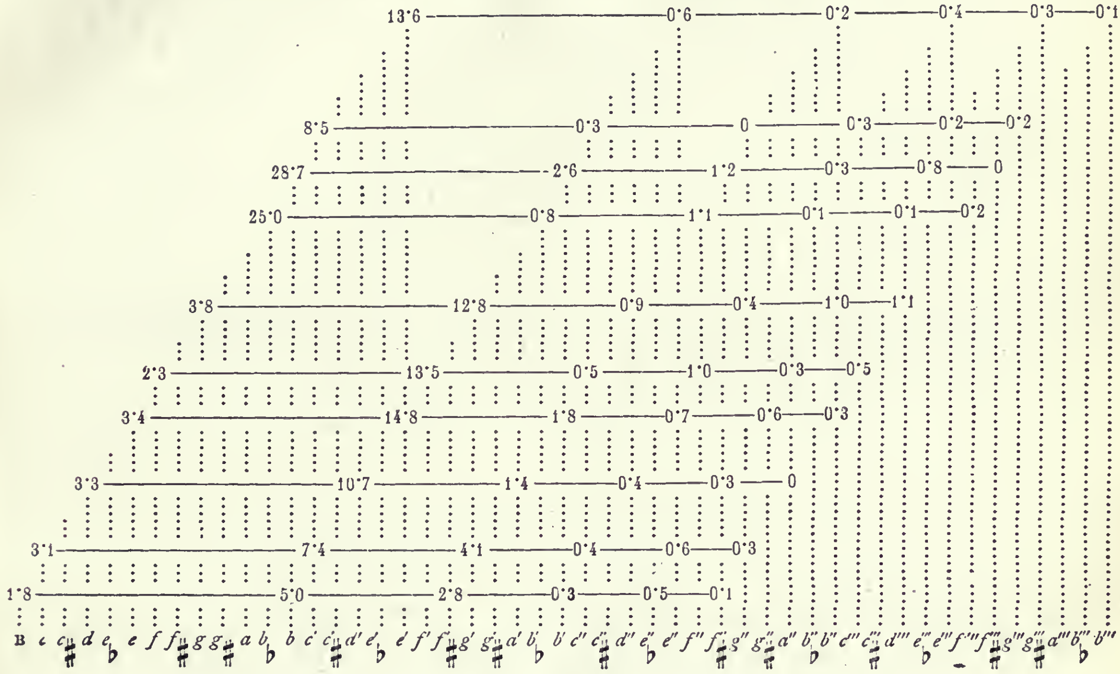
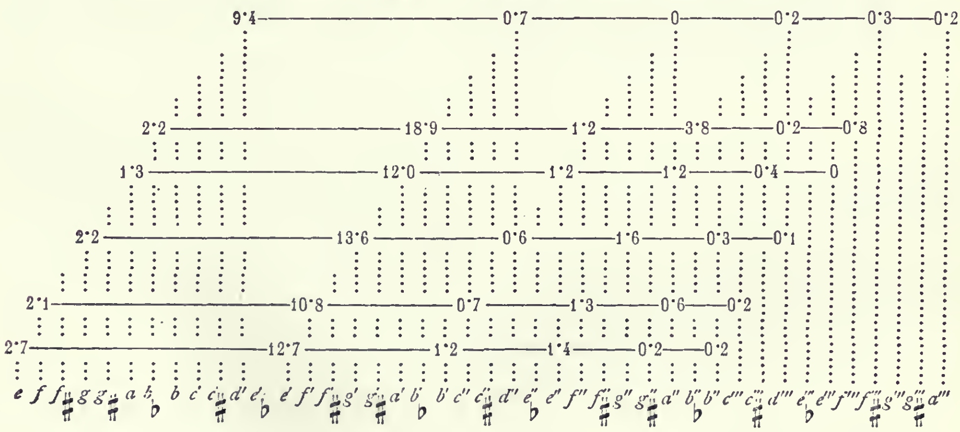


TABLE VI.—*Harmonic Constituents of ū Sung at Various Pitches by Voice No.* 1.



a simple harmonic curve, so that the form on *e*, for example, is not readily distinguished by eye from that on *e'*, an octave higher. Below *d* the curve breaks up into a somewhat irregular triplet.

We will call the curves for the notes above *b♭ simple ū* curves, and those from *g* downwards *duplex ū* curves. The analysis of the duplex curves shows that they consist of a very weak prime tone with an immensely strong second partial. The figures and tables might easily be extended above *e'*, but it is unnecessary to continue them, for the higher notes all give the same close approximation to the simple harmonic wave. We cannot say that we have obtained a good *ū* below *d* from any voice. The marks on the tinfoil were always feeble, and the sound as repeated by the phonograph was little more than an inarticulate groan. It will be observed that the note *a* is wanting in the tables and in the figure. It has been omitted because we had difficulty in getting a good *ū* back from the phonograph when the vowel was sung on this note. We have no doubt a good *ū* can be pronounced at this pitch, but we observed a constant tendency towards the *ō* character both in the curves and in the sound; that is to say, there was a tendency to give the two lowest partials instead of a simple prime, or a strong second and very weak prime. Moreover, the curves would vary in character

during the utterance, which was seldom, if ever, the case at other pitches, indeed, the constancy of the wave-forms produced by the voice over hundreds of periods has usually been very striking.

In order to maintain the *ū* quality throughout a series of *ū*'s, the change from the single to the duplex form had to be quite abrupt. To test the *ū* at this critical point *slurs* were executed by voice 5 both up and down; that is, attempts were made to

TABLE VII.—*Harmonic Constituents of ō Artificially Produced at Various Pitches.*

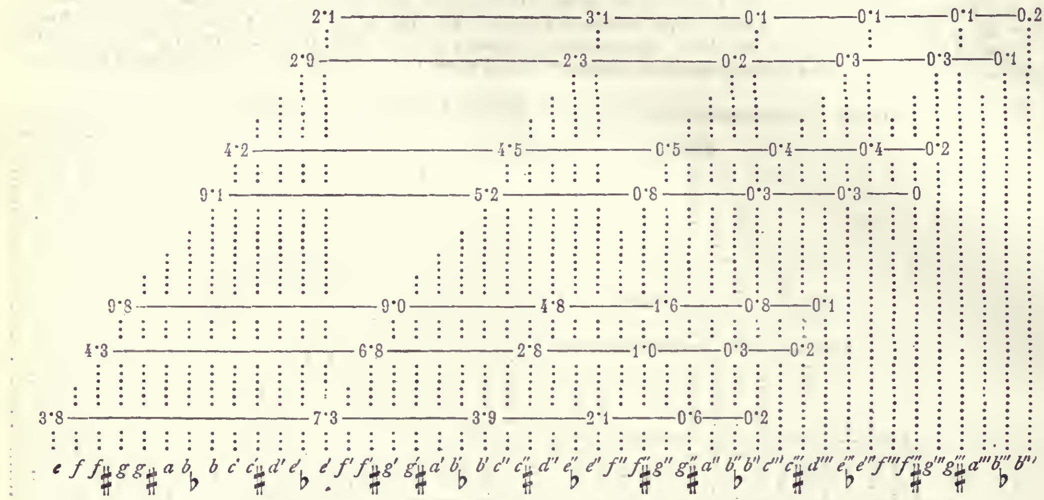


TABLE VIII.—*Harmonic Constituents of a° Sung at Various Pitches by Voice No.* 5.

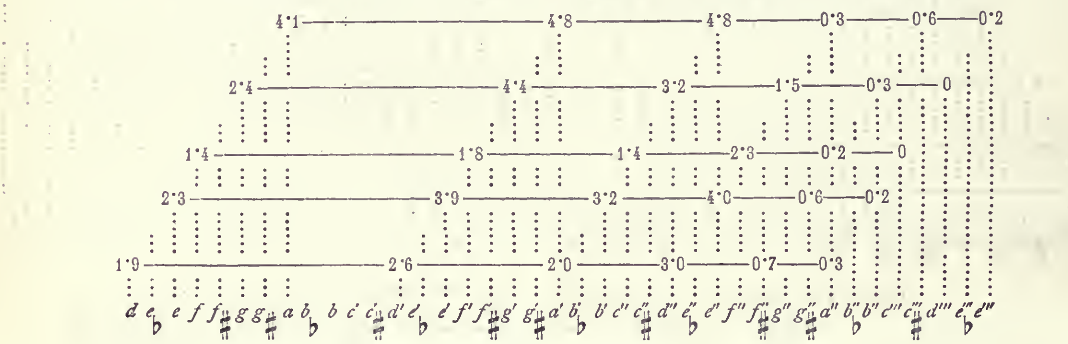
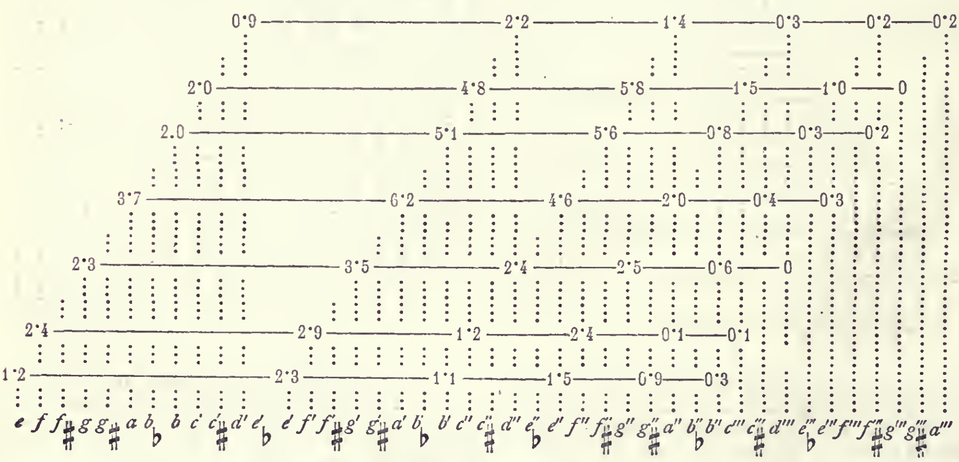


TABLE IX.—*Harmonic Constituents of ā Sung at Various Pitches by Voice No.* 5.



continue to say *ū*, and at the same time gradually raise and lower the pitch past *a*. At this pitch one of two things invariably happened. Either the curves became so like those for *ō* that they could not be distinguished from *ō* at the same pitch either by eye or ear, or they sunk to such an insignificant size as to indicate an interval of almost total silence.

One voice (No. 7) carried the simple harmonic form of *ū* down as low as *f*. This voice had a very limited range, and could not produce the duplex form at all. Moreover, the sound of the simple *ū*, while being spoken on *f* or *g*, was less vocal and more like a whistle than the duplex *ū* given by other voices.

One and the same voice, when singing on *a* or *b'♭*, would sometimes give the simple and sometimes the duplex form.

The following general propositions are, we think, established by these experiments:—

1. The generic character of *ū* from *d* upwards is given by the prominence of a single partial tone. Above *b♭* this partial is the prime. Below *a* with certain voices it is the octave of the prime.

2. The amplitude of the second partial is sometimes as much as nine times greater than that of the prime, but the sound will be recognised as *ū* when the second partial is only three or four times the size of the prime, provided the pitch of the prime is below *a*.

3. Below *d* the experiments do not warrant positive conclusions. The prominence of a single partial is less marked, and at the same time the vowel quality of the spoken sound is very poor.

4. The average pitch of the reinforced tones is generally lower than the average pitch of the group for *ō*.

5. We have not detected any single note having the marked characteristics for *ū* that were possessed by the note *b'♭* for *ō*.

6. There is a critical pitch in the neighbourhood of *a* or *b♭*, at which a sudden change takes place in the wave-forms given by certain voices in singing *ū*. Further experiments are desirable in this sound at this pitch. The critical pitch is not perfectly constant for a single voice.

As some doubt may not unnaturally be felt whether or not the singular change of character observed in *ū* at or near the note *a* may not have been due to some peculiarity of the instrument, we may repeat that, being fully aware of this danger, we tried the experiment repeatedly with changed mouth-pieces, changed vibrating discs, changed springs, and with different thicknesses of tin-foil, but invariably with the result that certain voices passed at this pitch from the simple to the duplex *ū*. It must not be forgotten, too, that one voice, using the same instrument, carried the simple *ū* as low as *f*.

7. This points to the fact that the change from simple to duplex *ū* is not made on account of the requirements of the ear, but on account of the difficulty in adjusting the mouth-cavity so as to continue the simple form on the lower notes. We have seen that, not only do different voices make the change at different pitches, but a single voice gives sometimes one and sometimes the other form at pitches near its critical point. We thus see that the simple and duplex forms overlap, and that in this case sounds of the same pitch, which differ by a whole octave in the pitch of their single prominent tone, are accepted by the ear as generically the same vowel.

The prominent tone in *ū* is found to lie in the region from *a* upwards, but we have not found any one sharply-defined pitch to be characteristic of *ū*.

With reference to the theory of characteristic tones in vowel-sounds we draw attention to the very great prominence of the tone *b'♭* (the characteristic tone of *ō*) when voice 1 sang *ū* or *b♭*. What may be termed the average pitch in *ū* is for notes near *d* and *e* lower than that in *ō*, owing to the absence of the third partial; but it is noteworthy that, as the voice runs up the scale, the constituents of *ū* are on an average higher than those of *ō* for *g*, and in the case of voice 1 even for *b♭*; this arises from the comparative smallness of the prime. When the simple form is reached the average pitch of the constituents is lower than that for *ō*. If, instead of looking at the average pitch, we look at the pitch of the highest and most prominent constituent, we find that this pitch is identical for *ū* and *ō* when these vowels are sung on *g* by voices 1 and 5, and when sung on *b♭* by voice 1.

Further light is thrown on the whole theory of vowels by experiments on the vowel *ō* as uttered by a mechanical contrivance made by Prof. Crum Brown and lent by him to us for investigation by help of the phonograph. It consisted of a bagpipe reed fitted to a tube leading into a gutta-percha resonance chamber which had such a form and such openings (tentatively arrived at) as caused it to speak a very good *ō* when the reed was blown. No one listening to this sound could doubt that the letter *ō* was being distinctly spoken. We held this apparatus to the phonograph, and the record obtained from it gave back a remarkably good *ō*, better indeed than the original, as the jarring noise of the reed was lost. When the pitch of the reed was changed, the same vowel continued to be given. The pitches in our experiments with this resonance bottle ranged from *e* to *e'*. We had thus a constant cavity producing unmistakably the same vowel throughout an octave.

Table VII. gives the constituents of each *ō* thus artificially produced. They agree with the *ō*'s pronounced by the voice in being composed of a prime and its octave for notes above *b*, also in having the first three partials on *e* and *f*♯. On *g* the third is stronger than in the human voice. No specially strong reinforcement appears to exist in this artificial *ō* for *b'♭*, or any tone near it, but there is a wide range of reinforcement extending on both sides of this pitch. It may be stated that this artificial *ō* was what might be called a bright *ō* whose pitch of general or of maximum resonance might be expected to be somewhat higher than that of the *ō*'s given by the human voice.

We draw attention to the fact that even supposing the prime not to have been reinforced at all, this irregular gutta-percha cavity must have reinforced tones, more or less strongly, through a range of at least fifteen semitones, namely, from *e'* to *g''*. This fact follows from the analyses, but it was confirmed by another and independent experiment. A short tube was inserted into the bottle in place of the reed and the end of the tube applied to the ear so that the cavity acted as a resonator to sounds from outside. The keys of a piano were then struck in succession and the listener noticed by the bumming noise in the bottle what tones were reinforced. On working down the scale resonance was first noticed at *g''*♯. It increased, and became excessively strong on *f''*, and remained nearly equally strong on *e''*. It then diminished a little, but became very intense again on *c''*♯, then diminished again, but even on *g'* and *f'*♯ it was much stronger than could be accounted for by the strengthening of the second partial. Below this the experiment was not suited to detect the influence of the cavity as a resonator on account of the reinforcement of upper partials in the notes struck. It sufficed, however, to prove that this irregular cavity possessed not only one, but probably several proper tones, so near to one another as to give the effect of a resonator strengthening, more or less, every tone between widely distant limits.

We have also made a few experiments on the vowel sounds "awe" and "ah," which we shall write *a*° and *ā* respectively. The phonograph spoke these sounds fairly well, though not quite so well as the sounds *ō* and *ū*. It may here be remarked that our phonograph was not capable of registering very high tones; a shrill whistle, however loud, produced no effect on the tinfoil. This fact proves, independently of the analyses, that in the case of those vowel sounds which it did speak well, the comparatively low partials were sufficient fully to characterise the vowel. The observations show that for *a*° the first three partials are prominent where *ō* has only two; and that for the same part of the scale *ā* has four consecutive partials all prominent. The fifth partial was considerably strong for *ā* when sung on *e*, where *ō* was composed of three partials and *a*° of four. Tables VIII. and IX. show the analyses made of these two vowels as sung by voice 5.

This communication is already so long that we must defer our general remarks to another number. FLEEMING JENKIN
J. A. EWING

EXPERIMENTS ON THE RELATIVE SPECIFIC GRAVITIES OF SOLID AND MELTED MATERIALS AT THE TEMPERATURE OF FUSION

DR. MUIRHEAD communicates the following account of experiments undertaken at his request, for the purpose of testing the notion that the earth's crust, as it cooled, became relatively heavier than the molten mass within; that the crust, breaking into fragments, sank; and this process, going on time after time, by and by built up a sort of honeycombed arrangement of the earth's interior:—

"*Railway Works, Leeds*, March 30, 1878

"DEAR SIR,—I have carefully gone over the experiments *re* the melting of metals in contact with liquid metals. I was certain on this point, from more than half a century's observation, before you wrote me, and I think I stated that conclusion in a former letter. I have now only to indicate the order in which I conducted the experiments, the result of which I now communicate.

"With several different compounds of brass, at various temperatures, I melted similar compounds; skimming the metal in the crucible, I laid the solid piece carefully on the clean surface, which piece, coating itself partially by chilling the liquid metal, very soon re-absorbed a sufficient amount of heat to be fused,

and, fusing from the *bottom side*, gradually dissolved. I then placed similar blocks of metal endwise on, when, dipping beneath the surface, they bounded back to the surface, and subsequently dissolved, endway down. These results apply to various weights and sorts of compounds. I then conducted similar experiments with cast-iron, and found that the facts were still more conspicuous in the cast-iron (all of the same tendency) than in brass.

"Placing the iron on the surface of the liquid iron, a rapid chill set in, and a coating of iron, apparently about ⅛" thick, attached itself to the cold iron, but very shortly re-melted, when the cold iron disappeared with it. I then dropped a small piece of cold iron (the same being dried to prevent explosion) endwise on to the surfaces of the liquid metal, when, bounding back to the surface, it melted in that position.

"The argument applies precisely to the experiments conducted in lead.

"In all cases the cold metals were relieved of any exterior ingredient by being well filed over. In every case of brass and iron the material melted was about 1" in diameter and 4" in length, each piece being round. With regard to the lead, the pieces varied in size, weight, and form, but all the experiments resulted in the same way. "JOSEPH WHITLEY"

"*Railway Works, Leeds*, April 11, 1878

"MY DEAR DR. MUIRHEAD,—Confirming my letter of yesterday, I have now to report the results of several experiments which, you will see, perfectly coincide with and demonstrate the truth I have again and again assured you of, viz., that all liquid matters that are susceptible of solidification will, when solid, float upon similar matter when in a liquid state.

"I intimated to you in my last that I feared I could not, in a small crucible, sufficiently fuse granite and whinstone, and in my experiments of yesterday, although I melted my crucible, I did not sufficiently liquefy the granite so as to float a piece upon the liquid mass. I therefore deferred further manipulations till to-day, and having secured a quantity of whinstone, I also determined to alter my course and to take advantage of a much larger focus of heat than that of a furnace 30" × 20" × 20", with a 60-lb. crucible. So I called upon Messrs. Taylor Bros. and Co., ironmasters of this town, and with their permission I proceeded as follows:—

"Being passed over by their manager to a subordinate officer—a worthy and very intelligent fellow, and, by the way, a strong believer in the doctrine that matter sinks in like matter when melted—we went to a furnace where we had three tests with whinstone, which he said disappeared, and I believe that he was justified in the two first experiments, because he was not sufficiently up in his observation as to notice a stream of gas liberated from a bubble formed on the surface by the melting of the whinstone immediately under it. In the next furnace we went to we had a large quantity of liquid 'cinder' 'tapped out' of a furnace into a trough. I really wish you could have seen it; to me the sight was grand, the gases given off by the melting of the whinstone blazed with a sort of blending of tints of purple, yellow, and green. I never saw anything so fine in flame. The whinstone was like a thing of life, so buoyant—of course the specific gravities of the liquid and solid materials varied considerably, and hence the buoyancy of the whinstone. We then tried a large number of small pieces of cinder—same as the liquid mass before us; but my friend the officer insisted that they went to the bottom—they certainly, except in one instance, never returned to the surface, because they liquefied before they had time to rise. My whinstone being done, and seeing that the results were not so satisfactory to my friend, I remarked that I was prepared to go all day and all night rather than give up the task of convincing him that his conclusions were wrong. I therefore suggested we should take a larger furnace, and deal with larger masses. We therefore, instead of dealing with quantities of 8 oz. weight, and weights of 1 lb., took pieces of 5 and 6 lbs. weight, each of solid cinder. Had my faith not been implicit I might have been deceived, for No. 1, 2, and 3 pieces went to the bottom, and my friend said, 'Now are you satisfied?' and I replied 'No, I am not.' Imagine his astonishment when No. 1 came bounding to the surface, and floated about like a cork, when the mass of heat had dissolved the coating which it clothed itself in at entering the bath and had begun to melt the original piece, up came No. 2 and 3, and I let him float them about on the surface with an iron rabble, so as to sear, as it were, the lesson sufficiently deep into his soul that it might never be erased. There were eight or nine jolly fellows looking on, and who enjoyed the joke, when my friend took off his hat and bowing politely said, 'Well, I am exceedingly obliged for the lesson you have taught me, and I shall never forget that all solid matter floats upon like matter when melted, as ice floats upon water.' Of course I had a joke and told him he was only one of a few who believed in the doctrine, and that he was the last convert. "JOSEPH WHITLEY"

P.S.—In the name of science I take this opportunity of tendering to Messrs. Taylor Bros., of the Clarence Iron Works, Leeds, my grateful thanks for their generous acquiescence in my request, and the facilities they kindly rendered in the experiments.—J. W.

THE INFLUENCE OF LIGHT UPON BIOPLASM[1]

SOME twelve months ago we briefly recorded in NATURE the results of our observations on the effect of sunlight on bacteria, and other organisms commonly associated with putrefaction and decay. Most of the experiments were subsequently described in detail in a paper communicated to the Royal Society. The chief of our earlier conclusions may be summed up shortly as follows:—

1. Light is inimical to, and under favourable conditions may wholly prevent, the development of these organisms, its action on the common forms of bacteria being apparently more powerful and rapid than upon the mycelial fungi which are prone to appear in cultivation-fluids.

2. This action appears to attain its maximum in the waves of greatest refrangibility. It is demonstrable in yellow light, but towards the red end of the spectrum sinks to a minimum.

3. The fitness of the cultivation-fluid to act as a nidus is not impaired by the insolation.

We found, moreover, that tubes containing a cultivation fluid and plugged with cotton wool, when removed to a dark place after exposure to the sun for a sufficient period remained perfectly clear and free from organisms for months. We thought, therefore, that the "germs" in these solutions had been completely destroyed by the solar rays.

While, however, we believe that, if the insolation be sufficiently prolonged, all the germs or spores originally present may be killed, and that, as regards bacteria, the insolation, under favourable conditions, need not be of very long duration, we have reason to think that, by *cell-walled* organisms, the destructive action of light may be resisted for a considerable period, and that the first result is to reduce the spore to a state of torpidity in which it may lie dormant for many months.

The investigation of this point, however, must necessarily extend over a long time; and in the above remarks we would wish to be understood as offering a suggestion rather than a definite conclusion.

We noticed last year that sunlight had no retarding effect on the *action* of the "indirect ferments," or, at least, of the soluble ferment of yeast (*zymase* of Béchamp, *ferment inversive* of Berthelot), which we used for our experiment. More recently, however, we have tested the effect of prolonged insolation on the soluble ferment itself, and have found that, at the end of three weeks' exposure to a midsummer sun, the *zymase* had entirely lost its characteristic property of hydrating cane-sugar, while a corresponding specimen, which had been kept in the dark, still retained its energy. It would appear, therefore, that the action of light affords no means of distinction between the "organised" and the "indirect" ferments.

We have attempted to elucidate the intimate nature of this action of light upon the organisms which have formed the subject of our experiments, and we have evidence pointing strongly, as we think, to the solution of the problem. Early in 1877 we set ourselves to this task, and, in order to obtain some insight into the effect of light upon certain organic bodies, we made a number of observations upon oxalic acid.

We have elsewhere[2] shown that a solution containing 0·63 per cent. (decinormal) is entirely decomposed by a somewhat prolonged exposure to strong sunlight when air is present. We now find that in a corresponding solution, *in vacuo*, no change

[1] By Arthur Downes, M.D., and T. P. Blunt, M.A. Oxon.
[2] *Chemical News*.

whatever is produced. This points conclusively to oxidation as the cause of the phenomenon.

We may here remark that this conclusion agrees with the results of the recent observations of M. Chastaing[1] upon a number of organic bodies, which he found to be oxidised under the influence of light.

As regards our oxalic acid, we have also determined that the oxidation in this instance is probably of the hydrogen and not of the carbon of the molecule.

From analogy and from direct experiment we believe that the mode in which light injuriously affects the organisms with which our investigation deals, is neither more nor less than a gradual oxidation of their living bioplasm, similar to the oxidation of the comparatively simple molecule of oxalic acid.

There is a lingering belief in the minds of many that matter which is endowed with life can by its "vital resistance" the more endure and survive the effect of injurious influences. This belief, derived, perhaps, like many others from a misapprehension of the indirect for the direct and from a misapplication of analogies, has no support from our experiments. On the contrary, we have met with results which are best explained by the consideration that bioplasm is matter of the utmost complexity and instability of constitution, ever-changing and most instable when the vital forces are at their full. We believe, in a word, that instability of this life-stuff is a predisposing cause for the destructive action of light, while in its stabler conditions it is more resistant.

We wish to keep this note within the limits of brevity, and will only remark in conclusion that, since the organisms which have been the subject of our work may be regarded as "life-units," well fitted by their tenuity for the demonstration of the action of sunlight upon the "physical basis" of their life, we may reasonably expect, and, indeed, may see, that this action is not limited to these special cases. We have chosen, therefore, in the heading of these remarks, to indicate the wider field of their application, but we by no means wish to imply that the relations of light to bioplasm are in all cases so simple.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

AT the Graduation Ceremonial in Medicine and Law in connection with the University of Edinburgh held on August 1 the honorary degree of LL.D. was conferred on James Risdon Bennett, M.D. Edinburgh, F.R.S., President of the Royal College of Physicians, London, Sir Joseph Fayrer, M.D. Edinburgh, K.C.S.I., F.R.S., John Richard Green, M.A. Oxon, Joseph Lister, M.B. London, F.R.S., lately Professor of Clinical Surgery in the University of Edinburgh, and now of King's College, London.

AT the annual meeting of the Council of the Royal School of Mines the prizes were awarded as follows:—The two Royal Scholarships, of 15*l*. each, for first year's students, to Mr. R. G. Scott and Mr. W. Cross; the Royal Scholarship, of 25*l*., to Mr. R. Lancaster; the Edward Forbes medal and prize of books, to Mr. P. F. Frankland; the De la Beche medal and prize of books, to Mr. F. G. Mills; the Murchison medal and prize of books, to Mr. M. Terrero; and an extra medal to Syed Ali.

MRS. CATHERINE DAUNTESEY FOXTEN has bequeathed to Owens College, Manchester, the sum of 5,000*l*. to found two scholarships, one Legal and the other Medical.

WORK has been commenced on the extensive edifices of the new Berlin Polytechnic, which is to be situated a short distance out of the city at Charlottenburg. Five years are expected to elapse before the completion of this much-needed institution.

THE city of Lille has received from the French Department of Education the sum of 50,000 francs to assist in the foundation of a medical school.

THE Imperial ukase announcing the foundation of the new Siberian University states that it shall number four faculties, the medical, the legal, the historico-philosophical, and the physico-mathematical. Orders have been issued for the prompt construction of the edifices, towards the expense of which the provincial government of Tomsk has already appropriated 250,000 roubles—about 35,000*l*.

[1] *Ann. de Chim. et de Phys.*, 5 ser. t. xi.

SCIENTIFIC SERIALS

Kosmos, April.—On life in Kosmos, by Carl du Prel, discussing possibilities of life in other worlds than ours.—The kingdom Protista, first part, by E. Haeckel.—On the physiology of the new-born, by M. Preyer. Part 1: On hearing and sight.—On the nests and gardens of *Amblyornis inornata*, or, rather, theories about them, by O. Beccari.—The sway of ceremonial, by Herbert Spencer. Part 4: On presents.—The discontinuance of human sacrifices, by E. Krause.

May.—The kingdom Protista, by E. Haeckel, describing Amœba, Protococcus, Euglena, Flagellata, Volvocina, Noctiluca, Infusoria, Acineta, Rhizopods, Foraminifera, and Radiolaria; forty-one excellent woodcuts.—On the physiology of the newly-born, by M. Preyer; on smell and taste.—The animal kingdom, as regards similarity of adaptations, by W. von Reichenau.—The sway of ceremonial, by Herbert Spencer: On marks of honour.—Sexual selection in plants, by W. Focke.

June.—Contribution to experimental æsthetics, by Rudolf Redtenbacher, pursuing Fechner's researches and discussing his results, analysing the causes of pleasure as regards the shapes and textures of flat surfaces, as well as of crystals.—Ernst Haeckel continues his popular articles entitled "The Kingdom Protista." He diverges very much into theory as usual, expounding his doctrine that the true animal kingdom is defined by the formation of a gastrula in its embryonic stage. On the side of phylogeny he considers the protista ascended to true animalism by becoming parasites.—Fritz Müller describes the queens of the Meliponæ, investigated in Brazil. He finds in four species the queens, or in some cases the parthenogenetic females, extraordinarily alike, while the males and workers are very different.—Herbert Spencer's sixth paper on the sway of ceremonial deals with forms of address; these articles are extracted from the future second volume of "Sociology."

Zeitschrift für wissenschaftliche Zoologie, vol. xxx. part 4.—On the origin of the sexual products in hydroida, by J. Ciamician; two plates of tubularia and endendrium.—Further contribution on the genus analges or dermaleichens (crustacean), by G. Haller; three plates.—On the structure of *Reniera semitubulosa* (sponge), by E. Keller; two plates.—On the structure of the Malpighian vessels of insects, by E. Schindler; 74 pages, three plates.

Vol. xxx., supplement, part 2.—Anatomical and zoological observations on the amphipods and isopods, by F. Leydig; fifty pages, four plates.—On the development of the testes and the alternation of generations in the salpæ, by W. Salensky.—On a mathematical method in zoology, illustrated from the acarida, by P. Kramer.—The reproductive organs of some ectoparasitic trematodes, by Carl Vogt; four plates, thirty-seven pages.—On the movements of "flying fish" through the air, by Karl Möbius; one plate, forty pages; giving an account of all observations on the movements of these fish, the anatomical structures on which they depend, and the way in which the mechanism works.—Studies of the freshwater fauna of Switzerland, by F. A. Forel. He distinguishes the characters of the littoral, the pelagic, and the deep-sea fauna of Lake Leman; discusses the relations between the present fauna and that of recent geological periods, and the migrations of species; and concludes that the lacustrine fauna of the subalpine waters has entered by migration since the glacial period; that the littoral fauna has come from other lakes of other lands; the pelagic fauna from forms already differentiated as such before migration; and the deep fauna by modification from the littoral.—On mud-dwelling cladocera, by W. Kurz; one plate, eighteen pages.

Vol. xxx., supplement, part 3.—On the early embryonic development of *Tendra zostericola*, by W. Repiachoff, one plate.—On the comet-forms of star-fishes, with a discussion of the phylogeny of the echinodermata, by E. Haeckel, one plate.—Contributions on protozoa, by A. Schneider: on actinosphœrium; on development of miliola; on trichosphærium and chlamydomonas, one plate.—On the form and signification of organic muscle-cells, by W. Flemming. The author believes he has discovered the development of unstriped muscle-cells in *Salamandra maculata* out of connective tissue cells, one plate.—On the anatomy of the entomostracan *Limnadia hermanni*, by F. Spangenberg.—Studies on the history of the Polish Tur, by A. Wrzesniowski. In this exhaustive paper of sixty pages all the historical references are examined, and most interesting woodcuts copied from representations of two animals are given. The conclusion is that two of the Bovidæ

remained extant in Poland to comparatively recent times, viz., *Bos primigenius* and *Bison europæus*, and that the former was the last to die out, in the beginning of the seventeenth century.—On the unity of the structure of the brain in the different orders of insects, by J. H. L. Flögel, illustrated by two plates of capital photographic reproductions of microtome-sections, thirty-seven pages.—On *Archigetes sieboldi*, a sexual cestode-nurse, by R. Leuckart.—The epiphysis on the brain of plagiostomes, by E. Ehlers, illustrated by two plates from *Raia clavata* and *Acanthias vulgaris*.

Bulletin de l'Académie Royale de Belgique, No. 5, 1878.—This number contains a memoir by M. Firket, treating of geological and chemical phenomena which have altered the rocks of an important metalliferous region in the province of Liége. The first part refers to the silurian fault and the metalliferous veins of the Champ d'Oiseaux; the second to the constitution and course of beds of oligiste; and the third to epigenic transformation of oolithic oligiste into siderite, in contact with a vein of pyrites (*à propos* of metalliferous veins of the mine of Landenne).—M. Spring, having conceived some doubts as to the existence of pentathionic acid, has examined anew the so-called pentathionates of potassium and baryum, and he finds these tetrathionates instead of pentathionates.—M. van Beneden announces the discovery of some gigantic fossil reptiles (probably Iguanodon) in the coal formation of Bernissart, near Peruwelz; and there is some correspondence between Count Du Moncel and MM. Navez on the subject of the telephone.

Reale Istituto Lombardo di Scienze è Lettere, Rendiconti, vol. xi. fasc. x.—We note the following papers in this number:—Critical annotations on duodenal anchilostoma, by Prof. Sangalli.—Causes and circumstances which influence hereditary transmission in animals (continued).—Hereditary transmission in the act of fecundation; note by Prof. Lemoigne.—Cure of varicocele by means of the temporary sub-cutaneous elastic ligature, by Prof. Scarenzio.—First lines of a cardiographic study designed for clinical purposes, by S. de Giovanni.—On the electromotive force developed from saline solutions of different degrees of concentration, with metals which do not form the base, by Prof. Cantoni.—*Résumé* of meteorological observations at Milan, in the Royal Observatory of Brera in 1877, by S. Fisiani, Jun.

The Journal of the Russian Chemical and Physical Societies (vol. x. No. 6) contains the following more important papers: On the action of iodide of butyl upon isobutylene in the presence of metallic oxides, by Miss Julie Lermontoff.—On quinine and cinchonine, by A. Wischnegradsky and A. Boutlerow.—On the fatty acids which are formed by the action of alkali upon cinchonine, by M. Lubavin.—On the hexylenes resulting from tertiary hexylic alcohols, and on their condensation, by L. Tavein.—On the preparation of bromide of trimethylene, by T. Bogomollez.—On the structure of the hydrocarbon $C_{19}H_{14}$ resulting from chloride of triphenyl-carbinol, by V. Hemilian.—On the anhydride of glyceric acid, by N. Socoloff.—On the formation of albumen from one of its products of decomposition, by A. Danilewsky.—On the action of bromine upon the compound homologues with benzol in the presence of bromide of aluminium, by G. Gustavson.—On diallylpropylcarbinol, by P. and A. Saytzeff.—On the conversion of primary alcohols into ethers, by N. Menschutkin.—On the theory of the current, by P. Van der Vliet.—On the resistance of steel, by M. Picatscheff.

SOCIETIES AND ACADEMIES

PHILADELPHIA

Academy of Natural Sciences, January 29.—On the mode of recognition among ants, by Rev. H. C. McCook.—Notes on the natural history of Fort Maçon, North Carolina, by Dr. Elliot Coues and Dr. H. C. Yarrow.—Description of new invertebrate fossils from palæozoic rocks of Illinois and Indiana, by Dr. C. A. White.

February 5.—Note on *Calycanthus floridus*, by Mr. T. Meehan.

February 26.—On the alkali of the plains in Bridger Valley, Wyoming, by E. Goldsmith.—On the mechanical genesis of tooth-forms, by J. A. Ryder, an important paper on mammalian teeth.

March 26.—On the electric constitution of the solar system, by Jacob Ennis.

April 2.—On the toilet habits of ants, by Rev. H. C. McCook.—On the Basilica spider and her snare, and on the probable geographical distribution of a spider by the trade winds, by same author.—Notes on *Acer rubrum*, by Mr. T. Meehan.

April 6.—On the vegetative repetition of cerebral fissures, by Dr. A. J. Parker.

PARIS

Academy of Sciences, July 29.—M. Fizeau president, in the chair.—New communication on the subject of the notes on alcoholic fermentation found among the papers of Cl. Bernard, by M. L. Pasteur. M. Pasteur finds that the notes, as printed in the *Revue Scientifique*, are in several places incorrect; M. Pasteur intends to repeat Bernard's experiments. M. Berthelot made a few observations on M. Pasteur's communication.—On the variations of the intensity of currents transmitted across mediocre contacts, according to the pressure exercised upon them, by M. Th. Du Moncel.—Absorption by the living organism, of carbonic oxide introduced in determined proportions into the atmosphere, by M. N. Gréhant.—On the *rôle* of coal dust in the production of explosions in mines, by M. L. Simonin.—New theory of the alterations caused by the phylloxera on the roots of the European vine, by M. Millardet.—Observations of the periodic comet of Tempel, made with the equatorial of the garden of the Paris Observatory, by M. Pr. Henry.—On the covariants of binary forms, by M. C. Jordan.—Note on a theorem on relative movements, by M. Laisant.—On the non-existence of the lengthening of a conductor traversed by an electric current, independently of calorific action, by M. R. Blondlot.—New observations on the subnitrates of commercial bismuth, by M. A. Carnot.—Thermic formation of phosphoric hydrogen and of arsenious hydrogen, by M. J. Ogier.—Researches on amylic alcohol (continued); dextrogynous alcohol, by M. J. A. Le Bel.—On the identity of the inulines of various origins, by MM. Lescœur and Morelle.—On the diffusion of salicylic acid in the animal economy (presence in the cephalo-rachidian liquid), by MM. Ch. Livon and J. Bernard.—On the anatomical characters of the Aye-Aye, by M. Edm. Alix.—On the influence of leaves in the production of sugar in beet-root, by MM. B. Corenwinder and G. Contamine.—Age of the bed of Mont Dol (Ille-et-Vilaine), by M. Sirodot.

CONTENTS

PAGE

THURSDAY, AUGUST 15, 1878

THE COMING ECLIPSE

WHEN I wrote two articles in NATURE a little while ago, discussing the various methods which I thought might with advantage be employed next Monday, I little thought that it would fall to my lot to come to America to take part in the observations. The fates, however, have so ruled it, and here I am, in what was not long ago called the "Great American Desert," but by no means a martyr to science; for, although Rawlins—where I now am—is nearly 7,000 feet high, and near the Rocky Mountain divide; although elk and antelope may be shot within a mile of the town; yet the sluggard is roused at six by the voluminous steam whistle of the railway works: there is a thriving "city" and population.

The energy displayed by the American astronomers is, if possible, greater than I anticipated. There is scarcely a man of note among them who is not now along the totality line which runs from the Yellowstone Park to the Gulf of Mexico. Where the wonderful Union Pacific Railway cuts the line east and west there will be four stations—Rawlins, Separation, Fulmore, and Creton. Along this line will be gathered Professors Newcomb, Harkness, Draper, Watson, with their many assistants. In the middle region, including Denver Central City, and Pike's Peak, will be Professors Young, Holden, Langley, Cleveland Abbe, and General Myer, the chief of the Weather Signal Service. The parties under these are many of them numerous, Prof. Young's camp, for instance, including thirteen persons. In the southern region, at Pueblo and Los Animas, Prof. Hall heads a large party, including Prof. Wright, of Yale, and Prof. Thorpe and Dr. Schuster from the old country.

In all three groups of stations the various kinds of work have been divided in a most judicious manner. In all attempts will be made to obtain the spectrum of the chromosphere and coronal atmosphere in the way suggested in my previous articles; in all the structure of the coronal atmosphere will be carefully inquired into. So far as photographs of the corona are concerned, perhaps the strongest attack will be made by an impromptu party not referred to in the preceding enumeration. On my way here from Cheyenne it was my great good fortune to travel with Prof. Hayden, *facile princeps* among the great geological surveyors of this vast continent. He was on his way to the north, and, as usual, had with him a strong photographic equipment. As his march lies along the line of totality, he will obtain, or at all events endeavour to obtain, a large series of photographs.

It is agreed on all hands that never has such summer weather been known in this locality. Ordinarily the chances, as determined by the officers of the signal service from their registers, are—Northern stations, 80 per cent.; Denver, 60; Pike's Peak, 40; and Los Animas, 80; but here, for the last fortnight, fine mornings have been succeeded by a break-up in the weather in the afternoon, while at Denver matters have been much worse.

A most valuable second series of instructions, written by Prof. William Harkness, of the United States Navy, by direction of Admiral Rodgers, has been published. Of these Sections I., II., V., and VIII. describe such observations as can be made with ordinary apparatus, while the other sections relate mostly to observations which can only be carried out by persons who are able to command expensive apparatus, and who are skilled in astronomy and physics.

This is a most useful following up of the work of organisation undertaken in England for the first time in 1870 and carried out in 1871 and 1875.

Prof. Harkness has freely availed himself of the Instructions compiled for the English Expeditions of those years, and in his carefully written memorandum has given us an opportunity of seeing how the problems have been advanced of late years; he has also collected a valuable series of data which give permanent value to it. I do not think I can do better than refer to some of the more important points touched on in the Instructions.

All the most rapid varieties of lenses in the market suited for use as equatorial cameras are given in the following table, in which the corresponding intensity ratios have been taken from Dallmeyer's catalogue.

Reference No.	Description of photographic objective.	Intensity ratio.[1]	Focal distance of largest lens made.	Diameter of image of sun.	Exposure required for the corona.
			Inches.	Inches.	s. s.
1	Extra quick acting portrait	$\frac{1}{2}$	$5\frac{1}{2}$	0·051	0·3 to 1·6
2	Quick acting portrait ...	$\frac{1}{3}$	$13\frac{1}{2}$	·126	0·7 to 3·6
3	Ordinary portrait	$\frac{1}{4}$	24	·224	1·3 to 6·4
4	Portrait and Group (D) ...	$\frac{1}{6}$	33	·308	2·9 to 14·4
5	Rapid rectilinear	$\frac{1}{8}$	$33\frac{1}{2}$	0·313	5·1 to 25·6

Prof. Harkness points out that "the data from which to determine an approximate value of C for the corona are very limited." He considers that it is probably safe to conclude that, with a clear sky and a moderately high sun, exposures in which the value of C is about 0·002 will give only the prominences and the outline of the moon. When C becomes 0·08 the corona will begin to appear, and will increase in extent as the exposure increases, at least up to the point where C becomes 0·40. Accordingly, the shortest exposure specified in the table above corresponds to $C = 0·08$, and the longest to $C = 0·40$.

If we adopt a lens of thirty-three inches focus an attempt can be made to use the lens for another purpose, "even more important than photographing the corona," that is in the search for intra-Mercurial bodies. Prof. Harkness points out that the magnitude of its intensity ratio enables it to depict faint objects rapidly, and the extent of its angle of view is such as to embrace a field of more than forty degrees. The lens will cover a plate measuring twenty by twenty-two inches, but as it is desirable to keep the apparatus light, plates measuring seventeen by twenty inches, which will suffice to cover a space of thirty-three and a half degrees along the ecliptic, are recommended.

[1] If F is the equivalent focal distance of a photographic objective, d it working aperture, C the exposure constant, whose value depends upon the intensity of the light and the sensitiveness of the chemicals employed, and the time of exposure required to produce a good negative, then the intensity ratio is $\frac{d}{F}$, and $t = C\left(\frac{F}{d}\right)^2$.

"Assuming the adoption of an equatorial camera twenty inches square, provided with a lens whose intensity ratio is one-sixth and whose focal distance is about thirty-three inches, it yet remains to consider how this apparatus should be managed during a totality lasting only three minutes. As the illumination of different parts of the corona varies greatly, there can evidently be no certainty of geting all the details of the phenomenon unless a series of plates are taken, in which the exposures vary from the shortest possible up to the point where it is certain that an increase of time does not improve the picture. On this account it will be desirable to take as many as six plates, the exposures being, respectively—

3^s, 5^s, 10^s, 20^s, 40^s, and 60^s.

The first four of these plates will receive such short exposures that it is unlikely they will show anything but the corona, and therefore their size should be 4¼ by 5½ inches. With the last two plates the case is different. Their size should be 17 by 20 inches, because their longer exposures will probably suffice to bring out upon them any bright points which may exist within their field. A lens such as is here under consideration should depict an eighth-magnitude star in about one minute, but of course the intensity of the sky-illumination during totality will determine the limit of brightness at which faint luminous points will cease to impress themselves upon the negatives, and what this limit may be it is impossible to predict. The necessity for at least two large plates is evident when it is remembered that the image of a small bright point could not be distinguished from an accidental blemish in the film, and it would only be by finding it upon both plates that its true character could be unmistakably recognised. It is exceedingly desirable to determine accurately the maximum exposure that the corona will bear with advantage, and it is hoped that on at least one of the large plates it will prove to be over-exposed."

It has been proposed to photograph the red prominences on a scale of ten seconds of arc to a millimetre. The optical apparatus for the production of such pictures must have an equivalent focal distance of 2062·7 centimetres, or 812·1 inches, and if we take C equal to 0·002, which is probably very near the truth, the value of t for lenses of various apertures are given as follows in the instructions :—

Aperture of objective.	$\frac{F}{d}$	$\left(\frac{F}{d}\right)^2$	Exposure required.	Motion of moon.
Inches.			s.	"
6	135·3	18306	36·6	20·1
8	101·5	10302	20·6	11·3
10	81·2	6593	13·2	7·2
12	67·7	4583	9·2	5·0
15	54·1	2927	5·8	3·2
20	40·6	1648	3·3	1·8
26	31·2	973	1·9	1·0

As a prominence one minute high could scarcely be photographed with a six-inch objective, because twenty seconds of its height would be covered by the advancing moon before the exposure was over, Prof. Harkness thinks it does not seem possible to photograph prominences during eclipses on the scale here contemplated with an aperture much less than ten inches.

The section relating to telescopic observation is very full and complete ; full instructions concerning the structure of the corona are given, and the remark is made that "Since the spectroscope furnishes an efficient means of studying the red prominences at any time, it will be very undesirable to waste a single one of the precious moments of totality in examining them."

To facilitate the work of such astronomers as may desire to search for intra-Mercurial planets with considerable telescopic power, a chart is given showing every star so large as the seventh magnitude in that portion of the heavens which will be occupied by the sun on the 29th of July next. The black circle in R.A. 8h. 36m. Dec. + 18° 39′ indicates the position of the sun. Mercury, Regulus, and Mars will be pretty close together, and probably quite conspicuous during totality, but they are so far to the eastward that only the last-named comes within the limits of the chart. Venus may also be seen, but she will be low in the western sky. While looking for planets, the possibility of discovering a small comet, or a meteor stream, should be borne in mind.

"The corona forms a luminous background upon which the moon's limb is sometimes seen projecting beyond the sun ; and a little before totality it is even possible that the complete outline of the moon may become visible. Look for these phenomena, and note the time of their occurrence. It is difficult to assign any reason for the existence of rays, or brushes, of light at the cusps of the sun, but it is said they have been seen. If any such appearances present themselves, they should be carefully scrutinised to ascertain if they change either their position or intensity ; and the interior of the telescope should be examined to make sure that they do not originate in reflections, either from the tube or from the lenses."

The instructions as to the use of the spectroscope and polariscope are so full that they deserve reprinting *in extenso*. I shall therefore say nothing about them here except to express my belief that no stone has been left unturned to secure results, if results be possible. Spectroscopically, I suppose Dr. Draper and Prof. Young have the strongest outfit, while, so far as I know, Prof. Harkness is the only one who is equipped for photographing the polarisation of the corona.

For the first time thermo-electric observation forms part of eclipse work. One of the many points of interest here, to me, has been the observatory in which Mr. Edison has been experimenting on his tasimeter. It is truly a very wonderful instrument, and from the observations made last night on the heat of Arcturus, it is quite possible that he may succeed in his expectations. For its extreme delicacy I can personally vouch. The instrument, however, is so young, that doubtless there are many pitfalls to be discovered. Mr. Edison, however, is no unwary experimenter.

So much, then, for the present. The day after tomorrow will find us all busier than ever, and if the weather prove fine I hope I shall have, as in 1870 and 1871, another distinct advance in solar physics to chronicle.

J. NORMAN LOCKYER

Rawlins, Wyoming Territory, July 27

OUR NATURAL HISTORY COLLECTIONS

THE Bill to enable the trustees of the British Museum to remove the natural history collections to South Kensington, on which we commented in our issue of August 1, has passed both Houses and virtually become law. The measure having been introduced at a late period of the session, and hurried through all its stages, evidently for the express purpose of eluding observation, it could hardly have been expected that the result would have been otherwise. The assent of the Treasury was of course secured before the Bill was introduced, and it is by no means surprising that, what with Cyprus and Turkey, and the enormous pressure of other more interesting business, it was never discovered by the Government that the Bill was exactly contrary to the recommendations of the Royal Commission on Science. So far, therefore, the trustees have it all their own way, and are now authorised to continue at South Kensington the system of government that has made the state of our national natural history collections at Bloomsbury so long a byeword amongst naturalists. There remains, however, still one more chance of introducing some salutary reforms into the present system. Following on the authority to remove the collections, which the trustees have now obtained, money will be required to carry out the transfer, and to obtain the requisite funds a fresh application to Parliament will be necessary.

This application will, no doubt, be made in the ensuing session, but before it is complied with we trust that some sort of terms will be obtained from the trustees. In the first place they should be required to delegate the control of the New Natural History Museum to a small committee, in which should be included the two or three naturalists who now happen to be members of the trust. It is obvious that most of the great dignitaries of state and eminent noblemen who form the trustees of the British Museum, neither know nor care anything about natural history. Until recently, indeed, there has not been a single person who could fairly be called a naturalist on the trust. Lately, however, two excellent naturalists (Sir John Lubbock and Lord Walsingham) have become ordinary members of the trust, and Sir Joseph Hooker, as president of the Royal Society, is *ex officio* one of the trustees. A committee consisting of these gentlemen and of a few others, who might be presumed to have some general acquaintance with the requirements of modern science, would, we need hardly say, completely command the confidence of naturalists, and would answer to the board of visitors, which the Royal Commission on Science in their Report suggested should be constituted to look after the director. In the second place the chief executive officer of the new museum should be appointed secretary to the trustees *ad hoc*, and the estimates for the two buildings should be kept entirely separate. So long as there remains any sort of subordination of the natural history to the principal librarian, who is the sole executive officer of the trustees at Bloomsbury, the old policy will be continued. The natural history will be starved in order to feed the overgrown library, and the petty restrictions and regulations which have so long vexed the souls of the visitors to the British Museum will be continued at South Kensington. The best chance of obtaining the necessary reforms lies in an entire change of administration, and for this reason it is much to be lamented that the recommendation of the Royal Commission on Science to place the New Museum of Natural History directly under the control of the Government has not been attended to. Still if the Trustees can be induced to commit the management of the new institution to a well-selected delegacy and to appoint a director free from the evil influence of Bloomsbury, there is every hope that our New Museum of Natural History may be worthy of the nation, and take rank with the sister institutions of Paris, Leyden, and Berlin.

THE BRITISH ASSOCIATION

DUBLIN, *Monday*

TO-DAY the Reception Room was opened in Trinity College, the Examination Hall being used for this purpose; and the doors were scarcely opened before a rush for tickets took place, and 500 associate tickets were sold in the course of two or three hours. As usual, the local honorary secretaries are working vigorously, and so far the arrangements they have made seem to be excellent; in fact, it is generally admitted that at few places has the British Association had so excellent a reception nor so convenient and complete accommodation. One feature in this meeting is the shilling lunch tickets, which every member can procure, and that entitles him to an excellent lunch, costing at least double the price of his ticket. The numerous foreign visitors have been lodged at the chief hotels, and private hospitality has been offered to the leading members of the Association who have announced their intention of being present.

With regard to a paragraph in a contemporary on the soreness which has been produced in Dublin by the cavalier treatment local scientific men have received from the Council of the Association, and stating that local papers would be withheld, we are able to contradict the latter part of this statement, although it cannot be denied that considerable umbrage has been given by the singular omission of some most distinguished local names from the first list of sectional officers; notably is this the case in Section G, Mechanical Science. But the list is stated to be incomplete, and doubtless the omissions will be repaired at the first meeting of the General Committee before these lines are published.

The guide-book which is issued to the Association has each year become a more and more important work, and that to Dublin is certainly the most complete and most carefully compiled of any that we have seen. Nine months ago a guide-book committee was formed, and this committee, after breaking up into different sections, has worked unremittingly to make their undertaking as accurate and complete as possible. No better editors could have been chosen than Professors MacAlister and McNab, who, besides the distinguished position they hold in their own departments of zoology and botany, possess an intimate acquaintance with the topography and biology of the neighbourhood, and who, since the beginning of the year, have unsparingly devoted their time to their arduous editorial labours—labours that have been largely augmented by the unexpected and unfortunate delays on the part of the firm of printers to whom the MSS. was first sent months ago. Thus it comes about that a somewhat awkward division is made into two unequal parts; but the division is one created by the printers and not by the nature of the contents. It is right to add that the thanks of the Association are due to Messrs. Dollard for the energy and skill which they have shown in the final printing of the guide-book. The book is illustrated with three maps; the first is a ten-mile to the inch map of the County Dublin; the second an admirable

geological map of the county; and the third in a pocket is a six-inch map of Dublin and the neighbourhood. A concise and valuable topographical sketch of Dublin, by Dr. MacAlister, opens the series of papers, Dr. J. W. Moore having added to this paper an important set of Meteorological Tables, some hitherto unpublished. The Rev. Maxwell Close contributes an important paper on the Physical Geology of Dublin, Mr. W. H. Bailey a paper on the Palæontology of the Neighbourhood, Dr. Haughton on the Mineralogy of the County, J. T. Pim on the Textile Industries, Dr. Emerson Reynolds on the Places of Manufacturing and Industrial Interest in and around Dublin; then follows Historical Notes and Antiquities of Dublin, by Dr. MacAlister, after which an extended and valuable list of the Flora of the Counties of Dublin and Wicklow is contributed by Mr. W. Archer, F.R.S., Dr. Perceval Wright, the Rev. Eugene O'Meara, Dr. D. Moore, Mr. G. Pim, and Mr. A. G. More. The fauna of the county is next dealt with, being compiled by Mr. H. W. Mackintosh, Mr. W. F. Kirby, Prof. W. R. McNab, the Rev. B. W. Adams, Prof. MacAlister, Mr. A. G. More, and Mr. R. M. Barrington. This unique and invaluable guide-book ends with a notice of the more interesting of the corporation records compiled by the Rev. W. G. Carroll.

We have already noticed the extensive preparations made for the *soirée* to be given to the Association by the Royal Dublin Society on Thursday. At this *soirée*, among many other things, Mr. Johnstone Stoney, the honorary secretary of the Society, will show the remarkable absorption spectrum of chloro-chromic anhydride, Mr. Gordon his recent and beautiful experiments on electric induction, and Prof. Barrett the effect of inaudible vibrations on sensitive flames.

One of the special objects of interest to be seen in Dublin is the great telescope Mr. Howard Grubb is constructing for the Austrian Government. We may state that the mechanical part is almost complete; the object-glass, which is to be twenty-seven inches in clear aperture, is of course not ready, as the glass was only received from Paris in the spring of this year. Visitors, however, need not be disappointed, as there is plenty to see in the mechanical details of the instrument and the great forty-five-foot dome for the covering of the telescope, the largest dome ever constructed. We may add, in conclusion, that among other places of interest, a visit to the Royal College of Science on Stephen's Green, with its museum of Irish industries and its excellent equipment of physical and mechanical apparatus, should not be omitted.

Inaugural Address of William Spottiswoode, M.A., F.R.S., LL.D., D.C.L., President.

Introductory.

On looking back at the long array of distinguished men who both in this and in the sister countries have filled the chair of the British Association; on considering also the increased pains which have been bestowed upon, and the increased importance attaching to, the Presidential Address, it may well happen when, as on this occasion, your choice has fallen upon one outside the sphere of professional science, that your nominee should feel unusual diffidence in accepting the post. Two considerations have, however, in my own case outweighed all reasons for hesitation: first, the uniform kindness which I received at the hands of the Association throughout the eight years during which I had the honour of holding another office; and secondly, the conviction that the same goodwill which was accorded to your treasurer would be extended to your president.

These considerations have led me to arrange my observations under two heads, viz., I propose first to offer some remarks upon the purposes and prospects of the Association with which, through your suffrages, I have been so long and so agreeably connected; and secondly, to indulge in a few reflections, not indeed upon the details or technical progress, but upon the external aspects and tendencies of the science which on this occasion I have the honour to represent. The former of these subjects is, perhaps trite, but as an old man is allowed to become garrulous on his own hobby, so an old officer may be pardoned for lingering about a favourite theme. And although the latter may appear somewhat unpromising, I have decided to make it one of the topics of my discourse, from the consideration that the holder of this office will generally do better by giving utterance to what has already become part of his own thought, than by gathering matter outside of its habitual range for the special occasion. For, as it seems to me, the interest (if any), of an address consists, not so much in the multitude of things therein brought forward, as in the individuality of the mode in which they are treated.

British Association, past History.

The British Association has already entered its fifth decade. It has held its meetings, this the forty-eighth, in twenty-eight different towns. In six cities of note, viz., York, Bristol, Newcastle-on-Tyne, Plymouth, Manchester, and Belfast, its curve of progress may be said to have a node, or point through which it has twice passed; in the five Universities of Oxford, Cambridge, Dublin, Edinburgh, and Glasgow, and in the two great commercial centres, Liverpool and Birmingham, it may similarly be said to have a triple point, or one through which it has three times passed. Of our forty-six presidents, more than half (twenty-six, in fact), have passed away; while the remainder hold important posts in science, and in the public service, or in other avocations not less honourable in themselves, nor less useful to the commonwealth. And whether it be due to the salubrity of the climate, or to the calm and dispassionate spirit in which science is pursued by its votaries here, I do not pretend to say; but it is a fact that the earliest of our ex-presidents still living, himself one of the original members of the Association, is a native of and resident in this country.

At both of our former meetings held in Dublin, in 1835 and 1857 respectively, while greatly indebted to the liberal hospitality of the citizens at large, we were, as we now are, under especial obligations to the authorities of Trinity College for placing at our disposal buildings, not only unusually spacious and convenient in themselves, but full of reminiscences calculated to awake the scientific sympathies of all who may be gathered in them. At both of those former Dublin meetings the venerable name of Lloyd figured at our head; and if long-established custom had not seemed to preclude it, I could on many accounts have wished that we had met for a third time under the same name. And although other distinguished men, such as Dr. Robinson, Professors Stokes, Tyndall, and Andrews, are similarly disqualified by having already passed the presidential chair, while others again, such as Sir W. R. Hamilton, Dr. M'Cullagh, and Prof. Jukes, are permanently lost to our ranks; still we should not have had far to seek, had we looked for a president in this fertile island itself. But as every one connected with the place of meeting partakes of the character of host towards ourselves as guests, it has been thought by our oldest and most experienced members that we should better respond to an invitation by bringing with us a president to speak as our representative than by seeking one on the spot; and we may always hope on subsequent occasions that some of our present hosts may respond to a similar call.

But leaving our past history, which will form a theme more appropriate to our jubilee meeting in 1881 at the ancient city of York, I will ask your attention to a few particulars of our actual operations.

Its Relation to other Societies.

Time was when the Royal Societies of London and Edinburgh and the Royal Irish Academy were the only representative bodies of British science and the only receptacles of memoirs relating thereto. But latterly, the division of labour, so general in industrial life, has operated in giving rise to special societies, such as the Astronomical, the Linnæan, the Chemical, the Geological, the Geographical, the Statistical, the Mathematical, the Physical, and many others. To both the earlier or more general, and the later or more special societies alike, the British Association shows resemblance and affinity. We are general in our comprehensiveness; we are special in our sectional arrangement; and in this respect we offer not only a counterpart, but to some extent a counterpoise, to the general tendency to subdivision in science. Further still, while maintaining in their integrity all the elements of a strictly scientific body, we also include, in our character of a microcosm, and under our more social aspect, a certain freedom of treatment, and interaction of our various

branches, which is scarcely possible among separate and independent societies.

The general business of our meetings consists first, in receiving and discussing communications upon scientific subjects at the various sections into which our body is divided, with discussions thereon; secondly, in distributing, under the advice of our Committee of Recommendations, the funds arising from the subscriptions of members and associates; and thirdly, in electing a council upon whom devolves the conduct of our affairs until the next meeting.

The communications to the sections are of two kinds, viz., papers from individuals, and reports from committees.

Papers for the Sections, &c.

As to the subject matter of the papers, nothing which falls within the range of natural knowledge as partitioned among our sections, can be considered foreign to the purposes of the Association; and even many applications of science, when viewed in reference to their scientific basis, may properly find a place in our proceedings. So numerous, however, are the topics herein comprised, so easy the transition beyond these limits, that it has been thought necessary to confine ourselves within this range, lest the introduction of other matters, however interesting to individual members, should lead to the sacrifice of more important subjects. As to the form of the communications, while it is quite true that every scientific conclusion should be based upon substantial evidence, every theory complete before being submitted for final adoption, it is not the less desirable that even tentative conclusions and hypothetical principles, when supported by sufficient *primâ facie* evidence, and enunciated in such a manner as to be clearly apprehended, should find room for discussion at our sectional meetings. Considering, however, our limitations of time and the varied nature of our audience, it would seem not inappropriate to suspend, mentally if not materially, over the doors of our section rooms, the Frenchman's dictum, that no scientific theory "can be considered complete until it is so clear that it can be explained to the first man you meet in the street."

Special Reports.

Among the communications to the sections undoubtedly the most important, as a rule, are the Reports; that is to say, documents issuing from specially appointed committees, some of which have been recipients of the grants mentioned above. These reports are in the main of two kinds, first, accounts of observations carried on for a series of years, and intended as records of information on the special subjects; such, for instance, have been those made by the Kew Committee, by the Committees on Luminous Meteors, on British Rainfall, on the Speed of Steamships, on Underground Temperature, on the Exploration of Certain Geological Caverns, &c. These investigations, frequently originating in the energy and special qualifications of an individual, but conducted under the control of a committee, have in many cases been continued from year to year, until either the object has been fully attained or the matter has passed into the hands of other bodies, which have thus been led to recognise an inquiry into these subjects as part and parcel of their appropriate functions. The second class is one which is perhaps even more peculiar to the Association; viz., the reports on the progress and present state of some main topics of science. Among these may be instanced the early Reports on Astronomy, on Optics, on the Progress of Analysis; and later, those on Electrical Resistance and on Tides; that of Prof. G. G. Stokes on Double Refraction; that of Prof. H. J. Smith on the Theory of Numbers; that of Mr. Russell on Hyperelliptic Transcendents, and others. On this head Prof. Carey Foster, in his address to the Mathematical and Physical Section at our meeting last year, made some excellent recommendations, to which, however, I need not at present more particularly refer, as the result of them will be duly laid before the section in the form of the report from a committee to whom they were referred. It will be sufficient here to add that the wide extension of the sciences in almost every branch, and the consequent specialisation of the studies of each individual, have rendered the need for such reports more than ever pressing, and if the course of true science should still run smooth it is probable that the need will increase rather than diminish.

If time and space had permitted, I should have further particularised the committees, occasionally appointed, on subjects connected with education. But I must leave this theme for some future president, and content myself with pointing out that the British Association alone among scientific societies concerns itself directly with these questions, and is open to appeals for counsel and support from the great teaching body of the country.

Grants.

One of the principal methods by which this Association materially promotes the advancement of science, and consequently one of its most important functions, consists in grants of money from its own income in aid of special scientific researches. The total amount so laid out during the forty-seven years of our existence has been no less than 44,000*l.*; and the average during the last ten years has been 1,450*l.* per annum. These sums have not only been in the main wisely voted and usefully expended, but they have been themselves productive of much additional voluntary expenditure of both time and money on the part of those to whom the grants have been entrusted. The results have come back to the Association in the form of papers and reports, many of which have been printed in our volumes. By this appropriation of a large portion of its funds the Association has to some extent anticipated, nay, even it may have partly inspired the ideas now so much discussed, of the endowment of research. And whether the aspirations of those who advocate such endowment be ever fully realised or not, there can, I think, be no doubt whatever that the Association in the mattter of these grants has afforded a most powerful stimulus to original research and discovery.

Regarded from another point of view these grants, together with others to be hereafter mentioned, present a strong similarity to that useful institution the Professoriate Extraordinary of Germany, to which there are no foundations exactly corresponding in this country. For beside their more direct educational purpose, these professorships are intended, like our own grants, to afford to special individuals an opportunity of following out the special work for which they have previously proved themselves competent. And in this respect the British Association may be regarded as supplying, to the extent of its means, an elasticity which is wanting in our own universities.

Other Funds.

Besides the funds which through your support are at the disposal of the British Association there are, as is well known to many here present, other funds of more or less similar character at the disposal, or subject to the recommendations, of the Royal Society. There is the Donation Fund, the property of the Society; the Government Grant of 1,000*l.* per annum, administered by the Society; and the Government Fund of 4,000*l.* per annum (an experiment for five years) to be distributed by the Science and Art Department, both for research itself and for the support of those engaged thereon, at the recommendation of a committee consisting mainly of Fellows of the Royal Society. To these might be added other funds in the hands of different scientific societies.

But although it must be admitted that the purposes of these various funds are not to be distinguished by any very simple line of demarcation, and that they may therefore occasionally appear to overlap one another, it may still, I think, be fairly maintained that this fact does not furnish any sufficient reason against their co-existence. There are many topics of research too minute in their range, too tentative in their present condition, to come fairly within the scope of the funds administered by the Royal Society. There are others, ample enough in their extent, and long enough in their necessary duration, to claim for their support a national grant, but which need to be actually set on foot or tried before they can fairly expect the recognition either of the public or of the government. To these categories others might be added; but the above-mentioned instances will perhaps suffice to show that even if larger and more permanent funds were devoted to the promotion of research than is the case at present, there would still be a field of activity open to the British Association as well as to other scientific bodies which may have funds at their disposal.

On the general question it is not difficult to offer strong arguments in favour of permanent national scientific institutions; nor is it difficult to picture to the mind an ideal future when Science and Art shall walk hand in hand together, led by a willing minister into the green pastures of the Endowment of Research.[1] But while allowing this to be no impossible a

[1] It is worth while to compare the following passage from Plato's "Republic," Book vii. (Jowett's translation):—
After plane geometry, we took solids in revolution instead of taking solids

future, we must still admit that there are other and less promising possibilities, which under existing circumstances cannot be altogether left out of our calculations. I am therefore, on the whole, inclined to think that, while not losing sight of larger schemes, the wisest policy, for the present at all events, and pending the experiment of the Government Fund, will be to confine our efforts to a careful selection of definite persons to carry out definite pieces of work; leaving to them the honour (or the onus if they so think it) of justifying from time to time a continuation of the confidence which the government or other supporting body may have once placed in them.

Continuance of British Association.

Passing from the proceedings to other features and functions of our body, it should be remembered that the continued existence of the Association must depend largely upon the support which it receives from its members and associates. Stinted in the funds so arising, its scientific effectiveness would be materially impaired; and deprived of them, its existence would be precarious. The amount at our disposal in each year will naturally vary with the population, with the accessibility, and with other circumstances of the place of meeting; there will be financially, as well as scientifically, good years and bad years. But we have in our invested capital a sum sufficient to tide over all probable fluctuations, and even to carry us efficiently through several years of financial famine, if ever such should occur. This seems to me sufficient; and we have therefore, I think, no need to increase our reserve, beyond perhaps the moderate addition which a prudent treasurer will always try to secure, against expenditure which often increases and rarely diminishes.

But however important this material support may be to our existence and well being, it is by no means all that is required. There is another factor which enters into the product, namely, the personal scientific support of our best men. It is, I think, not too much to say, that without their presence our meetings would fail in their chief and most important element, and had best be discontinued altogether. We make, it must be admitted, a demand of sensible magnitude in calling upon men who have been actively engaged during a great portion of the year, at a season when they may fairly look for relaxation, to attend a busy meeting, and to contribute to its proceedings; but unless a fair quota at least of our veterans, and a good muster of our younger men, put in their appearance, our gatherings will be to little purpose. There was a period within my own recollection when it was uncertain whether the then younger members of our scientific growth would cast in their lot with us or not, and when the fate of the Association depended very much upon their decision. They decided in our favour; they have since become presidents, lecturers, and other functionaries of our body; with what result it is for you to judge.

Of the advantages which may possibly accrue to the locality in which our meetings are held, it is not for us to speak; but it is always a ground for sincere satisfaction to learn that our presence has been of any use in stimulating an interest, or in promoting local efforts, in the direction of science.

The Council.

The functions of the British Association do not, however, terminate with the meeting itself. Beside the special committees already mentioned, there remains a very important body, elected by the General Committee, viz., the Council, which assembles at the office in London from time to time as occasion requires. To this body belongs the duty of proposing a president, of preparing for the approval of the General Committee the list of vice-presidents and sectional officers, the selection of evening lecturers, and other arrangements for the coming meeting.

At the present time another class of questions occupies a good deal of the attention of the Council. In the first generation of the Association, and during the period of unwritten, but not yet traditional, law, questions relating to our own organisation or procedure either "settled themselves," or were wisely left to the discretionary powers of those who had taken part in our proceedings during the early years of our existence. These and other kindred subjects now require more careful formularisation and more deliberate sanction. And it is on the shoulders of the Council that the weight of these matters in general falls. These facts deserve especial mention on the present occasion, because one part of our business at the close of this meeting will be to bid farewell officially to one who has served us as assistant secretary so long and so assiduously that he has latterly become our main repertory of information, and our Mentor upon questions of precedent and procedure. The post hitherto held by Mr. Griffith (for it is to him that I allude) will doubtless be well filled by the able and energetic member who has been nominated in his place; but I doubt not that even he will be glad for some time to come to draw largely upon the knowledge and experience of his predecessor.

But, beside matters of internal arrangement and organisation, the duties of the Council comprise a variety of scientific subjects referred to them by the General Committee, at the instance of the Committee of Recommendations, for deliberation and occasionally for action. With the increasing activity of our body in general, and more particularly with that of our various officers, these duties have of late years become more varied and onerous than formerly; nor is it to be wished that they should diminish in either variety or extent.

Once more, questions beyond our own constitution, and even beyond the scope of our own immediate action, such as education, legislation affecting either the promotion or the applications of science to industrial and social life, which have suggested themselves at our meetings, and received the preliminary sanction of our Committee of Recommendations, are frequently referred to our Council. These, and others which it is unnecessary to particularise, whether discussed in full council or in committees specially appointed by that body, render the duties of our councillors as onerous as they are important.

Its Relations with Government.

While the government has at all times, but in a more marked manner of late years, recognised the Royal Society of London, with representatives from the sister societies of Dublin and of Edinburgh, as the body to which it should look for counsel and advice upon scientific questions, it has still never shown itself indisposed to receive and entertain any well-considered recommendation from the British Association. Two special causes have in all probability contributed largely to this result. First, the variety of elements comprised by the Association, on account of which its recommendations imply a more general concurrence of scientific opinion than those of any other scientific body. Secondly, the peculiar fact that our period of maximum activity coincides with that of minimum activity of other scientific bodies is often of the highest importance. At the very time when the other bodies are least able, we are most able, to give deliberate consideration, and formal sanction, to recommendations whether in the form of applications to government or otherwise which may arise. In many of these time is an element so essential, that it is not too much to say that without the intervention of the British Association many opportunities for the advancement of science, especially at the seasons in question, might have been lost. The government has, moreover, formally recognised our scientific existence by appointing our president for the time being a member of the Government Fund Committee; and the public has added its testimony to our importance and utility by imposing upon our president and officers a variety of duties, among which are conspicuous those which arise out of its very liberal exercise of civic and other hospitality.

Presidential Addresses

Of the nature and functions of the presidential address this is perhaps neither the time nor the place to speak; but if I might for a moment forget the purpose for which we are now assembled, I would take the opportunity of reminding those who have not attended many of our former meetings, that our annual volumes contain a long series of addresses on the progress of science, from a number of our most eminent men, to which there is perhaps no parallel elsewhere. These addresses are perhaps as remarkable for their variety in mode of treatment as

in themselves; whereas after the second dimension the third, which is concerned with cubes and dimensions of depth, ought to have been followed.

It is true, Socrates; but these subjects seem to be as yet hardly explored.

Why, yes, I said, and for two reasons; in the first place, no government patronises them, which leads to a want of energy in the study of them, and they are difficult; in the second place, students cannot learn them unless they have a teacher. But then a teacher is hardly to be found, and even if one could be found, as matters now stand the students of these subjects, who are very conceited, would not mind him; that, however, would be otherwise if the whole state patronised and honoured them, then they would listen, and there would be continuous and earnest search, and discoveries would be made; since even now, disregarded as they are by the world, and maimed of their fair proportions, and although none of their votaries can tell the use of them, still these studies force their way by their natural charm, and very likely they may emerge into light.

for the value of their subject-matter. Some of our presidents, and especially those who officiated in the earlier days of our existence, have passed in review the various branches of science, and have noted the progress made in each during the current year. But, as the various sciences have demanded more and more special treatment on the part of those who seriously pursue them, so have the cases of individuals who can of their own knowledge give anything approaching to a general review become more and more rare. To this may be added the fact that although no year is so barren as to fail in affording sufficient crop for a strictly scientific budget, or for a detailed report of progress in research, yet one year is more fertile than another in growths of sufficient prominence to arrest the attention of the general public, and to supply topics suitable for the address. On these accounts, apparently, such a presidential survey has ceased to be annual, and has dropped into an intermittence of longer period. Some presidents have made a scientific principle, such as the time-element in natural phenomena, or continuity, or natural selection, the theme of their discourse, and have gathered illustrations from various branches of knowledge. Others again, taking their own special subject as a fundamental note, and thence modulating into other kindred keys, have borne testimony to the fact that no subject is so special as to be devoid of bearing or of influence on many others. Some have described the successive stages of even a single but important investigation; and while tracing the growth of that particular item, and of the ideas involved in it, have incidentally shown to the outer world what manner of business a serious investigation is. But there is happily no pattern or precedent which the president is bound to follow; both in range of subject-matter and in mode of treatment each has exercised his undoubted right of taking an independent line. And it can hardly be doubted that a judicious exercise of this freedom has contributed more than anything else to sustain the interest of a series of annual discourses extending now over nearly half a century.

The nature of the subjects which may fairly come within the scope of such a discourse has of late been much discussed; and the question is one upon which every one is of course entitled to form his own judgment; but lest there should be any misapprehension as to how far it concerns us in our corporate capacity, it will be well to remind my hearers that as, on the one hand, there is no discussion on the presidential address, and the members as a body express no opinion upon it, so, on the other, the Association cannot fairly be considered as in any way committed to its tenour or conclusions. Whether this immunity from comment and reply be really on the whole so advantageous to the president as might be supposed need not here be discussed, but suffice it to say that the case of an audience assembled to listen without discussion finds a parallel elsewhere, and in the parallel case it is not always considered that the result is altogether either advantageous to the speaker or conducive to excellence in the discourse.

Their Range of Subjects.

But, apart from this, the question of a limitation of range in the subject-matter for the presidential address is not quite so simple as may at first sight appear. It must, in fact, be borne in mind that, while on the one hand knowledge is distinct from opinion, from feeling, and from all other modes of subjective impression, still the limits of knowledge are at all times expanding, and the boundaries of the known and the unknown are never rigid or permanently fixed. That which in time past or present has belonged to one category, may in time future belong to the other. Our ignorance consists partly in ignorance of actual facts, and partly also in ignorance of the possible range of ascertainable fact. If we could lay down beforehand precise limits of possible knowledge, the problem of physical science would be already half solved. But the question to which the scientific explorer has often to address himself is, not merely whether he is able to solve this or that problem, but whether he can so far unravel the tangled threads of the matter with which he has had to deal as to weave them into a definite problem at all. He is not like a candidate at an examination with a precise set of questions placed before him; he must first himself act the part of the examiner and select questions from the repertory of nature, and upon them found others, which in some sense are capable of definite solution. If his eye seem dim, he must look steadfastly and with hope into the misty vision, until the very clouds wreath themselves into definite forms. If his ear seem dull, he must listen patiently and with sympathetic trust to the intricate whisperings of nature—the goddess, as she has been called, of a hundred voices—until here and there he can pick out a few simple notes to which his own powers can resound. If, then, at a moment when he finds himself placed on a pinnacle from which he is called upon to take a perspective survey of the range of science, and to tell us what he can see from his vantage ground; if, at such a moment, after straining his gaze to the very verge of the horizon, and after describing the most distant of well-defined objects, he should give utterance also to some of the subjective impressions which he is conscious of receiving from regions beyond; if he should depict possibilities which seem opening to his view; if he should explain why he thinks this a mere blind alley and that an open path; then the fault and the loss would be alike ours if we refused to listen calmly, and temperately to form our own judgment on what we hear; then assuredly it is we who would be committing the error of confounding matters of fact and matters of opinion, if we failed to discriminate between the various elements contained in such a discourse, and assumed that they had all been put on the same footing.

Presidential Difficulties.

But to whatever decision we may each come on these controverted points, one thing appears clear from a retrospect of past experience; viz., that first or last, either at the outset in his choice of subject or in the conclusions ultimately drawn therefrom, the president, according to his own account at least, finds himself on every occasion in a position of "exceptional, or more than usual difficulty." And your present representative, like his predecessors, feels himself this moment in a similar predicament. The reason which he now offers is that the branch of science which he represents is one whose lines of advance, viewed from a mathematician's own point of view, offer so few points of contact with the ordinary experiences of life or modes of thought, that any account of its actual progress which he might have attempted must have failed in the first requisite of an address, namely, that of being intelligible.

View of Mathematics here taken.

Now if this esoteric view had been the only aspect of the subject which he could present to his hearers, he might well have given up the attempt in despair. But although in its technical character mathematical science suffers the inconveniences, while it enjoys the dignity, of its Olympian position; still in a less formal garb, or in disguise, if you are pleased so to call it, it is found present at many an unexpected turn; and although some of us may never have learnt its special language, not a few have, all through our scientific life, and even in almost every accurate utterance, like Molière's well-known character, been talking mathematics without knowing it. It is, moreover, a fact not to be overlooked that the appearance of isolation, so conspicuous in mathematics, appertains in a greater or less degree to all other sciences, and perhaps also to all pursuits in life. In its highest flight each soars to a distance from its fellows. Each is pursued alone for its own sake, and without reference to its connection with, or its application to, any other subject. The pioneer and the advanced guard are of necessity separated from the main body, and in this respect mathematics does not materially differ from its neighbours. And therefore as the solitariness of mathematics has been a frequent theme of discourse, it may be not altogether unprofitable to dwell for a short time upon the other side of the question, and to inquire whether there be not points of contact in method or in subject-matter between mathematics and the outer world which have been frequently overlooked; whether its lines do not in some cases run parallel to those of other occupations and purposes of life; and lastly, whether we may not hope for some change in the attitude too often assumed towards it by the representatives of other branches of knowledge and of mental activity.

In his Preface to the "Principia," Newton gives expression to some general ideas which may well serve as the key-note for all future utterances on the relation of mathematics to natural, including also therein what are commonly called artificial, phenomena.

Newton's Preface.

"The ancients divided mechanics into two parts, rational and practical; and since artisans often work inaccurately, it came to pass that mechanics and geometry were distinguished in this way—that everything accurate was referred to geometry, and everything inaccurate to mechanics. But the inaccuracies appertain to the artisan and not to the art, and geometry itself has its

foundation in mechanical practice, and is in fact nothing else than that part of universal mechanics which accurately lays down and demostrates the art of measuring."[1] He next explains that rational mechanics is the science of motion resulting from forces, and adds, "The whole difficulty of philosophy seems to me to lie in investigating the forces of nature from the phenomena of motion; and in demonstrating that from these forces other phenomena will ensue." Then, after stating the problems of which he has treated in the work itself, he says: "I would that all other natural phenomena might similarly be deduced from mechanical principles. For many things move me to suspect that everything depends upon certain forces in virtue of which the particles of bodies, through forces not yet understood, are either impelled together so as to cohere in regular figures, or are repelled and recede from one another."

Burrowes' Remarks.

Newton's views, then, are clear; he regards mathematics not as a method independent of, though applicable to, various subjects, but as itself the higher side or aspect of the subjects themselves; and it would be little more than a translation of his notions into other language, little more than a paraphrase of his own words, if we were to describe the mathematical as one aspect of the material world itself, apart from which all other aspects are but incomplete sketches, and however accurate after their own kind, are still liable to the imperfections of the inaccurate artificer. Mr. Burrowes, in his Preface to the first volume of the *Transactions* of the Royal Irish Academy, has carried out the same argument, approaching it from the other side; "No one science," he says, "is so little connected with the rest as not to afford many principles whose use may extend considerably beyond the science to which they primarily belong, and no proposition is so purely theoretical as to be incapable of being applied to practical purposes. There is no apparent connection between duration and the cycloidal arch, the properties of which have furnished us with the best method of measuring time; and he who has made himself master of the nature and affections of the logarithmic curve has advanced considerably towards ascertaining the proportionable density of the air at various distances from the earth. The researches of the mathematician are the only sure ground on which we can reason from experiments; and how far experimental science may assist commercial interests is evinced by the success of manufactures in countries where the hand of the artificer has taken its direction from the philosopher. Every manufacture is in reality but a chemical process, and the machinery requisite for carrying it on but the right application of certain propositions in rational mechanics." So far your academician. Every subject, therefore, whether in its usual acceptation scientific or otherwise, may have a mathematical aspect; as soon, in fact, as it becomes a matter of strict measurement, or of numerical statement, so soon does it enter upon a mathematical phase. This phase may, or it may not, be a prelude to another in which the laws of the subject are expressed in algebraical formulæ or represented by geometrical figures. But the real gist of the business does not always lie in the mode of expression; and the fascination of the formulæ or other mathematical paraphernalia may after all be little more than that of a theatrical transformation scene. The process of reducing to formulæ is really one of abstraction, the results of which are not always wholly on the side of gain; in fact, through the process itself the subject may lose in one respect even more than it gains in another. But long before such abstraction is completely attained, and even in cases where it is never attained at all, a subject may to all intents and purposes become mathematical. It is not so much elaborate calculations or abstruse processes which characterise this phase, as the principles of precision, of exactness, and of proportion. But these are principles with which no true knowledge can entirely dispense. If it be the general scientific spirit which at the outset moves upon the face of the waters, and out of the unknown depth brings forth light and living forms; it is no less the mathematical spirit which breathes the breath of life into what would otherwise have ever remained mere dry bones of fact, which re-unites the scattered limbs and re-creates from them a new and organic whole.

And as a matter of fact, in the words used by Prof. Jellett at our meeting at Belfast, viz., "Not only are we applying our methods to many sciences already recognised as belonging to the legitimate province of mathematics, but we are learning to apply the same instrument to sciences hitherto wholly or partially independent of its authority. Physical science is learning more and more every day to see in the phenomena of nature modifications of that one phenomenon (namely, motion) which is peculiarly under the power of mathematics." Echoes are these, far off and faint perhaps, but still true echoes, in answer to Newton's wish that all these phenomena may some day "be deduced from mechanical principles."

[1] Compare with this the latter part of Plato's "Philebus" on knowledge and the handicraft arts; also Prof. Jowett's Introduction thereto.

Mathematics, Literature, and Art.

If, turning from this aspect of the subject, it were my purpose to enumerate how the same tendency has evinced itself in the arts, unconsciously it may be to the artists themselves, I might call as witnesses each one in turn with full reliance on the testimony which they would bear. And, having more special reference to mathematics, I might confidently point to the accuracy of measurement, to the truth of curve, which, according to modern investigation, is the key to the perfection of classic art. I might triumphantly cite not only the architects of all ages, whose art so manifestly rests upon mathematical principles, but I might cite also the literary as well as the artistic remains of the great artists of the Cinquecento, both painters and sculptors, in evidence of the geometry and the mechanics which, having been laid at the foundation, appear to have found their way upwards through the superstructure of their works.[1] And in a less ambitious sphere, but nearer to ourselves in both time and place, I might point with satisfaction to the great school of English constructors of the eighteenth century in the domestic arts; and remind you that not only the engineer and the architect, but even the cabinet-makers, devoted half the space of their books to perspective and to the principles whereby solid figures may be delineated on paper, or what is now termed descriptive geometry.[2]

Nor perhaps would the sciences which concern themselves with reasoning and speech, nor the kindred art of music, nor even literature itself, if thoroughly probed, offer fewer points of dependence upon the science of which I am speaking. What, in fact, is logic but that part of universal reasoning; grammar but that part of universal speech: harmony and counterpoint but that part of universal music, "which accurately lays down," and demonstrates (so far as demonstration is possible) precise methods appertaining to each of these arts? And I might even appeal to the common consent which speaks of the mathematical as the pattern form of reasoning and model of a precise style.

Taking, then, precision and exactness as the characteristics which distinguish the mathematical phase of a subject, we are naturally led to expect that the approach to such a phase will be indicated by increasing application of the principle of measurement, and by the importance which is attached to numerical results. And this very necessary condition for progress may, I think, be fairly described as one of the main features of scientific advance in the present day.

Measurements in Physics.

If it were my purpose, by descending into the arena of special sciences, to show how the most various investigations alike tend to issue in measurement, and to that extent to assume a mathematical phase, I should be embarrassed by the abundance of instances which might be adduced. I will therefore confine myself to a passing notice of a very few, selecting those which exemplify not only the general tendency, but also the special character of the measurements now particularly required, viz., that of minuteness, and the indirect method by which alone we can at present hope to approach them. An object having a diameter of an 80,000th of an inch is perhaps the smallest of which the microscope could give any well-defined representation; and it is improbable that one of 120,000th of an inch could be singly discerned with the highest powers at our command.[3] But the solar beams and the electric light reveal to us the presence of bodies far smaller than these. And, in the absence of any means of observing them singly, Prof. Tyndall has suggested a scale of these minute objects in terms of the lengths of luminiferous waves. To this he was led, not by any attempt at individual measurement, but by taking account of them in the

[1] See "Trattato della Pittura," by Leonardo da Vinci; also the Memoir on the MSS. of L. d. V., by Venturi, 1797.

[2] "The Gentleman and Cabinet Maker's Director," by Thomas Chippendale. London, 1754. "The Cabinet Maker and Upholsterer's Drawing Book," by Thomas Sheraton. London, 1793.

[3] See Sorby's Address to the Microscopical Society, 1876.

aggregate, and observing the tints which they scatter laterally when clustered in the form of actinic clouds.[1] The small bodies with which experimental science has recently come into contact are not confined to gaseous molecules, but comprise also complete organisms; and the same philosopher has made a profound study of the momentous influence exerted by these minute organisms in the economy of life.[2] And if, in view of their specific effects, whether deleterious or other, on human life, any qualitative classification, or quantitative estimate be ever possible, it seems that it must be effected by some such method as that indicated above.

Again, to enumerate a few more instances of the measurement of minute quantities, there are the average distances of molecules from one another in various gases and at various pressures; the length of their free path, or range open for their motion without coming into collision; there are movements causing the pressures and differences of pressure under which Mr. Crookes' radiometers execute their wonderful revolutions.[3] There are the excursions of the air while transmitting notes of high pitch, which through the researches of Lord Rayleigh appear to be of a diminutiveness altogether unexpected.[4] There are the molecular actions brought into play in the remarkable experiments by Dr. Ker,[5] who has succeeded, where even Faraday failed, in effecting a visible rotation of the plane of polarisation of light in its passage through electrified dielectrics, and on its reflection at the surface of a magnet. To take one more instance, which must be present to the minds of us all, there are the infinitesimal ripples of the vibrating plate in Mr. Graham Bell's most marvellous invention. Of the nodes and ventral segments in the plate of the telephone which actually converts sound into electricity and electricity into sound, we can at present form no conception. All that can now be said is that the most perfect specimens of Chladni's sand figures on a vibrating plate, or of Kundt's lycopodium heaps in a musical tube,[6] or even Mr. Sedley Taylor's more delicate vortices in the films of the phoneidoscope,[7] are rough and sketchy compared with these. For notwithstanding the fact that in the movements of the telephone-plate we have actually in our hand the solution of that old world-problem the construction of a speaking-machine, yet the characters in which that solution is expressed are too small for our powers of decipherment. In movements such as these we seem to lose sight of the distinction, or perhaps we have unconsciously passed the boundary between massive and molecular motion.[8]

Through the phonograph [9] we have not only a transformation, but a permanent and tangible record of the mechanism of speech. But the differences upon which articulation (apart from loudness, pitch, and quality) depends, appear from the experiments of Fleeming Jenkin and of others, to be of microscopic size. The microphone affords another instance [10] of the unexpected value of minute variations,—in this case of electric currents; and it is remarkable that the gist of the instrument seems to lie in obtaining and perfecting that which electricians have hitherto most scrupulously avoided, viz., loose contact.

Once more, Mr. De La Rue has brought forward as one of the results [11] derived from his stupendous battery of 10,000 cells, strong evidence for supposing that a voltaic discharge, even when apparently continuous, may still be an intermittent phenomenon; but all that is known of the period of such intermittence is, that it must recur at exceedingly short intervals. And in connection with this subject, it may be added that, whatever be the ultimate explanation of the strange stratification which the voltaic discharge undergoes in rarefied gases, it is clear that the alternate disposition of light and darkness must be dependent on some periodic distribution in space or sequence in time which can at present be dealt with only in a very general way. In the exhausted column we have a vehicle for electricity not constant like an ordinary conductor, but itself modified by the passage of the discharge, and perhaps subject to laws differing materially from those which it obeys at atmospheric pressure. It may also be that some of the features accompanying stratification form a magnified image of phenomena belonging to disruptive discharges in general; and that consequently so far from expecting among the known facts of the latter any clue to an explanation of the former, we must hope ultimately to find in the former an elucidation of what is at present obscure in the latter. A prudent philosopher usually avoids hazarding any forecast of the practical application of a purely scientific research. But it would seem that the configuration of these striæ might some day prove a very delicate means of estimating low pressures, and perhaps also for effecting some electrical measurements.

Now, it is a curious fact that almost the only small quantities of which we have as yet any actual measurements are the wave-lengths of light; and that all others, excepting so far as they can be deduced from these, await future determination. In the mean time, when unable to approach these small quantities individually, the method to which we are obliged to have recourse is, as indicated above, that of averages, whereby, disregarding the circumstances of each particular case, we calculate the average size, the average velocity, the average direction, &c., of a large number of instances.[1] But although this method is based upon experience, and leads to results which may be accepted as substantially true; although it may be applicable to any finite interval of time, or over any finite area of space (that is, for all practical purposes of life) there is no evidence to show that it is so when the dimensions of interval or of area are indefinitely diminished. The truth is that the simplicity of nature which we at present grasp is really the result of infinite complexity; and that below the uniformity there underlies a diversity whose depths we have not yet probed, and whose secret places are still beyond our reach.

The present is not an occasion for multiplying illustrations, but I can hardly omit a passing allusion to one all-important instance of the application of the statistical method. Without its aid social life, or the History of Life and Death, could not be conceived at all, or only in the most superficial manner. Without it we could never attain to any clear ideas of the condition of the poor, we could never hope for any solid amelioration of their condition or prospects. Without its aid, sanitary measures, and even medicine, would be powerless. Without it, the politician and the philanthropist would alike be wandering over a trackless desert.

Mathematical Methods and other Subjects.

It is, however, not so much from the side of science at large as from that of mathematics itself, that I desire to speak. I wish from the latter point of view to indicate connections between mathematics and other subjects, to prove that hers is not after all such a far-off region, nor so undecipherable an alphabet, and to show that even at unlikely spots we may trace under-currents of thought which, having issued from a common source, fertilise alike the mathematical and the non-mathematical world.

Having this in view, I propose to make the subject of special remark some processes peculiar to modern mathematics; and, partly with the object of incidentally removing some current misapprehensions, I have selected for examination three methods in respect of which mathematicians are often thought to have exceeded all reasonable limits of speculation, and to have adopted for unknown purposes an unknown tongue.

And it will be my endeavour to show not only that in these very cases our science has not outstepped its own legitimate range, but that even art and literature have unconsciously employed methods similar in principle. The three methods in question are first, that of imaginary quantities; secondly that of manifold space, and thirdly, that of geometry not according to Euclid.

Imaginaries.

First it is objected that, abandoning the more cautious methods of ancient mathematicians, we have admitted into our formulæ quantities which by our own showing, and even in our own nomenclature, are imaginary or impossible; nay, more, that out of them we have formed a variety of new algebras to which there is no counterpart whatever in reality, but from which we claim to arrive at possible and certain results.

On this head it is in Dublin, if anywhere, that I may be per-

[1] *Phil. Trans.* of the Royal Society, 1870, p. 333; and 1876, p. 27.
[2] *Phil. Trans.* 1877, p. 149.
[3] "On Attraction and Repulsion Resulting from Radiation," *Phil. Trans.* 1874, p. 501; 1875, p. 519; 1876, p. 325.
[4] *Philosophical Magazine*, April, 1878.
[5] *Philosophical Magazine*, 1875, vol. ii. pp. 337, 446; 1877, vol. i. p. 321; 1878, vol. i. p. 161.
[6] *Poggendorff's Annalen*, tom. xxxv. p. 337.
[7] Royal Society's *Proceedings*, 1878.
[8] The papers on the telephone are too numerous to specify.
[9] See various papers in NATURE, and elsewhere, during the last twelve months.
[10] Royal Society's *Proceedings*, May 9, 1878.
[11] *Phil. Trans.* vol. clxix. pp. 55 and 155, and other papers catalogued in the Appendix to Part II. of the Memoir.

[1] See Maxwell "On Heat," chap. xxii.

mitted to speak. For to the fertile imagination of the late Astronomer-Royal for Ireland we are indebted for that marvellous calculus of quaternions, which is only now beginning to be fully understood, and which has not yet received all the applications of which it is doubtless capable. And even although this calculus be not coextensive with another (the Ausdehnungslehre of Grassmann)[1] which almost simultaneously germinated on the Continent, nor with ideas more recently developed in America (Pierce's "Linear Associative Algebras");[2] yet it must always hold its position as an original discovery and as a representative of one of the two great groups of generalised algebras (viz., those the squares of whose units are respectively negative unity and zero) the common origin of which must still be marked on our intellectual map as an unknown region. Well do I recollect how in its early days we used to handle the method as a magician's page might try to wield his master's wand, trembling as it were between hope and fear, and hardly knowing whether to trust our own results until they had been submitted to the present and ever ready counsel of Sir W. R. Hamilton himself.

To fix our ideas, consider the measurement of a line, or the reckoning of time, or the performance of any mathematical operation. A line may be measured in one direction or in the opposite; time may be reckoned forward or backward; an operation may be performed or be reversed, it may be done or may be undone; and if having once reversed any of these processes we reverse it a second time, we shall find that we have come back to the original direction of measurement or reckoning, or to the original kind of operation.

Suppose, however, that at some stage of a calculation our formulæ indicate an alteration in the mode of measuremen such that if the alteration be repeated, a condition of things ntot the same as, but the reverse of the original, will be produced. Or suppose that, at a certain stage, our transformations indicate that time is to be reckoned in some manner different from future or past, but still in a way having definite algebraical connection with time which is gone and time which is to come.[3] It is clear that in actual experience there is no process to which such measurements correspond. Time has no meaning except as future or past; and the present is but the meeting point of the two. Or, once more, suppose that we are gravely told that all circles pass through the same two imaginary points at an infinite distance, and that every line drawn through one of these points is perpendicular to itself. On hearing the statement we shall probably whisper, with a smile or a sigh, that we hope it is not true, but that in any case it is a long way off, and perhaps, after all, it does not very much signify. If, however, we are not satisfied to dismiss the question on these terms, the mathematician himself must admit that we have here reached a definite point of issue. Our science must either give a rational account of the dilemma, or yield the position as no longer tenable.

Explanation of them.

Special modes of explaining this anomalous state of things have occurred to mathematicians. But, omitting details as unsuited to the present occasion, it will, I think, be sufficient to point out in general terms that a solution of the difficulty is to be found in the fact that the formulæ which give rise to these results are more comprehensive than the signification which has been given to them; and when we pass out of the condition of things first contemplated they cannot (as it is obvious they ought not) give us any results intelligible on that basis. But it does not therefore by any means follow that upon a more enlarged basis the formulæ are incapable of interpretation; on the contrary, the difficulty at which we have arrived indicates that there must be some more comprehensive statement of the problem which will include cases impossible in the more limited, but possible in the wider view of the subject.

[1] Grunert's Archiv., vol. vi. p. 337; also separate work, Berlin, 1862.

[2] "Linear Associative Algebra," by Benjamin Pierce, Washington City, 1870.

[3] Sir W. Thomson, *Cambridge Mathematical Journal*, vol. iii. p. 174. Jevons' "Principles of Science," vol. ii. p. 438. But an explanation of the difficulty seems to me to be found in the fact that the problem, as stated, is one of the conduction of heat, and that the "impossibility" which attaches itself to the expression for the "time" merely means that previous to a certain epoch the conditions which gave rise to the phenomena were not those of conduction, but those of some other action of heat. If, therefore, we desire to comprise the phenomena of the earlier as well as of the later period in one problem, we must find some more general statement; viz., that of physical conditions which at the critical epoch will issue in a case of conduction. I think that Prof. Clifford has somewhere given a similar explanation.

A very simple instance will illustrate the matter. If from a point outside a circle we draw a straight line to touch the curve, the distance between the starting point and the point of contact has certain geometrical properties. If the starting point be shifted nearer and nearer to the circle the distance in question becomes shorter, and ultimately vanishes. But as soon as the point passes to the interior of the circle the notion of a tangent and distance to the point of contact cease to have any meaning; and the same anomalous condition of things prevails as long as the point remains in the interior. But if the point be shifted still further until it emerges on the other side, the tangent and its properties resume their reality; and are as intelligible as before. Now the process whereby we have passed from the possible to the impossible, and again repassed to the possible (namely the shifting of the starting point) is a perfectly continuous one, while the conditions of the problem as stated above have abruptly changed. If, however, we replace the idea of a line touching by that of a line cutting the circle, and the distance of the point of contact by the distances at which the line is intercepted by the curve, it will easily be seen that the latter includes the former as a limiting case, when the cutting line is turned about the starting point until it coincides with the tangent itself. And further, that the two intercepts have a perfectly distinct and intelligible meaning whether the point be outside or inside the area. The only difference is that in the first case the intercepts are measured in the same direction; in the latter in opposite directions.

The foregoing instance has shown one purpose which these imaginaries may serve, viz., as marks indicating a limit to a particular condition of things, to the application of a particular law, or pointing out a stage where a more comprehensive law is required. To attain to such a law we must, as in the instance of the circle and tangent, reconsider our statement of the problem; we must go back to the principle from which we set out, and ascertain whether it may not be modified or enlarged. And even if in any particular investigation, wherein imaginaries have occurred, the most comprehensive statement of the problem of which we are at present capable fails to give an actual representation of these quantities; if they must for the present be relegated to the category of imaginaries; it still does not follow that we may not at some future time find a law which will endow them with reality, nor that in the meantime we need hesitate to employ them, in accordance with the great principle of continuity, for bringing out correct results.

Illustration from Art and Literature.

If, moreover, both in geometry and in algebra we occasionally make use of points or of quantities which from our present outlook have no real existence, which can neither be delineated in space of which we have experience, nor measured by scale as we count measurement; if these imaginaries, as they are termed, are called up by legitimate processes of our science; if they serve the purpose not merely of suggesting ideas, but of actually conducting us to practical conclusions; if all this be true in abstract science, I may perhaps be allowed to point out, at all events in illustration, that in art unreal forms are frequently used for suggesting ideas, for conveying a meaning for which no others seem to be suitable or adequate. Are not forms unknown to biology, situations incompatible with gravitation, positions which challenge not merely the stability but the very possibility of equilibrium—are not these the very means to which the artist often has recourse in order to convey his meaning and to fulfil his mission? Who that has ever revelled in the ornamentation of the Renaissance, in the extraordinary transitions from the animal to the vegetable, from faunic to floral forms, and from these again to almost purely geometric curves, who has not felt that these imaginaries have a claim to recognition very similar to that of their congeners in mathematics? How is it that the grotesque paintings of the middle ages, the fantastic sculpture of remote nations, and even the rude art of the prehistoric past, still impress us, and have an interest over and above their antiquarian value; unless it be that they are symbols which, although hard of interpretation when taken alone, are yet capable from a more comprehensive point of view of leading us mentally to something beyond themselves, and to truths which, although reached through them, have a reality scarcely to be attributed to their outward forms?

Again, if we turn from art to letters, truth to nature and to fact is undoubtedly a characteristic of sterling literature; and yet in the delineation of outward nature itself, still more in that

of feelings and affections, of the secret springs of character and motives of conduct, it frequently happens that the writer is driven to imagery, to an analogy, or even to a paradox, in order to give utterance to that of which there is no direct counterpart in recognised speech. And yet which of us cannot find a meaning for these literary figures, an inward response, to imaginative poetry, to social fiction, or even to those tales of giant and fairyland written, it is supposed, only for the nursery or schoolroom? But in order thus to reanimate these things with a meaning beyond that of the mere words, have we not to reconsider our first position, to enlarge the ideas with which we started; have we not to cast about for something which is common to the idea conveyed and to the subject actually described, and to seek for the sympathetic spring which underlies both; have we not, like the mathematician, to go back as it were to some first principles, or as it is pleasanter to describe it, to become again as a little child?

Manifold Space.

Passing to the second of the three methods, viz., that of manifold space, it may first be remarked that our whole experience of space is in three dimensions, viz., of that which has length, breadth, and thickness; and if for certain purposes we restrict our ideas to two dimensions as in plane geometry, or to one dimension as in the division of a straight line, we do this only by consciously and of deliberate purpose setting aside, but not annihilating, the remaining one or two dimensions. Negation, as Hegel has justly remarked, implies that which is negatived, or, as he expresses it, affirms the opposite. It is by abstraction from previous experience, by a limitation of its results, and not by any independent process, that we arrive at the idea of space whose dimensions are less than three.

It is doubtless on this account that problems in plane geometry which, although capable of solution on their own account, become much more intelligible, more easy of extension, if viewed in connection with solid space, and as special cases of corresponding problems in solid geometry. So eminently is this the case that the very language of the more general method often leads us almost intuitively to conclusions which from the more restricted point of view require long and laborious proof. Such a change in the base of operations has, in fact, been successfully made in geometry of two dimensions, and although we have not the same experimental data for the further steps, yet neither the modes of reasoning, nor the validity of its conclusions, are in any way affected by applying an analogous mental process to geometry of three dimensions, and by regarding figures in space of three dimensions as sections of figures in space of four in the same way that figures in plano are sometimes considered as sections of figures in solid space. The addition of a fourth dimension to space not only extends the actual properties of geometrical figures, but it also adds new properties which are often useful for the purposes of transformation or of proof. Thus it has recently been shown that in four dimensions a closed material shell could be turned inside out[1] by simple flexure, without either stretching or tearing,[2] and that in such a space it is impossible to tie a knot.

Again, the solution of problems in geometry is often effected by means of algebra: and as three measurements, or co-ordinates as they are called, determine the position of a point in space, so do three letters or measurable quantities serve for the same purpose in the language of algebra. Now many algebraical problems involving three unknown or variable quantities admit of being generalised so as to give problems involving many such quantities. And as, on the one hand, to every algebraical problem involving unknown quantities or variables by ones, or by twos, or by threes, there corresponds a problem in geometry of one or of two or of three dimensions, so on the other it may be said that to every algebraical problem involving many variables there corresponds a problem in geometry of many dimensions.

There is, however, another aspect under which even ordinary space presents to us a four-fold, or indeed a manifold character. In modern physics space is not regarded as a vacuum in which bodies are placed and forces have play, but rather as a plenum with which matter is co-extensive. And from a physical point of view the properties of space are the properties of matter, or of the medium which fills it. Similarly from a mathematical point of view, space may be regarded as a *locus in quo*, as a plenum, filled with those elements of geometrical magnitude which we take as fundamental. These elements need not always be the same. For different purposes different elements may be chosen; and upon the degree of complexity of the subject of our choice will depend the internal structure or manifoldness of space.

Thus, beginning with the simplest case, a point may have any singly infinite multitude of positions in a line, which gives a one-fold system of points in a line. The line may revolve in a plane about any one of its points, giving a two-fold system of points in a plane; and the plane may revolve about any one of the lines, giving a three-fold system of points in space.

Suppose, however, that we take a straight line as our element, and conceive space as filled with such lines. This will be the case if we take two planes, *e.g.*, two parallel planes, and join every point in one with every point in the other. Now the points in a plane form a two-fold system, and it therefore follows that the system of lines is four-fold; in other words, space regarded as a plenum of lines is four-fold. The same result follows from the consideration that the lines in a plane, and the planes through a point, are each two-fold.

Again, if we take a sphere as our element we can through any point as a centre draw a singly infinite number of spheres, but the number of such centres is triply infinite; hence space as a plenum of spheres is four-fold. And generally, space as a plenum of surfaces has a manifoldness equal to the number of constants required to determine the surface. Although it would be beyond our present purpose to attempt to pursue the subject further, it should not pass unnoticed that the identity in the four-fold character of space, as derived on the one hand from a system of straight lines, and on the other from a system of spheres, is intimately connected with the principles established by Sophus Lie in his researches on the correlation of these figures.

If we take a circle as our element we can around any point in a plane as a centre draw a singly infinite system of circles; but the number of such centres in a plane is doubly infinite; hence the circles in a plane form a three-fold system, and as the planes in space form a three-fold system, it follows that space as a plenum of circles is six-fold.

Again, if we take a circle as our element, we may regard it as a section either of a sphere, or of a right cone (given except in position) by a plane perpendicular to the axis. In the former case the position of the centre is three-fold; the directions of the plane, like that of a pencil of lines perpendicular thereto, two-fold; and the radius of the sphere one-fold; six-fold in all. In the latter case, the position of the vertex is three-fold; the direction of the axis two-fold; and the distance of the plane of section one-fold; six-fold in all, as before. Hence space as a plenum of circles is six-fold.

Similarly, if we take a conic as our element we may regard it as a section of a right cone (given except in position) by a plane. If the nature of the conic be defined, the plane of section will be inclined at a fixed angle to the axis; otherwise it will be free to take any inclination whatever. This being so, the position of the vertex will be three-fold, the direction of the axis two-fold, the distance of the plane of section from the vertex one-fold, and the direction of that plane one-fold if the conic be defined, two-fold if it be not defined. Hence, space as a plenum of definite conics will be seven-fold, as a plenum of conics in general, eight-fold. And so on for curves of higher degrees.

This is, in fact, the whole story and mystery of manifold space. If not seriously regarded as a reality in the same sense as ordinary space, it is a mode of representation, or a method which, having served its purpose, vanishes from the scene. Like a rainbow, if we try to grasp it, it eludes our very touch; but, like a rainbow, it arises out of real conditions of known and tangible quantities, and if rightly apprehended it is a true and valuable expression of natural laws, and serves a definite purpose in the science of which it forms part.

Illustrations.

Again, if we seek a counterpart of this in common life, I might remind you that perspective in drawing is itself a method not altogether dissimilar to that of which I have been speaking; and that the third dimension of space, as represented in a picture, has its origin in the painter's mind, and is due to his skill, but has no real existence upon the canvas which is the groundwork of his art. Or again, turning to literature, when in legendary tales, or in works of fiction, things past and future are pictured as present, has not the poetic fancy brought time into correlation with the three dimensions of space, and brought all alike to a

[1] S. Newcomb "On Certain Transformations of Surfaces," *American Journal of Mathematics*, vol. i. p. 1.

[2] Tait "On Knots," *Transactions* of the Royal Society of Edinburgh, vol. xxviii. p. 145. Klein, *Mathematische Annalen*, ix. p. 478.

common focus? Or once more, when space already filled with material substances is mentally peopled with immaterial beings, may not the imagination be regarded as having added a new element to the capacity of space, a fourth dimension of which there is no evidence in experimental fact?

Non-Euclid Geometry.

The third method proposed for special remark is that which has been termed non-Euclidean geometry, and the train of reasoning which has led to it may be described in general terms as follows: some of the properties of space which on account of their simplicity, theoretical as well as practical, have, in constructing the ordinary system of geometry, been considered as fundamental, are now seen to be particular cases of more general properties. Thus a plane surface and a straight line may be regarded as special instances of surfaces and lines whose curvature is everywhere uniform or constant. And it is, perhaps, not difficult to see that, when the special notions of flatness and straightness are abandoned, many properties of geometrical figures which we are in the habit of regarding as fundamental, will undergo profound modification. Thus a plane may be considered as a special case of the sphere, viz., the limit to which a sphere approaches when its radius is increased without limit. But even this consideration trenches upon an elementary proposition relating to one of the simplest of geometrical figures. In plane triangles the interior angles are together equal to two right angles; but in triangles traced on the surface of a sphere, this proposition does not hold good. To this, other instances might be added.

Further, these modifications may affect not only our ideas of particular geometrical figures, but the very axioms of the science itself. Thus, the idea which, in fact, lies at the foundation of Euclid's method that a geometrical figure may be moved in space without change of size or alteration of form, entirely falls away, or becomes only approximate in a space wherein dimension and form are dependent upon position. For instance, if we consider merely the case of figures traced on a flattened globe like the earth's surface, or upon an egg-shell, such figures cannot be made to slide upon the surface without change of form, as is the case with figures traced upon a plane or even upon a sphere. But, further still, these generalisations are not restricted to the case of figures traced upon a surface; they may apply also to solid figures in a space whose very configuration varies from point to point. We may, for instance, imagine a space in which our rule or scale of measurement varies as it extends, or as it moves about, in one direction or another; a space, in fact, whose geometric density is not uniformly distributed. Thus we might picture to ourselves such a space as a field having a more or less complicated distribution of temperature, and our scale as a rod instantaneously susceptible of expansion or contraction under the influence of heat; or we might suppose space to be even crystalline in its geometric formation, and our scale and measuring instruments to accept the structure of the locality in which they are applied. These ideas are doubtless difficult of apprehension, at all events at the outset; but Helmholtz has pointed out a very familiar phenomenon which may be regarded as a diagram of such a kind of space. The picture formed by reflection from a plane mirror may be taken as a correct representation of ordinary space, in which, subject to the usual laws of perspective, every object appears in the same form and of the same dimensions, whatever be its position. In like manner the picture formed by reflexion from a curved mirror may be regarded as the representation of a space wherein dimension and form are dependent upon position. Thus in an ordinary convex mirror objects appear smaller as they recede laterally from the centre of the picture; straight lines become curved; objects infinitely distant in front of the mirror appear at a distance only equal to the focal length behind. And by suitable modifications in the curvature of the mirror, representations could similarly be obtained of space of various configurations.

Its Meaning and Use.

The diversity in kind of these spaces is of course infinite; they vary with the mode in which we generalise our conceptions of ordinary space; but upon each as a basis it is possible to construct a consistent system of geometry, whose laws, as a matter of strict reasoning, have a validity and truth not inferior to those with which we are habitually familiar. Such systems having been actually constructed, the question has not unnaturally been asked, whether there is anything in nature or in the outer world to which they correspond; whether, admitting that for our limited experience ordinary geometry amply suffices, we may understand that for powers more extensive in range or more minute in definition some more general scheme would be requisite? Thus, for example, although the one may serve for the solar system, is it legitimate to suppose that it may fail to apply at distances reaching to the fixed stars, or to regions beyond? Or again, if our vision could discern the minute configuration of portions of space, which to our ordinary powers appear infinitesimally small, should we expect to find that all our usual geometry is but a special case, sufficient indeed for daily use, but after all only a rough approximation to a truer although perhaps more complicated scheme? Traces of these questions are in fact to be found in the writings of some of our greatest and most original mathematicians. Gauss, Riemann, and Helmholtz have thrown out suggestions radiating, as it were, in these various directions from a common centre; while Cayley, Sylvester, and Clifford in this country, Klein in Germany, Lobatscheffsky in Russia, Bolyai in Hungary, and Beltrami in Italy, with many others, have reflected similar ideas with all the modifications due to the chromatic dispersion or their individual minds. But to the main question the answer must be in the negative. And, to use the words of Newton, since "geometry has its foundation in mechanical practice," the same must be the answer until our experience is different from what it now is. And yet, all this notwithstanding, the generalised conceptions of space are not without their practical utility. The principle of representing space of one kind by that of another, and figures belonging to one by their analogues in the other, is not only recognised as legitimate in pure mathematics, but has long ago found its application in cartography. In maps or charts, geographical positions, the contour of coasts, and other features, belonging in reality to the earth's surface, are represented on the flat; and to each mode of representation, or projection as it is called, there corresponds a special correlation between the spheroid and the plane. To this might perhaps be added the method of descriptive geometry, and all similar processes in use by engineers, both military and civil.

It has often been asked whether modern research in the field of pure mathematics has not so completely outstripped its physical applications as to be practically useless; whether the analyst and the geometer might not now, and for a long time to come, fairly say, "hic artem remumque repono," and turn his attention to mechanics and to physics. That the Pure has outstripped the Applied is largely true; but that the former is on that account useless is far from true. Its utility often crops up at unexpected points: witness the aids to classification of physical quantities, furnished by the ideas (of Scalar and Vector) involved in the "Calculus of Quaternions;" or the advantages which have accrued to physical astronomy from Lagrange's "Equations," and from Hamilton's "Principle of Varying Action;" on the value of complex quantities, and the properties of general integrals, and of general theorems on integration for the theories of electricity and magnetism. The utility of such researches can in no case be discounted, or even imagined beforehand; who, for instance, would have supposed that the calculus of forms or the theory of substitutions would have thrown much light upon ordinary equations; or that Abolian functions and hyperelliptic transcendents would have told us anything about the properties of curves; or that the calculus of operations would have helped us in any way towards the figure of the earth? But upon such technical points I must not now dwell. If, however, as I hope, it has been sufficiently shown that any of these more extended ideas enable us to combine together, and to deal with as one, properties and processes which from the ordinary point of view present marked distinctions, then they will have justified their own existence; and in using them we shall not have been walking in a vain shadow, nor disquieting our brains in vain.

Mathematical Symmetry.

These extensions of mathematical ideas would, however, be overwhelming, if they were not compensated by some simplifications in the processes actually employed. Of these aids to calculation I will mention only two, viz., symmetry of form and mechanical appliances; or, say, mathematics as a fine art, and mathematics as a handicraft. And first, as to symmetry of form. There are many passages of algebra in which long processes of calculation at the outset seem unavoidable. Results are often obtained in the first instance through a tangled maze of

formulæ, where at best we can just make sure of their process step by step, without any general survey of the path which we have traversed, and still less of that which we have to pursue. But almost within our own generation a new method has been devised to clear this entanglement. More correctly speaking, the method is not new, for it is inherent in the processes of algebra itself, and instances of it, unnoticed perhaps or disregarded, are to be found cropping up throughout nearly all mathematical treatises. By Lagrange, and to some extent also by Gauss, among the older writers, the method of which I am speaking was recognised as a principle; but beside these perhaps no others can be named until a period within our own recollection. The method consists in symmetry of expression. In algebraical formulæ combinations of the quantities entering therein occur and recur; and by a suitable choice of these quantities the various combinations may be rendered symmetrical and reduced to a few well-known types. This having been done, and one such combination having been calculated, the remainder, together with many of their results, can often be written down at once, without further calculations, by simple permutations of the letters. Symmetrical expressions, moreover, save as much time and trouble in reading as in writing. Instead of wading laboriously through a series of expressions which, although successively dependent, bear no outward resemblance to one another, we may read off symmetrical formulæ, of almost any length, at a glance. A page of such formulæ becomes a picture: known forms are seen in definite groupings; their relative positions, or perspective as it may be called, their very light and shadow, convey their meaning almost as much through the artistic faculty as through any conscious ratiocinative process. Few principles have been more suggestive of extended ideas or of new views and relation than that of which I am now speaking. In order to pass from questions concerning plane figures to those which appertain to space, from conditions having few degrees of freedom to others which have many—in a word, from more restricted to less restricted problems—we have in many cases merely to add lines and columns to our array of letters or symbols already formed, and then read off pictorially the extended theorems.

Mechanical Methods.

Next as to mechanical appliances. Mr. Babbage, when speaking of the difficulty of insuring accuracy in the long numerical calculations of theoretical astronomy, remarked that the science which in itself is the most accurate and certain of all had, through these difficulties, become inaccurate and uncertain in some of its results. And it was doubtless some such consideration as this, coupled with his dislike of employing skilled labour where unskilled labour would suffice, which led him to the invention of his calculating machines. The idea of substituting mechanical for intellectual power has not lain dormant; for beside the arithmetical machines, whose name is legion (from Napier's Bones, Earl Stanhope's calculator, to Schultz's and Thomas's machines now in actual use), an invention has lately been designed for even a more difficult task.[1] Prof. James Thomson has in fact recently constructed a machine which, by means of the mere friction of a disc, a cylinder, and a ball, is capable of effecting a variety of the complicated calculations which occur in the highest application of mathematics to physical problems. By its aid it seems that an unskilled labourer may, in a given time, perform the work of ten skilled arithmeticians. The machine is applicable alike to the calculation of tidal, of magnetic, of meteorological, and perhaps also of all other periodic phenomena. It will solve differential equations of the second and perhaps of even higher orders. And through the same invention the problem of finding the free motions of any number of mutually attracting particles, unrestricted by any of the approximate suppositions required in the treatment of the lunar and planetary theories, is reduced to the simple process of turning a handle.

When Faraday had completed the experimental part of a physical problem, and desired that it should thenceforward be treated mathematically, he used irreverently to say, "Hand it over to the calculators." But truth is ever stranger than fiction; and if he had lived until our day, he might with perfect propriety have said, "Hand it over to the machine."

Mathematics and Observation.

Had time permitted, the foregoing topics would have led me to point out that the mathematician, although concerned only with abstractions, uses many of the same methods of research as are employed in other sciences, and in the arts, such as observation, experiment, induction, imagination. But this is the less necessary because the subject has been already handled very ably, although with greater brevity than might have been wished, by Prof. Sylvester in his address to Section A at our meeting at Exeter.

Origin of Mathematical Ideas.

In an exhaustive treatment of my subject there would still remain a question which in one sense lies at the bottom of all others, and which through almost all time has had an attraction for reflective minds, viz., what was the origin of mathematical ideas? Are they to be regarded as independent of, or dependent upon, experience? The question has been answered sometimes in one way and sometimes in another. But the absence of any satisfactory conclusion may after all be understood as implying that no answer is possible in the sense in which the question is put; or rather that there is no question at all in the matter, except as to the history of actual facts. And, even if we distinguish, as we certainly should, between the origin of ideas in the individual and their origin in a nation or mankind, we should still come to the same conclusion. If we take the case of the individual, all we can do is to give an account of our own experience; how we played with marbles and apples; how we learnt the multiplication table, fractions, and proportion; how we were afterwards amused to find that common things conformed to the rules of number; and later still how we came to see that the same laws applied to music and to mechanism, to astronomy, to chemistry, and to many other subjects. And then, on trying to analyse our own mental processes, we find that mathematical ideas have been imbibed in precisely the same way as all other ideas, viz., by learning, by experience, and by reflection. The apparent difference in the mode of first apprehending them and in their ultimate cogency arises from the difference of the ideas themselves, from the preponderance of quantitative over qualitative considerations in mathematics, from the notions of absolute equality and identity which they imply.

If we turn to the other question, How did the world at large acquire and improve its idea of number and of figures? How can we span the interval between the savage who counted only by the help of outward objects, to whom fifteen was "half the hands and both the feet," and Newton or Laplace? The answer is the history of mathematics and its successive developments, arithmetic, geometry, algebra, &c. The first and greatest step in all this was the transition from number in the concrete to number in the abstract. This was the beginning not only of mathematics but of all abstract thought. The reason and mode of it was the same as in the individual. There was the same general influx of evidence, the same unsought for experimental proof, the same recognition of general laws running through all manner of purposes and relations of life. No wonder then if, under such circumstances, mathematics, like some other subjects, and perhaps with better excuse, came after a time to be clothed with mysticism; nor that, even in modern times, they should have been placed upon an *à priori* basis as in the philosophy of Kant.

Their Survival and Transition.

Number was so soon found to be a principle common to many branches of knowledge that it was readily assumed to be the key to all. It gave distinctness of expression, if not clearness of thought, to ideas which were floating in the untutored mind, and even suggested to it new conceptions. In "the one" "the all," "the many in one" (terms of purely arithmetic origin), it gave the earliest utterance to men's first crude notions about God and the world. In "the equal," "the solid," "the straight," and "the crooked," which still survive as figures of speech among ourselves, it supplied a vocabulary for the moral notions of mankind, and quickened them by giving them the power of expression. In this lies the great and enduring interest in the fragments which remain to us of the Pythagorean philosophy.

The consecutive processes of mathematics led to the consecutive processes of logic, but it was not until long after mankind had attained to abstract ideas that they attained to any clear notion of their connection with one another. The leading ideas of mathematics became the leading ideas of logic. The "one" and the "many" passed into the "whole" and its "parts;" and thence into the "universal" and the "particular." The fallacies of logic, such as the well-known puzzle of Achilles and the tortoise, partake of the nature of both sciences. And perhaps the conception of the infinite and the infinitesimal, as

[1] Royal Society's *Proceedings*, February 3, 1876, and May 9, 1878.

well as of negation, may have been in early times transferred from logic to mathematics. But the connection of our ideas of number is probably anterior to the connection of any of our other ideas. And as a matter of fact, geometry and arithmetic had already made considerable progress when Aristotle invented the syllogism.

General Ideas.

General ideas there were beside those of mathematics—true flashes of genius which saw that there must be general laws to which the universe conforms, but which saw them only by occasional glimpses, and through the distortion of imperfect knowledge; and although the only records of them now remaining are the inadequate representations of later writers, yet we must still remember that to the existence of such ideas is due not only the conception but even the possibility of physical science. But these general ideas were too wide in their grasp, and in early days at least were connected to their subjects of application by links too shadowy to be thoroughly apprehended by most minds, and so it came to pass that one form of such an idea was taken as its only form, one application of it as the idea itself; and philosophy, unable to maintain itself at the level of ideas, fell back upon the abstractions of sense, and, by preference, upon those which were most ready to hand, namely, those of mathematics. Plato's ideas relapsed into a doctrine of numbers; mathematics into mysticism, into Neo-Platonism, and the like. And so, through many long ages, through good report and evil report, mathematics have always held an unsought-for sway. It has happened to this science, as to many other subjects, that its warmest adherents have not always been its best friends. Mathematics have often been brought in to matters where their presence has been of doubtful utility. If they have given precision to literary style, that precision has sometimes been carried to excess, as in Spinoza and perhaps Descartes; if they have tended to clearness of expression in philosophy, that very clearness has sometimes given an appearance of finality not always true;[1] if they have contributed to definition in theology, that definitiveness has often been fictitious, and has been attained at the cost of spiritual meaning.[2] And, coming to recent times, although we may admire the ingenuity displayed in the logical machines of Earl Stanhope and of Stanley Jevons, in the formal logic of De Morgan, and in the calculus of Boole; although as mathematicians we may feel satisfaction that these feats (the possibility of which was clear *à priori*) have been actually accomplished; yet we must bear in mind that their application is really confined to cases where the subject matter is perfectly uniform in character, and that beyond this range they are liable to encumber rather than to assist thought.

Not unconnected with this intimate association of ideas and their expression is the fact that, which ever may have been cause, which ever effect, or whether both may not in turn have acted as cause and effect, the culminating age of classic art was contemporaneous with the first great development of mathematical science.[3] In an earlier part of this discourse I have alluded to the importance of mathematical precision recognised in the *technique* of art during the Cinquecento; and I have now time only to add that, on looking still further back, it would seem that sculpture and painting, architecture and music, nay even poetry itself, received a new, if not their first true, impulse at the period when geometric form appeared fresh chiselled by the hand of the mathematician, and when the first ideas of harmony and proportion rang joyously together in the morning tide of art.

Relations of Science to Literature and Art.

Whether the views on which I have here insisted be in any way novel, or whether they be merely such as from habit or from inclination are usually kept out of sight, matters little. But whichever be the case, they may still furnish a solvent of that rigid aversion which both literature and art are too often inclined to maintain towards science of all kinds. It is a very old story that, to know one another better, to dwell upon similarities rather than upon diversities, are the first stages towards a better understanding between two parties; but in few cases has it a truer application than in that here discussed. To recognise the common growth of scientific and other instincts until the time of harvest is not only conducive to a rich crop, but it is also a matter of prudence, lest, in trying to root up weeds from among the wheat, we should at the same time root up that which is as valuable as wheat. When Pascal's father had shut the door of his son's study to mathematics, and closeted him with Latin and Greek, he found on his return that the walls were teeming with formulæ and figures, the more congenial product of the boy's mind. Fortunately for the boy, and fortunately also for science, the mathematics were not torn up, but were suffered to grow together with other subjects. And all said and done, the lad was not the worse scholar or man of letters in the end. But, truth to tell, considering the severance which still subsists in education and during our early years between literature and science, we can hardly wonder if, when thrown together in the afterwork of life, they should meet as strangers; or if the severe garb, the curious implements, and the strange wares of the latter, should seem little attractive when contrasted with the light companionship of the former. The day is yet young, and in the early dawn many things look weird and fantastic which in fuller light prove to be familiar and useful. The outcomings of science, which at one time have been deemed to be but stumbling-blocks scattered in the way, may ultimately prove stepping-stones which have been carefully laid to form a pathway over difficult places for the children of "sweetness and of light."

Concluding Remarks.

The instances on which we have dwelt are only a few out of many in which mathematics may be found ruling and governing a variety of subjects. It is as the supreme result of all experience, the framework in which all the varied manifestations of nature have been set, that our science has laid claim to be the arbiter of all knowledge. She does not indeed contribute elements of fact, which must be sought elsewhere; but she sifts and regulates them; she proclaims the laws to which they must conform if those elements are to issue in precise results. From the data of a problem she can infallibly extract all possible consequences, whether they be those first sought, or others not anticipated; but she can introduce nothing which was not latent in the original statement. Mathematics cannot tell us whether there be or be not limits to time or space; but to her they are both of indefinite extent, and this in a sense which neither affirms nor denies that they are either infinite or finite. Mathematics cannot tell us whether matter be continuous or discrete in its structure; but to her it is indifferent whether it be one or the other, and her conclusions are independent of either particular hypothesis. Mathematics can tell us nothing of the origin of matter, of its creation or its annihilation; she deals only with it in a state of existence; but within that state its modes of existence may vary from our most elementary conception to our most complex experience. Mathematics can tell us nothing beyond the problems which she specifically undertakes; she will carry them to their limit, but there she stops, and upon the great region beyond which she is imperturbably silent.

Conterminous with space and coeval with time is the kingdom of mathematics; within this range her dominion is supreme; otherwise than according to her order nothing can exist; in contradiction to her laws nothing takes place. On her mysterious scroll is to be found written for those who can read it that which has been, that which is, and that which is to come. Everything material which is the subject of knowledge has number, order, or position; and these are her first outlines for a sketch of the universe. If our more feeble hands cannot follow out the details, still her part has been drawn with an unerring pen, and her work cannot be gainsaid. So wide is the

[1] For example, in Herbart's "Psychologie."

[2] A specimen will be found in the Moralia of Gregory the Great, Lib. I., c. xiv., of which I quote only the arithmetical part:—

"Quid in septenario numero, nisi summa perfectionis accipitur? Ut enim humanæ rationis causas de septenario numero taceamus, quæ afferunt, quòd idcirco perfectus sit, quia exprimo pari constat, et primo impari; ex primo, qui dividi potest, et primo, qui dividi non potest; certissimè scimus, quòd septenarium numerum Scriptura Sacra pro perfectione ponere consuevit. . . . . A septenario quippe numero in duodenarium surgitur. Nam septenarius suis in se partibus multiplicatus, ad duodenarium tenditur. Sive enim quatuor per tria, sive per quatuor tria ducantur, septem in duodecim vertuntur. . . . . Jam superiùs dictum est, quòd in quinquagenario numero, qui septem hebdomadibus ac monade additâ impletur, requies designatur; denario autem numero summa perfectionis exprimitur."

[3] Approximate dates B.C. of—

Sculptors, Painters, and Poets.		Mathematicians.	
Stesichorus,	600.	Thales,	600.
Pindar,	522-442.	Pythagoras,	550.
Æschylus,	500-450.	Anaxagoras,	500-450.
Sophocles,	495-400.	Hippocrates,	460.
Euripides,	480-400.		
Phidias,	488-432.		
Praxiteles,	450-400.	Theætetus,	440.
Zeuxis,	400.	Archytas,	400.
Apelles,	350.		
Scopas,	350.	Euclid,	323-283.

range of mathematical science, so indefinitely may it extend beyond our actual powers of manipulation, that at some moments we are inclined to fall down with even more than reverence before her majestic presence. But so strictly limited are her promises and powers, about so much that we might wish to know does she offer no information whatever, that at other moments we are fain to call her results but a vain thing, and to reject them as a stone when we had asked for bread. If one aspect of the subject encourages our hopes, so does the other tend to chasten our desires; and he is perhaps the wisest, and in the long run the happiest among his fellows, who has learnt not only mathematics, but also the larger lesson which they indirectly teach, namely, to temper our aspirations to that which is possible, to moderate our desires to that which is attainable, to restrict our hopes to that of which accomplishment, if not immediately practicable, is at least distinctly within the range of conception. That which is at present beyond our ken may, at some period and in some manner as yet unknown to us, fall within our grasp; but our science teaches us, while ever yearning with Goethe for "Light, more light," to concentrate our attention upon that of which our powers are capable, and contentedly to leave for future experience the solution of problems to which we can at present say neither yea nor nay.

It is within the region thus indicated that knowledge in the true sense of the word is to be sought. Other modes of influence there are in society and in individual life, other forms of energy beside that of intellect. There is the potential energy of sympathy, the actual energy of work; there are the vicissitudes of life, the diversity of circumstance, health, and disease, and all the perplexing issues, whether for good or for evil, of impulse and of passion. But although the book of life cannot at present be read by the light of science alone, nor the wayfarers be satisfied by the few loaves of knowledge now in our hands, yet it would be difficult to overstate the almost miraculous increase which may be produced by a liberal distribution of what we already have, and by a restriction of our cravings within the limits of possibility.

In proportion as method is better than impulse, deliberate purpose than erratic action, the clear glow of sunshine than irregular reflection, and definite utterances than an uncertain sound; in proportion as knowledge is better than surmise, proof than opinion; in that proportion will the mathematician value a discrimination between the certain and the uncertain, and a just estimate of the issues which depend upon one motive power or the other. While on the one hand he accords to his neighbours full liberty to regard the unknown in whatever way they are led by the noblest powers that they possess, so on the other he claims an equal right to draw a clear line of demarcation between that which is a matter of knowledge, and that which is at all events something else, and to treat the one category as fairly claiming our assent, the other as open to further evidence. And yet, when he sees around him those whose aspirations are so fair, whose impulses so strong, whose receptive faculties so sensitive, as to give objective reality to what is often but a reflex from themselves, or a projected image of their own experience, he will be willing to admit that there are influences which he cannot as yet either fathom or measure, but whose operation he must recognise among the facts of our existence.

SECTION C.

GEOLOGY.

OPENING ADDRESS BY THE PRESIDENT, JOHN EVANS, D.C.L., F.R.S., F.G.S., &c.

IN opening the proceedings of this Section, I cannot but call attention to the fact that the present is the third occasion on which the British Association has met in this city, its first meeting here having taken place in the year 1835, or forty-three years ago. On that occasion, as indeed for many years afterwards, the two distinct, though to some extent cognate branches of study, geology and geography, were classed in the same section, and its president was a man of whom Irish science may well be proud, and who, I am thankful to say, is still living to enjoy his well-deserved honours—the veteran geologist, Sir Richard John Griffith, the author of the first geological map of Ireland. It seems hardly credible that the construction of this map was commenced in the summer of 1812, or sixty-six years ago; but the records of the Geological Society of London testify to the still more remarkable fact that Sir Richard Griffith was elected a fellow of that Society in 1808—seventy years ago. Indeed, in 1854, when the Wollaston medal was awarded to the then Dr. Griffith, the president, the late Prof. Edward Forbes, spoke as he said reverentially to one of the earliest members of the Society, and to a geologist who appeared in print before he, the president, was born. It was well said on that occasion that the map lately mentioned was one of the most remarkable geological maps ever produced by a single geologist; and I make no doubt that those who are at present engaged on the Geological Survey of this island will testify, as did their predecessors, to the value of this "surprising monument of observation and skill." When speaking of the Geological Survey of Ireland, it will not, I am sure, be thought out of place if I offer here a tribute of respect to the memory of one who was originally a student in the college within whose walls we are assembled, and who subsequently occupied posts of the highest importance in connection with the Geological Society of Dublin and the Geological Survey of Ireland, besides filling the professorial Chair of Geology in this University: I mean Dr. Thomas Oldham, the late Director of the Geological Survey of India. With the marvellous amount of work which he was enabled to accomplish in that country you are all acquainted, and you will all share in the regret that the period of his well-earned retirement—that "requies optimorum meritorum"—should have been so quickly cut short by death. His name will, however, long survive, and future students of geology will have no difficulty in recognising the distinguished labourer in their science after whom the Cambrian *Oldhamia* of the Wicklow hills so worthily received its name.

But to return to this Association.

On the next occasion of its meeting in Dublin, in 1857, Section C had become devoted to geology alone, and geography was excluded, the president being Lord Talbot de Malahide, a nobleman whom also we still have among us, and who is alike well known to archæologists and geologists.

As the last meeting of the Association in this city took place twenty-one years ago, it would at first sight appear that in opening our proceedings I might with propriety dwell on the progress which has been made within that period in the development of the geology of Ireland. I must, however, remind you that it is only four years since the Association held its meeting in what I may almost call the neighbouring town of Belfast, when the accomplished chief of the Geological Survey in Ireland presided over this section and delivered an address, in which some of the more interesting features of the country, especially those of the volcanic district of the north-east of this island, were discussed. During the present year, moreover, he has published his comprehensive work on the "Physical Geology and Geography of Ireland," which I commend to you as far more likely to call your attention to the characteristic features of the country and the latest discoveries with regard to its geology than anything I could compile.

In addition to this, there has appeared during the present year another interesting volume, which records the impressions of a highly intelligent foreign geologist on visiting this country. I mean the "Aus Irland" of Dr. Arnold von Lassaulx, Professor of Mineralogy in the University of Breslau. For this volume, in which shrewd remarks on the country and its inhabitants are mingled with geological observations and valuable comparisons of the Irish formations with those of other countries, we are indebted to the meeting of the British Association having been held two years ago at Glasgow, which attracted the author to visit the British Islands.

So much having lately been published upon the geology of this country, I shall content myself with making a very few general observations with regard to it, and propose subsequently to touch briefly on some of those questions which, within the last twelve months, have occupied the attention of those who are engaged in the advancement of our science.

As to the geology of this country, I may observe that we are here assembled just on the edge of that great central plain which forms so important a feature in the map of Ireland, and which stretches from Dublin Bay on the east coast, to Galway Bay on the west, with hardly a portion of it attaining to an elevation of 300 feet above the sea, over a tract of country nearly 150 miles in extent in almost every direction.

The boundaries of this great plain and those of the carboniferous limestone almost coincide, so that we have here the somewhat remarkable feature of a formation which in England is of such a character as to have received the name of the mountain limestone, constituting in the neighbouring island nearly the whole of the plain country. In some of the north-western

counties, however, as for instance Fermanagh and Sligo, it assumes its more mountainous character. Nearly the whole of this central plain is overlain with boulder clay, limestone gravel, or middle drift, and extensive bogs, so that the subjacent rock is but occasionally seen. In several places detached bosses of old red sandstone rise through the limestone, and there is also good reason for believing, with Prof. Hull, that the whole of the area was at one time covered with the upper members of the carboniferous group, including the true coal measures, of which unfortunately but small patches remain, and those upon the margin of the plain. From the absence of the upper palæozoic, mesozoic, and cainozoic formations over the area, Prof. Hull has arrived at the conclusion that the surface remained in the condition of dry land, while that of England was being submerged beneath the waters of the sea, over the bed of which nearly all these formations were deposited. To a certain extent, however, he leaves it an open question whether some of the mesozoic strata which occur over the north-east of Ireland may not have been deposited over the centre and south. The amount of denudation over this central area has, no doubt, been such that the chances of even Prof. Judd finding traces of these later deposits appear at first sight to be but small; but whether the whole of this vast amount of denudation is due to the wasting influence of rain, rivers, and other sub-aërial agents of erosion, is a question which I venture to regard as at all events open to discussion. It appears to be the case that in some parts of the north of Ireland the whole of the upper carboniferous beds had been denuded before the deposition of any permian strata, as these are deposited immediately on the carboniferous limestone; and if this amount of denudation had taken place in pre-permian times in the north, there seems a possibility of the same having been the case in central Ireland. If so, it is possible that some traces of the later deposits may yet be found on the central plain. Certainly, if we are still to regard the white chalk as a deep-sea deposit, the cretaceous rocks of the north-east of Ireland must have at one time extended farther south than they do at present, and somewhere or other there must have been shore deposits of that period formed farther south than the Upper Greensand of Antrim. The careful investigations of Prof. Judd have largely extended our knowledge of the secondary rocks of the western coast and islands of Scotland, and he has been able to show that the Jurassic series of the Western Highlands could not have had a thickness of less than three thousand feet. It is therefore hard to believe that with such a development in so closely neighbouring a district, the deposits of the same age in Ireland can have been restricted to their present area.

Prof. Judd considers that the amount of denudation in the Scottish Highlands since the mesozoic, and even the miocene period, has been enormous, and that the great surface features of the Highlands were produced in pliocene times. It seems therefore possible, if not probable, that so long a period of exposure to sub-aërial influence as that assigned to the central plain of Ireland by Prof. Hull would have resulted in a more uneven land surface than that which we now find. At all events, the history of this remarkable physical feature is one which is of high interest, and can hardly as yet be considered as closed.

With regard to the mountainous districts surrounding the central plain, we shall, I believe, have the opportunity of visiting some parts of the Wicklow Mountains, a district from which a portion, at all events, of the native gold of Ireland was procured in ancient times, as indeed it continues to be. Of the abundance of gold in this country in early times, a glance at the magnificent collection of ancient ornaments preserved in the museum of the Royal Irish Academy will serve to give an idea. Even in times more recent than those in which the bulk of these ornaments were made, gold was an important product of this country, and I am tempted to quote a few lines from an early English poem, "The Libell of Englishe Policye," written in the year 1436. In treating of the commodities of Ireland, the author says that the country is

"So large, so gode, and so commodious,
That to declare is straunge and merveilous.
For of silver and gold there is the ore
Among the wilde Irish, though they be pore;
For they ar rude and can thereon no skille,
So that, if we hadde ther pese and good wille,
To mine and fine and metal for to pure
In wilde Irishe mighte we find the cure;
As in Londone saith a jewellere
Which broughte from thennes gold oor to us here,
Wherof was fined metal gode and clene,
That at the touch no better coud be sene."

Sir William Wilde has observed that the south-western half of Ireland has yielded a greater amount of gold antiquities than the north-western, and probably this would hold good with regard to the production of the metal itself, though it has been found in the counties of Antrim, Tyrone, and Derry, as well as in those of Dublin, Wicklow, Wexford, and Kildare.

The north-east of Ireland possesses, however, another geological feature peculiar to itself in that great expanse of volcanic beds which formed the subject of Professor Hull's address to this section at the Belfast meeting. My only object in now mentioning them is again to call attention to their containing the only remains of a miocene flora which are to be found in this island. Analogous beds were detected in the corresponding basalts in the Island of Mull by the Duke of Argyll in 1851. With the exception of the Hempstead beds of the Isle of Wight, which should probably be classed as oligocene, and the Bovey Tracey beds of Devonshire, these are almost the only deposits of miocene age in the British Isles. The contrast presented by the scarcity of deposits of this period in Britain with their abundance in the north-west, centre, and south of France, Switzerland, and generally in the south of Europe, is striking. Instead of thick deposits covering hundreds of square miles of country, like the miocene beds bordering the Pyrenees or those of the great system of the Auvergne, we have small patches owing their preservation either to volcanic outbursts having covered them up, or to some favourable circumstance having preserved them from total denudation. Whether we are to assume with the late Prof. Edward Forbes, that the general dearth of these strata in the British Isles arose from the extent of dry land which prevailed during the long interval between the eocene and pliocene periods, or whether we assume the former existence of widespread marine deposits which have since been entirely removed, the case is not one without difficulty. At all events, the absence of representatives of this period within the British area has a tendency to prevent a due appreciation of the enormous extent of the miocene period being generally felt in this country. Nor, generally speaking, do we, I think, take a fair estimate of the remoteness in time to which we must date back the commencement of that lengthened period. Prof. Haughton, judging from the maximum observed thickness of each successive deposit, has calculated that a greater interval of time now separates us from the miocene period than that which was occupied in producing all the secondary and tertiary strata from the triassic to the miocene epoch, and, without endorsing the whole of my accomplished friend's conclusions, I incline to concur in such an estimate. When it is considered that the Ballypalidy beds of Antrim and the Lough Neagh clays are the sole representatives in Ireland of two periods of such length and importance as the miocene and pliocene, their high interest will be more apparent, and I trust that no opportunity of minutely studying them will be neglected.

There is one other point with regard to Irish geology on which it will be well to say a few words, though it is of a negative rather than a positive character. I mean the absence, so far as at present known, of palæolithic implements in this country. It is true that Prof. Hull, in the book to which I am so much indebted, speaks of a raised beach on the Antrim coast as containing worked flints of that rude form and finish known as palæolithic; but this is a slip of the pen, by which the author has fallen into the not uncommon error of applying a term which is merely significant of the age of the implements to their external character. However rude may be the workmanship of the flint implements found at Kilroot, they belong to the neolithic, and not to the palæolithic period. So far as I am aware no example of any implement belonging to the age of the mammoth, rhinoceros, and other members of the quaternary fauna has as yet been found in Ireland. Indeed, the remains of *Elephas primigenius* and its associates are of exceedingly rare occurrence in this country, though they have been found with those of bear and reindeer in the Shandon Cave near Dungarvan. It is, of course, impossible to foretell what future researches may bring to light; but judging from analogy it seems hardly probable that until ancient river-gravels containing the remains of the quaternary group of mammals are found in this island, veritable palæolithic instruments will be discovered. The association of the two classes of remains is so constant that we may fairly assume that the animals formed the principal food of the palæolithic hunters, and that any causes which lead to the absence of the one class will lead to the absence of the other also.

There, is, however, one member of that old quaternary group

which is far more abundant in Ireland than it is in England or on the continent of Europe—the *Megaceros*—which has rightly received the appellation of *Hibernicus*.

I hope that we may have an opportunity, under the guidance of Mr. Richard Moss, of seeing some of the remains of this "antlered monarch of the waste" in the position in which they were originally interred, and it will be an interesting question for consideration whether these remains can be regarded as of the same geological age as those of the English caves and river-gravels, or whether they do not for the most part belong to what Prof. Boyd Dawkins has termed the pre-historic period. It seems by no means improbable that this gigantic stag survived in this country for ages after he had become extinct in other lands, and that the view held by Prof. Hull of his extinction being due to persecution by man is correct. If this be so it would seem to follow that the human occupation of Ireland is of far more recent date than that of the sister country.

And this brings me to one of those questions which have of late been occupying the attention of geologists. I mean the date which is to be assigned to the implement-bearing beds of palæolithic age in England. Dr. James Geikie has held that for the most part they belong to an interglacial episode towards the close of the glacial period, and regards it as certain that no palæolithic bed can be shown to belong to a more recent date than the mild era that preceded the last great submergence.

His follower, Mr. Skertchley, records the finding of palæolithic implements in no less than three interglacial beds, each underlying boulder clays of different ages and somewhat different characters, the Hessle, the purple, and the chalky boulder clay. This raises two main questions, first, as to how far Dr. Croll's theory of the great alternations of climate during the glacial period can be safely maintained; and secondly, how far the observations as to the discovery of implements in the so-called Brandon beds underlying the chalky boulder clay can be substantiated. Another question is how far the palæolithic deposits can be divided into those of modern and ancient valleys, separated from each other by the purple boulder clay, and the later of the two older than the Hessle beds. It would be out of place here to discuss these questions at length. I will only observe, that in a considerable number of cases the gravels containing the implements can be distinctly shown to be of much later date than the chalky boulder clay, and that if the implements occur in successive beds in the same district, each separated from the other by an enormous lapse of time, during which the whole country was buried beneath incredibly large masses of invading ice, and the whole mammalian fauna was driven away, it is a very remarkable circumstance. It is not the less remarkable because this succession of different palæolithic ages seems to be observable in one small district only, and there is as close a resemblance between the instruments of the presumably different ages as there is between those of admittedly the same date. I have always maintained the probability of evidence being found of the existence of Man at an earlier period than that of the post-glacial or quaternary river gravels, but, as in all other cases, it appears to me desirable that the evidence brought forward should be thoroughly sifted and all probability of misapprehension removed before it is finally accepted. In the present state of our knowledge, I do not feel confident that the evidence as to these three successive palæolithic deposits has arrived at this satisfactory stage. At the same time is must be borne in mind that if we make the palæolithic period to embrace not only the river gravels but the cave deposits of which the south of France furnishes such typical examples, its duration must have been of vast extent.

In connection with the question of glacial and interglacial periods, I may mention that of climatal changes in general, which has formed another subject to which much attention has of late been given. The return of the Arctic Expedition, and the reports of the geological observations made during its progress, which have been published by Captain Feilden, one of the naturalists to the expedition, in conjunction with Mr. De Rance and Prof. Heer, have conferred additional interest on the question of possible changes in the position of the poles of the earth, and on other kindred speculations. Near Discovery Harbour, about latitude 81° 40', miocene beds were found containing a flora somewhat differing from that which was already known to exist within the Arctic regions. "The Grinnell Land lignite," say the authors of the report, "indicates a thick peat moss, with probably a small lake, with water-lilies on the surface of the water, and reeds on the edges, with birches, poplars, and taxodiums on the banks, and with pines, firs, spruce, elms, and hazel-bushes on the neighbouring hills." When we consider that all of the genera here represented have their present limits at least from twelve to fifteen degrees further south, while the taxodium is now confined to Mexico and the south of the United States, such a sylvan landscape as that described seems entirely out of place in a district within six hundred miles of the pole, to which indeed, if land then extended so far, these Arctic forests must have also extended in miocene times. Making all allowance for the possibility of the habits of such plants being so changed that they could subsist without sunlight during six months of a winter of even longer duration, I cannot see how so high a temperature as that which appears necessary, especially for the evergreen varieties, could have been maintained, assuming that Grinnell Land was then as close to the North Pole as it is at the present day. Nor is this difficulty decreased when we look back to formations earlier than the miocene, for the flora of the secondary and palæozoic rocks of the Arctic regions is identical in character with that of the same rocks when occurring twenty or thirty degrees farther south, while the corals, encrinites, and cephalopods of the carboniferous limestone are such as, from all analogy, might be supposed to indicate a warm climate.

The general opinion of physicists as to the possibility of a change in the position of the earth's axis has recently undergone modifications somewhat analogous in character to those which, in the opinion of some geologists, the position of the axis has itself undergone. Instead of a fixed dogma as to the impossibility of change, we find a divergence of mathematical opinion and variations of the pole differing in extent, allowed by different mathematicians who have of late gone into the question, as, for instance, the Rev. J. F. Twisden,[1] Mr. George Darwin,[2] Prof. Haughton,[3] the Rev. E. Hill,[4] and Sir William Thomson.[5] All agree in the theoretical possibility of a change in the geographical position of the earth's axis of rotation being effected by a redistribution of matter on the surface, but they do not appear to be all in accord as to the extent of such changes. Mr. Twisden, for instance, arrives at the conclusion that the elevation of a belt twenty degrees in width, such as that which I suggested in my presidential address to the Geological Society in 1876, would displace the axis by about ten miles only, while Prof. Haughton maintains that the elevation of two such continents as Europe and Asia would displace it by about sixty-nine miles, and Sir W. Thomson has not only admitted, but asserted as highly probable, that the poles may have been in ancient times "very far from their present geographical position, and may have gradually shifted through ten, twenty, thirty, forty, or more degrees without at any time any perceptible sudden disturbance of either land or water."

I am glad to think that this question, to which I to some extent assisted to direct attention, has been so fully discussed, but I can hardly regard its discussion as being now finally closed. It appears to me doubtful whether eventually it will be found possible to concede to this globe that amount of solidity and rigidity which at present it is held to possess, and which, to my mind at all events, seems to be in entire disaccordance with many geological phenomena. Yet this, as the Rev. O. Fisher[6] has remarked, is presupposed in all the numerical calculations which have been made. I am also doubtful whether in the calculations which have been made, sufficient regard has been shown to the fact that a great part of the exterior of our spheroidal globe consists of fluid which, though of course connected with the more solid part of the globe by gravity, is readily capable of readjusting itself upon its surface, and may, to a great extent, be left out of the account in considering what changes might arise from the disturbance of the equilibrium of the irregular spherical or spheroidal body which it partially covers. It appears to me also possible that some disturbances of equilibrium may take place in a mysterious manner by the redistribution of matter or otherwise in the interior of the globe. Capt. F. J. Evans,[7] arguing from the changes now going on in terrestrial magnetism, has suggested the possibility of some secular changes being due to internal, and not to external causes; and it really be true that there is a difference between the longest and shortest equatorial radii of

1 *Quart. Journ. Geol. Soc.*, 1878, p. 35.
2 *Proc.* R. S., vol. xxv. p. 328. *Phil. Trans.*, clxvii. p. 271.
3 *Proc.* R.S., 1877, 1878.
4 *Geol. Mag.*, June, 1878.
5 Rep. Brit. Assoc., 1876, p. 11.
6 *Geol. Mag.*, July, 1878.
7 NATURE, vol. xviii. p. 80.

the earth, amounting to six thousand three hundred and seventy-eight feet,[1] such a fact would appear to point to a great want of homogeneity in the interior of our planet, and might suggest a possible cause for some disturbance of equilibrium.

I have mentioned Prof. Haughton among those who, from mathematical considerations, have arrived at the conclusion that a geographical change in the position of the axis of rotation of the earth is not only possible but probable. In a recent paper, however, he has maintained that notwithstanding this possibility or probability, we can demonstrate that the pole has not sensibly changed its position during geological periods. He arrives at this conclusion by pointing out that in the Parry Islands, Alaska and Spitzbergen, there are triassic and Jurassic deposits of much the same tropical character, and then by a geometrical method fixing the north pole somewhere near Pekin, and the south pole in Patagonia, within seven hundred miles of a spot where Jurassic ammonites occur, shows that such a theory is untenable. In the same way he fixes the pole in miocene times near Yakutsk, within eight hundred miles of certain miocene coal-beds of the Japanese islands. These objections are at first sight startling, but I think it will be found that if, instead of drawing great circles through certain points, we regard those points as merely isolated localities in a belt of considerable width, there is no need of fixing the pole of either the Jurassic or the miocene period with that amount of nicety with which Prof. Haughton has ascertained its position. The belt may indeed be made to contain the very places on which the objection is founded. Still the method proposed is a good one, and I hope that as our knowledge of foreign geology extends it may be still further pursued. There is, however, one farther consideration to be urged, and that is as to the safety of regarding all deposits of one geological period as contemporaneous in time. Although an almost identical flora may be discovered in two widely-separated beds, it appears to me that chronologically they are more probably of different ages than absolutely contemporaneous; and, inasmuch as the duration of the miocene period must have been enormous, there would be time—if once we assume the wandering of the poles—for such wandering to have been considerable between the beginning and end of the period.

I must not, however, detain you longer upon this phase of geological speculation, but will advert to a subject of more practical interest, the discovery of palæozoic rocks under London. So long ago as 1856 the Kentish Town boring had shown that immediately below the gault red and variegated sandstones and clays occurred, which Professor Prestwich regarded as probably of old red or Devonian age. The boring of Messrs. Meux & Co. has now shown that under Tottenham Court Road, at a depth of little more than nine hundred feet from the surface, there are true Devonian beds, with characteristic fossils, and that Mr. Godwin Austen's prophecy of the existence of palæozoic rocks at an accessible depth under London has proved true. Prof. Prestwich, from a consideration of the French and Belgian coal-fields, inclines to the belief that in the district north of London carboniferous strata may be found. Unfortunately the expense of conducting deep borings, even with the admirable appliances of the Diamond Boring Company, is so great that I almost despair of another experimental borehole, like that carried out in the Wealden district under the auspices of Mr. Willett, being undertaken.

In the department of theoretical geology I would call your attention to some experiments by M. Daubrée, of which he has given accounts at different times to the French Academy of Sciences. In these experiments he has attempted to reproduce on a small scale various geological phenomena, such as faulting, cleavage, jointing, and the elevation of mountain chains. Although the analogy between work in the laboratory and that on the grand scale of nature may not in all cases be perfect, yet these experiments are in the highest degree instructive, and reflect no little credit on the ingenuity of the distinguished chief of the École des Mines.

With regard to recent progress in palæontology, I must venture to refer you to Prof. Alleyne Nicholson's inaugural address lately delivered to the Edinburgh Geological Society, but I cannot pass over in silence the magnificent discoveries in North America, which are principally due to the researches of Profs. Marsh, Leidy, and Cope. The *Diceratherium*, a rhinoceros with two horns placed transversely, and the *Dinoceras*, somewhat allied to the elephant, but with six horns, arranged in pairs, are as marvellous as some of the beasts seen by Sir John Maundeville on his travels, or heard of by Pliny. But perhaps the most remarkable series of remains ever discovered are those which so completely link the existing horse with the *Eohippus* and *Orohippus*, and still farther extend the pedigree of the genus *Equus*, which had already been some years ago so ably traced by Prof. Huxley.

[1] Thomson and Tait, "Phil." p. 648.

Of these American discoveries, as well as those made in the tertiary beds of Europe, M. Albert Gaudry has largely availed himself in his recent beautiful volume on the links in the animal world in geological times, a work which will long be a text-book on the inter-relation of different orders, genera, and species. I am tempted to make use of some portions of M. Gaudry's own analysis of the book, which he communicated to the Geological Society of France. Beginning with the marsupials of the close of the secondary and beginning of the tertiary period, he shows that they are succeeded by such animals as the *Pterodon*, the *Hyænodon*, the *Proviverra*, and *Arctocyon*, which present a mixture of marsupial and placental characters, and to some extent justify a theory of the transition from one order to the other. He next examines the marine mammalia, and points out that, so far as at present known, they make their appearance later than those of the land, and that the examination of the pelvis of the *Halitherium* tends to support the idea that the mammals, such as the sirenians, which at the present day have no hind limbs, are descended from terrestrial quadrupeds, for those limbs in the *Halitherium* are much less reduced than in its recent successors, the dugong and manatee. After tracing the numerous links which are to be found between the extinct and living pachydermata, he proceeds to show that, notwithstanding the great distance between them and the ruminants, transitions may be seen. The earliest ruminants were devoid of horns and antlers, but possessed upper incisors, and by a comparison of the molars of different genera it may readily be conceived how the large bosses of the omnivorous teeth of the pachyderms gradually shaded into the small crescents of the teeth of the ruminants. At the same time the passage from the heavy and complicated extremities of the limbs of the pachyderms to the simpler and lighter feet of the ruminants can be traced. The history of the horse family is also discussed, and the descent of existing proboscidians from the mastodonts is shown to be probable, though the previous forms from which the mastodonts and dinotheria are derived are as yet unknown. Nor can the origin of the carnivora as yet be suggested, though passages between the six existing families of the order may be observed. In conclusion M. Gaudry devotes a chapter to the quadrumana, and thinks that palæontological observations tend to diminish the isolation in which these mammals now stand with regard to the other orders.

One of the most important features insisted on by M. Gaudry is that to which I have already alluded—the development of the complicated molars of most mammals. His view is that by a comparison with early and with fœtal forms the probability may be shown of these compound teeth being made up of what in earlier forms were simple teeth—or, as he has termed them, denticules—which have coalesced in the same manner as have some other parts of the normal bony skeleton. In the compound teeth the denticules in some cases preserve their original conical form, as in the pig tribe; in others are elongated transversely, so as by their junction to form ridges, as in the tapirs; while in others, again, they are drawn out into longitudinal crescents, as in the ruminants. Between these forms there are, of course, innumerable transitions. They do not, however, appear to me to affect the importance of M. Gaudry's observations, which must be regarded as of the highest value in all attempts to trace the inter-relation of different forms of mammalian life. I must not, however, detain you longer on this subject, as I trust that I have said enough to show the importance and interest of this book.

The discoveries of early forms of birds with teeth do not come within M. Gaudry's province; but Prof. Marsh has largely added to our knowledge of these remarkable forms. The tertiary *Odontopteryx toliapicus* from Sheppey, described by Prof. Owen, seems rather to be endowed with bony tooth-like processes in the jaw than actual teeth, and the head of the *Argillornis* from the same locality is at present unknown. But the *Hesperornis* and *Ichthyornis* from the cretaceous beds of America possess veritable teeth, in the one case set in a long groove in the jaw, and in the other in actual sockets. Such intermediate, or, as Prof. Huxley would term them, intercalary, forms, tend

materially to bridge over the gap which at first sight appears to exist between reptiles and birds, but which to many palæontologists was far from being impassable, long before the discoveries just mentioned. The amphicœlous character of the vertebræ of *Ichthyornis* presents another most remarkable peculiarity, which is also of high significance. I hear rumours of the discovery of another *Archæopteryx* in the Solenhofen Slates, which is said to present the head in a much more complete condition than that in which it occurs on the magnificent slab now in the British Museum. As yet, I believe, the jaws have not had the matrix removed from them; but should they prove to be armed with teeth, it will to me be a cause of satisfaction rather than surprise, as confirming an opinion which some fifteen years ago[1] I ventured to express, that this remarkable creature may have been endowed with teeth, either in lieu of or combined with a beak.

I must not, however, detain you longer with any of these general remarks, which are, moreover, becoming somewhat egotistic, but will now proceed to the business of this section, in which I hope that more than one paper of great value and interest will be forthcoming.

[1] *Nat. Hist. Rev.*, vol. v. p. 421.

SECTION D.

BIOLOGY.

OPENING ADDRESS IN THE DEPARTMENT OF ZOOLOGY AND BOTANY, BY PROF. W. H. FLOWER, F.R.S., PRESIDENT OF THE SECTION.

ON the 10th of January, 1778, died the great Swedish Naturalist, Charles Linné, more commonly known as Linnæus, a name which will ever be mentioned with respect and regard in an assembly devoted to the cultivation of the sciences of Zoology and Botany, as whatever may be the future progress of those sciences, the numerous writings of Linnæus, and especially the publication of the "Systema Naturæ," can never cease to be looked upon as marking an era in their development. That work contained a systematic exposition of all that was known on these subjects expressed in language the most terse and precise. The accumulated knowledge of all the workers at zoology, botany, and mineralogy since the world began, was here collected together by patient industry, and welded into a complete and harmonious whole by penetrating genius.

Exactly a century has passed since Linnæus died. What of the progress of the subjects to which he devoted his long and laborious life? This one century is a brief space compared with the ages which have passed since man began to dwell upon the earth, surrounded by living objects, which have, more and more as time rolled on, awakened his curiosity, stimulated his faculties to observe, and impelled him to record the knowledge so gained for the benefit of those to come. How does it stand in comparison with those which preceded it, in the contributions it has thus acquired and recorded?

It may be not without interest in commencing our work at this meeting to cast our eyes back and take stock, as it were, of the knowledge of a hundred years ago, and of that of the present time, and see what advances have been made; to look at the living world as it was known to Linnæus and as it is known to ourselves. The "Systema Naturæ," the last edition of which, revised by the author, was published in 1766, will be a convenient basis for the comparison, but as the subject is one which, even in a most superficial outline, might reach such lengths as would well tire out the most patient of audiences, and absorb time which will be more profitably occupied by the valuable contributions which are forthcoming from other members of the Association, I will merely take a small section of the work, about 100 pages out of the first of the four volumes, those devoted to the first class Mammalia. The comparison of this part is perhaps the easiest, as the contrast is the least striking, and the progress has been comparatively the slowest. The knowledge of large, accessible, and attractive-looking animals had naturally preceded that of minute and obscure organisms, and hence, while in many other departments the advance has altogether revolutionized the knowledge of Linnæus, in the vertebrated classes, especially the one of which I shall now speak, it has only extended and reformed it.

In taking the "Systema Naturæ" of Linnæus, the comparison is certainly carried back somewhat beyond the hundred years which have elapsed since his death, and the brilliant contributions to the knowledge of the Mammalia of Buffon and Daubenton just then beginning to be known, and the systematic compilation of Erxleben (published in 1777), are ignored, but for the present purpose, especially considering the limited time at my disposal, it will be best not to go beyond the actual text of the work in question.

Before considering systematically the different groups into which Linnæus divides the class, I must remark in passing upon what is the greatest, and indeed most marvellous difference between the knowledge of zoology of our time and that of Linnæus. Now we know that the animals at present existing upon the earth are merely the survivors of an immensity of others, different in form, characters, and mode of life, which have peopled the earth through vast ages of time, and to which numerically our existing forms are infinitesimally small, and that the knowledge we possess of an immense number of them, fully justifies the expectation of an enormous further advance in this direction. In the time of Linnæus the existence in any past time of a species having no longer living representatives on the earth, though perhaps the speculation of a few philosophical minds, had not been received among the certainties of science, and at all events found no place in the great work we are now considering.

In the twelfth edition of the "Systema Naturæ" we find the class Mammalia divided into seven orders: I. *Primates*, II. *Bruta*, III. *Feræ*, IV. *Glires*, V. *Pecora*, VI. *Belluæ*, VII. *Cete*. These orders contain forty genera without any intermediate subdivisions. The genera are again divided into species, of which the total number is 220.

The first order, PRIMATES, contains four genera: *Homo*, *Simia*, *Lemur*, and *Vespertilio*.

The vexed question of man's place in the zoological system was thus settled by Linnæus. He belongs to the class *Mammalia*, and the order *Primates*, the same order which includes all known monkeys, lemurs, and bats: he differs only generically from these animals. But then we must remember that the Linnæan genera were not our genera, they correspond usually to what we call families, sometimes to entire orders. So that practically man's position is much the same as that to which, after several vicissitudes, as his separation as an order by Blumenbach and Cuvier, or as a subclass by Owen, he has returned in the systems of nearly all the zoologists of the present day who treat of him as a subject for classification upon zoological and not metaphysical grounds.

Yet since the time of Linnæus the whole science of anthropology has been created. There is certainly an attempt at the division of the species *Homo sapiens* into six varieties in the "Systema Naturæ," but it has scarcely any scientific basis. Zoological anthropology may be said to have commenced with Blumenbach, who, it is interesting to recall as an evidence of the rapid growth of the science, was a contemporary with most of us in this room, for he died as lately as 1840, although his first work on the subject, "De generis humani varietate nativa," was published three years before the death of Linnæus, too late, however, to influence the work we are now chiefly speaking of. The scientific study of the natural history of man is therefore, we may say, but one century old. To what it has grown during that time you are probably aware. Scarcely an important centre of civilisation in the world but has a special society devoted to its cultivation. It forms by itself a special department of the Biological Section of our Association, a department of such importance, that on this occasion no less distinguished a person than a former most eminent president of the whole Association was thought fit to take charge of it. From him you will doubtless hear what is its present scope, aim, and compass. I need only remind you that except the one cardinal point of the zoological relation of man to other forms of life, which Linnæus appears to have appreciated with intuitive perception, all else that you will now hear in that department was not dreamt of in his philosophy.

As might naturally be supposed, apes and monkeys have, for various reasons, attracted the attention of observers of nature from very early times, and consequently Linnæus was able to give rather a goodly list of species of these animals, amounting to thirty-three, but of their mutual affinities, and of the important structural differences which exist between many of them, he seems to have had no idea, his three divisions being simply regulated by the condition of the tail, whether absent, short, or long.

We now know that the so-called anthropoid or man-like apes,

the gorilla, chimpanzee, orang and gibbons, form a group apart from all the others of such importance, that everything related to their history, structure, and habits, has been most assiduously studied, and there is now an immense literature devoted to this group alone. Nothing could better illustrate the advances we have made in a hundred years, than the contrast of our present knowledge of these forms with that of Linnæus. It is true that, as shown in the most interesting story of the gradual development of our knowledge relating to them in the first chapter of Huxley's "Man's Place in Nature," the animal now called gorilla was, without doubt, the pongo, well known to, and clearly described by, our countryman, Andrew Battle, a contemporary of Shakespeare; and that a really accurate and scientific account of the anatomy of the chimpanzee had been published as far back as 1699 by Dr. Edward Tyson, who as the first English comparative anatomist, I am proud to claim as in some sort a predecessor in the chair I have the honour to hold in London, as he is described on the title page of his work as "Reader of Anatomy at Chirurgeons' Hall."

Linnæus was, however, not acquainted with these, and his second species of the genus *Homo*, *H. troglodytes*, and his first of the genus *Simia*, *S. satyrus*, were both made up of vague and semi-fabulous accounts of the animals now known as chimpanzees and orangs, but hopelessly confounded together. Of the gorilla, and what is stranger still, of any of the important genus of gibbons or long-armed apes of South-eastern Asia, he had at the time he revised the "Systema" no idea.

The remaining monkeys, we now know, fall into three very distinct sections: the *Cercopithecidæ* of the Old World, and the *Cebidæ* and *Hapalidæ* of the New, or by whatever other names we may like to designate them. Although members of all three groups appear in the list in the "Systema," they are all confusedly mixed together. Even that the American monkeys belong to a totally different stock from those of the Old World, does not seem to have been suspected.

The genus *Lemur* of Linnæus comprehends five species, of which the first four were all the then known forms of a most interesting section of the Mammalia. These animals, mostly inhabitants of the great island of Madagascar, though some are found in the African continent, and others in some of the Southern and Eastern parts of Asia, constitute a well-defined group, but one of which the relations are very uncertain. At one time, as in the system of Linnæus, they were closely associated with the monkeys. As more complete knowledge of their organisation has been gradually attained, the interval which separates them structurally from those animals has become continually more evident, and since they cannot be placed within the limits of any of the previously constituted orders, it has been considered advisable by some naturalists to increase the ordinal divisions in their behalf and to allow them to take rank as a distinct group, related to the *Primates* on the one hand, and to the *Carnivora* and *Insectivora* on the other. The knowledge of their relations, however, bids fair to be greatly increased by the discoveries of fossil forms lately made both in France and America, some of which seem to carry their affinities even to the *Ungulata*.

Existing upon the earth at present, besides the more ordinary lemurs to which the species known to Linnæus belong, there are two aberrant forms, each represented by a single species. These are the little *Tarsius* of Borneo and Celebes, and the singular *Chiromys*, or Aye-aye, which, though an inhabitant of the head-quarters of the group, Madagascar, and living in the same forests and under the same conditions as the most typical lemurs, exhibits a most remarkable degree of specialization in the structure both of limbs and teeth, the latter being modified so as to resemble, at least superficially, those of the Rodents, a group with which in fact it was once placed. It was discovered by Sonnerat in Madagascar in 1780, two years after the death of Linnæus. The specimen brought to Paris by this traveller was the only one known until 1860. Since that date, however, its native land has been more freely open than before to explorers, and many specimens have been obtained, one having lived for several years in the Gardens of the London Zoological Society.

The history of a name is often not a little curious. Linnæus applied the term *Lemures*, *i.e.*, the departed spirits of men, to these animals on account of their nocturnal habits and ghost-like aspect. The hypothetical continent in the Indian Ocean, supposed to have connected Madagascar with the Malayan Archipelago is called by Mr. Sclater, *Lemuria*, as the presumed original home of the lemur-like animals. Although the steps are not numerous, it might puzzle a classical scholar, ignorant of zoology, to explain the connection between this continent and the Roman festival of the same name.

The fifth animal which Linnæus places in his genus *Lemur*, under the name of *L. volans*, is the very singular creature to which the generic term *Galeopithecus* has since been applied. It is one of those completely aberrant forms, which having no near existing relations, and none yet discovered among extinct forms, are perfect puzzles to systematic zoologists. It is certainly not a lemur, and not a bat, as has been supposed by some. We shrink from multiplying the orders for the sake of single genera containing only two closely allied species; so we have generally allowed it to take refuge among the *Insectivora*, though without being able to show to which of that somewhat heterogeneous group it has any near affinities.

The fourth genus of the *Primates* is *Vespertilio*, comprising six species of bats. This genus has now by universal consent expanded into an order, and one of the best characterised and distinctly circumscribed of any in the class: indeed those who have worked most at the details of the structure of bats, find so much diversity in the characters of the skull, teeth, digestive organs, &c., associated with the modification of the fore-limbs for flight common to all, as almost to entitle them to be regarded rather as a sub-class. Anatomical, as well as palæontological evidence, show that they must have diverged from the ordinary mammalian type at a very far distant date, as the earliest known forms, from the eocene strata, are quite as specialised as any now existing, and no trace has hitherto been discovered of forms linking them to any of the non-volant orders. By the publication within the last few weeks of a valuable monograph on the existing species of the group, entitled "A Catalogue of the Chiroptera in the Collection of the British Museum," by G. E. Dobson, we are enabled to contrast our present knowledge with that of the time of Linnæus. Although the author has suppressed a large number of nominal species which formerly encumbered our catalogues, and wisely abstained from the tendency of most monographists to multiply genera, he describes four hundred species, arranged in eighty genera: nearly double the number of species, and exactly double the number of genera, of the whole class Mammalia in the "Systema Naturæ," and these Dr. Günther remarks in his preface are probably only a portion of those existing. The small size, nocturnal habits, and difficulty of capture of these animals, are sufficient reasons for the supposition that there are still large numbers unknown to science. In the list of Linnæus, the first primary group of Dobson, the *Megachiroptera*, now containing seventy species, is represented by a single one *V. Vampyrus*, obviously a *Pteropus*, to which the bloodthirsty habits of the fabulous vampyre are attributed, but which is not absolutely identified with any one of the known species. The other species described by Linnæus can almost all be identified with bats at present well known.

A curious example of the results of basing classification upon a few, and those somewhat artificial characters, is afforded by one of the true bats, now called *Noctilio leporinus*, though admitted by Linnæus to be "*simillimus vespertilionibus similiter pedibus alatus*," being separated from the others, not only generically, but even placed in another order, that of the *Glires* or Rodents, because it did not, or was supposed not to, fall under the definition of the order *Primates*, which begins "*Dentes primores incisores superiores IV. paralleli.*' In reality this bat has four upper incisors, but the outer ones are so small as to have been overlooked when first examined. But even if this were not so no one would now dream of basing an animal's position upon such a trivial character when opposed to the totality of its organization and habits.

The characters of the incisor teeth are placed in the first rank in the definitions of all the orders in the "Systema Naturæ," and hence the next order called BRUTA, characterized by "*dentes primores nulli superius aut inferius*," contains a curious mixture of heterogeneous animals, as the names of the genera *Elephas*, *Trichechus*, *Bradypus*, *Myrmecophaga*, *Manis*, and *Dasypus* will indicate. In contains, in fact, all the animals then known comprised in the modern orders of *Proboscidea*, *Sirenia*, and *Edentata*, together with the walrus, one of the *Carnivora*. The name *Bruta* has been revived for one of these orders, that more generally called *Edentata*, but I think very inappropriately, for it was certainly not equivalent, and if retained at all, should rather belong to the Proboscidea, as *Elephas* stands first in the list of genera, and was probably in the mind of Linnæus when he assigned the name to the group.

It is curious to find that the striking differences between the African and the Indian elephants, now so well understood by every beginner in zoology, and all the facts which have already been accumulated relating to the numerous extinct forms of Proboscideans, whether Mammoths, Mastodons, or Dinotheria, were quite unknown to Linnæus. One species only, *Elephas maximus*, represented in the zoology of a hundred years ago, was all that was known of the elephants or elephant-like animals.

The genus *Trichechus* of this edition exhibits a very curious phase of zoological knowledge. It contains two species. (1) *T. rosmarus*, the Walrus, now known to be a modified seal, and therefore a member of the Linnæan order Feræ, and (2) *T. manatus*, a name under which were included all the known forms of Manatees and Dugongs, in fact the whole of the modern order *Sirenia;* animals widely removed in all essential points of their organisation from the walrus, with which they are here generically united. Their position, however, between the elephant on the one hand and the sloths on the other, is far better than their association with the Cetacea, as in Cuvier's system, an association from which it has been most difficult to disengage them, notwithstanding their total dissimilarity, except in a few external characters. Although the discovery of many fossil forms has done much to link together the few existing species and to show the essential unity of the group, it has thrown no light upon their origin, or their affinities to other mammals. They still stand, both by their structure and their habits, a strangely isolated group, and it baffles conjecture to say whence they have been derived, or how they have attained their present singular organisation.

The remaining genera of the Linnæan order *Bruta* constitute the group out of which Cuvier, following Blumenbach, formed his order *Edentata*, a name certainly not happily chosen for a division which includes species like the great armadillo, having a larger number of teeth than any other land mammal, but which, nevertheless, has been so generally adopted, and is so well understood, that to attempt to change it would only introduce an element of confusion. Four out of five of the principal modifications of form in the group at present known, are indicated by the four Linnæan genera, *Bradypus* or Sloth, *Myrmecophaga* or Ant-eater, *Manis* or Pangolin, and *Dasypus* or Armadillo. The advances during the century have consisted in the accumulation of a great mass of details respecting these groups, the addition of a fifth and very distinct existing form, the *Orycteropus* or Cape Anteater, and the discovery of numerous and very remarkable extinct forms, such as the megatheriums and glyptodons of South America, so fully known by their well-preserved osseous remains. There is, however, still much to be done in working out the real relationship of the somewhat isolated members of the order, if it be a natural order, both to each other, and to the rest of the Mammalia, from which they stand widely removed in many points of organisation.

The third order of Linnæus, FERÆ, contained all the then known animals, which, with whatever diversities of general structure, agreed in their predatory habits, and possessed certain general characters of teeth and claws to correspond, though the terse definition of "*Dentes primores superiores sex, acutiusculi, canini solitarii*," is by no means universally applicable to them. This order was broken up by Cuvier into the orders Carnivora and Insectivora, and the genus *Didelphys*, included in it by Linnæus, has been since by universal assent removed to another group.

The first six genera belong to the very well-defined and probably natural group now called *Carnivora*. The one placed at the head of the list, *Phoca*, is equivalent to the large and important modern sub-order *Pinnipedia*, the walrus, however, though essentially a seal, having been, as before mentioned, relegated by Linnæus to another order on account of its aberrant dentition. But three species are recorded in the genus. *P. ursina*, the Sea-bear of the North Pacific (now *Otaria ursina*), *P. leonina*, founded on Anson's sea-lion, now commonly called the elephant seal, or sea-elephant (*Macrorhinus proboscideus*, or more properly *leoninus*), and *P. vitulina*, the Common Seal.

The terrestrial sub-order of Carnivora is represented by five genera. (1) *Canis*, including the dog, wolf, hyæna, fox, arctic fox, jackal, &c. (2) *Felis*, with only six species, but still one of the few Linnæan genera, which covers exactly the same ground as at present in the opinion of the majority of zoologists, although it may be mentioned as an example of the tendency towards excessive and unnecessary multiplication of generic names which exists in some quarters, that it has been divided into as many as fourteen. (3) *Viverra*, a heterogeneous group, containing ichneumons, coatis, and skunks, animals belonging to three very distinct families, according to modern ideas. (4) *Mustela*, a far more natural group, being nearly equivalent to the modern family *Mustelidæ*; and, lastly, a very comprehensive genus, *Ursus*, consisting of *U. meles*, the Badger, *U. lotor*, the Raccoon, *U. luscus*, the Wolverene, and all the true bears known, comprised in the single species *U. arctos*. Many interesting forms of Carnivora, as *Cryptoprœta*, *Proteles*, *Eupleres*, *Ailurus* and *Ailuropus*, have no place in the Linnæan system, being comparatively modern discoveries. The very recent date (1869) at which the last-named remarkable animal was made known to science by the enterprising researches of the Abbé David into the fauna of Eastern Thibet, gives hope that we may not yet be at the end of the discovery of even large and hitherto unsuspected forms of existing mammals.

Next in the Linnæan system comes the genus *Didelphys*, constituted for the reception of five species of American opossums. This is a very interesting landmark in the history of the progress of the knowledge of the animal life of the world, as these five opossums, forming a genus in the midst of the order Feræ, were all that was then known of the great sub-class *Marsupialia*, now constituting a group entirely apart from the ordinary members of the class. It is difficult now to imagine an animal world without kangaroos, without wombats, without phalangers, without thylacines, without dasyures, and so many other familiar forms, and yet such was the animal world known to Linnæus. It is true that a species of kangaroo from one of the islands of the Austro-Malayan Archipelago was described as long ago as 1714 by De Bruijn, who saw it alive at the house of the Dutch governor of Batavia, and that Captain Cook and Sir Joseph Banks saw and killed kangaroos on the east coast of Australia in 1770, and had published figures and descriptions of them in 1773, or five years before the death of Linnæus, but the work we are now considering contains no traces of knowledge of the existence of such a remarkable and now so well-known animal.

The three remaining genera of FERÆ, *Talpa*, *Sorex*, and *Erinaceus*, contained all the known species of the present order INSECTIVORA, which now embraces many and very varied forms, quite unsuspected a century ago, and to which it is probable that others will be added by the time the exploration of the animal products of the world is completed.

The fourth order, GLIRES, has remained practically unchanged to our day, although the name *Rodentia* has generally superseded that bestowed upon it by Linnæus. The five genera of the "Systema Naturæ," *Hystrix Lepus*, *Castor*, *Mus*, and *Sciurus*, have been vastly increased, partly by subdivision and partly by the discovery of new forms. *Noctilio* is, as before mentioned, removed to the Chiroptera, but its loss is well compensated for by *Hydrochœrus*, the well-known Capybora, the largest existing member of the group, which in the Linnæan system is placed among the Belluæ, in the same genus with the pigs.

The fifth Linnæan order, PECORA, is a fairly natural group, equivalent to Cuvier's *Ruminantia;* but it is no longer considered of the value of an order, since the animals composing it have now been shown to be as closely related to certain of those belonging to the next order as they are to each other. The first genus, *Camelus*, contains both the American lamas and the Old World camels, the demonstration of the common origin and close affinities of which has been one of the important results of the recent discoveries in the palæontology of the Western continent. In the next genus, *Moschus*, were placed the well-known musk deer of the highlands of Central Asia, and two small African antelopes, which have no special affinity with it. The subsequent inclusion in the same genus of the small chevrotains (*Tragulinæ*), which was very natural at the time, as they agree perfectly with the musk in the absence of horns and the presence of large canine tusks, by which artificial characters the genus was defined by Linnæus, was one of those unfortunate associations which has greatly retarded the progress of knowledge of the true affinities of the group. Judging by the popular works on Zoology, it is still as difficult to apprehend that a chevrotain is not a musk deer, as it is that a manati is not a cetacean; both errors of the same kind, if not quite so gross, as that of regarding a whale as a fish, or a bat as a bird. The genus *Cervus* contains six species of true deer, including the moose, reindeer, red deer, fallow and roe, associated with the giraffe.

The twenty-one species at that time recognised of the great group of hollow-horned Ruminants are distributed quite artificially in three genera, *Capra*, *Ovis*, and *Bos*. Though subsequent investigations have greatly increased the number of species known, we are still in much uncertainty about their mutual affinities and generic distinctions. Being a group of comparatively modern origin, and only just attaining its complete development, variation has chiefly affected the less essential and superficial organs, and the process of extinction of intermediate forms has not operated sufficiently long to break it up into distinctly separated natural minor groups, as is the case with many of the older families, which yield, therefore, far more readily to the needs of systematic classification, especially as long as the extinct forms are unknown or ignored.

The sixth order of land mammals, BELLUÆ, corresponding to the *Pachydermata* of Cuvier, contains what is now known to be a heterogeneous collection, viz., the horses, the hippopotamus, the pigs, rhinoceros, and the rodent capybara. The abolition of these two last orders and the entire rearrangement of the ungulate mammals into two different natural groups, now called *Artiodactyla* and *Perissodactyla*, first indicated by Cuvier in the "Ossimens Fossiles," from the structure of the limbs alone, and afterwards confirmed by Owen from comparison of every part of the organisation, has been one of the most solid advances made in our knowledge of the relations of the mammalia during the present century.

The past history of this, as of so many other groups of vertebrated animals, has been brought to light in an unexpected manner by the wonderful discoveries of fossil remains made during the last ten years in the Rocky Mountains of America, discoveries the importance of which will only be fully appreciated when the elaborate and beautifully illustrated work which Prof. Marsh has now in progress is completed.

The last Linnæan order, CETÆ, is exactly conterminous with the order so named, or rather more generally modified to *Cetacea*, in the best modern systems, for Linnæus did not commit the error of Cuvier and others, of including the Sirenia among the whales. His knowledge of the animals composing the group was necessarily very imperfect, indeed it is only within the last few years, especially since the impulse given to their study by Eschricht of Copenhagen, that the great difficulties which surround the investigation of the structure and habits of these denizens of the open sea have been so far surmounted that we have begun to obtain clear views of their organisation, affinities, and geographical distribution.

Two most remarkable forms of mammals, so abnormal in their organisation as now to be generally considered deserving the rank of a distinct sub-class, the *Echidna* and *Ornithorhynchus*, were first made known to science in 1792 and 1799 respectively, and consequently have no place in the "Systema Naturæ." The very recent discovery of a third form to this group, or at least a very striking modification of one of the forms, the large New Guinea echidna (*Acanthoglossus bruijnii*), is the last important acquisition to our knowledge of the class.

In this brief review of the progress of one small section of one branch of zoological knowledge it will be seen that it is chiefly of systems of arrangement, of classification, and of names that I have been treating. By many biologists of the present day these are looked upon as the least attractive and least profitable branches of the subject. The interest of classification, though it has lost much in some senses by the modern advances of scientific biology, has, however, gained vastly in others. The idea that has now, chiefly in consequence of the writings of Darwin, taken such strong hold upon all working naturalists—the idea of a gradual growth and progressive evolution, and therefore genetic connection between all living things—breaks down the artificial barriers which zoologists raise around their groups, and shows that such names as *species*, *genera*, *families*, *orders*, &c., are merely more or less clumsy attempts to express various shades of differences among creatures connected by infinite gradations, and in this sense destroys the importance attached to them by our predecessors. On the other hand, it immensely increases the interest contained in the word relationship, as it implies that the word is used in a real and not, as formerly, in a metaphorical sense. There is a kind of classification, such as we might apply to inanimate substances or manufactured articles. We may say, for instance, that a tumbler, a wine-glass, and a tea-cup are more closely related to each other than either one is to a chair or a table, and that they might be formed into one group, and the last-named objects be placed in a second. This kind of classification is certainly useful in its way for methodical arrangement and descriptive purposes. It is the kind of arrangement which Linnæus and his contemporaries applied to animals. It is, however, a very different classification from that which supposes that the members of a group having common essential characters are descended from a common ancestor, and have gradually, by whatever cause or means, become differentiated from other groups. On this view a true classification, if it could be obtained, would be a revelation of the whole secret of the evolution of animal life, and it is no wonder that many are willing to devote so large a share of their energies to endeavour to attain it.

The right application of the principles of nomenclature, first clearly established by Linnæus, to the groups we form is, again, by no means to be despised, as laxity and carelessness in this respect are becoming more and more the greatest hindrances to the study of zoology. The introduction of any new term, especially a generic name, and indeed the use of an old one by any person whose authority carries weight, has an appreciable effect upon the progress of science, and should never be done without a full sense of the responsibility incurred. All beginners are puzzled and often repelled by the confused state of zoological nomenclature to an extent to which those who have advanced so far as only to care for the things, and to whom the actual names by which they are called are comparatively indifferent, have little idea. Those whose special gift or inclination leads them to the pursuit of other branches of biology, as morphology, physiology, embryology, &c., must have definite names for the objects they observe, depict, or describe, and are dependent upon the researches of the systematic zoologist for supplying them, and should not neglect to take his counsel, otherwise much of their work will lose its value.

Several times has the British Association thought this a worthy subject for the consideration of its members, and through the instrumentality of a committee of working naturalists drew up in 1842 an excellent code of regulations and suggestions on the subject of zoological nomenclature. These rules were revised and reprinted in 1865, and in accordance with a resolution adopted at the last annual meeting at Plymouth they have been again republished at the cost of the Association during the present year. The mere issue of such rules must have had a beneficial effect, as they have undoubtedly been a guide to many careful and conscientious workers. Unfortunately there are no means of enforcing them upon those of a different class, and there is still something wanting short of enforcing them, which possibly may be within the power of the Association to effect. In the administration of the judicial affairs of a nation, besides the makers of the laws, we have an equally essential body to interpret or apply the law to particular cases—the judges. However carefully compiled or excellent a code of regulations may be, dubious and difficult cases will arise, to which the application of the law is not always clear, and about which individual opinions will differ. The necessary permission given in the Association rules to change names which are either "glaringly false," or not "clearly defined," opens the door to considerable latitude of private interpretation. As what we are aiming at is simply convenience and general accord, and not absolute justice or truth, there are also cases in which the rigid law of priority, even if it can be ascertained, requires qualification, and other cases in which it may be advisable to put up with a small error or inconvenience to avoid falling into a larger one. I may name such cases as the propriety of reviving an obsolete or almost unknown name for one which, if not strictly legitimate, has been universally accepted, or the retention of a name when already applied to a different genus, instead of the institution of another in its place. For instance, should the name *Echidna*, by which the well-known monotrematous mammal is known in every text-book and catalogue in every language, be superseded by *Tachyglossus*, because the former name had previously been applied to a genus of snakes? or should the chimpanzee be no longer called *Troglodytes* lest it should be confounded with a wren? Should *Chiromys* be discarded for *Daubentonia*, *Trichechus* for *Odobenus*, and *Tapirus* for *Hydrochœrus?* Should the Java slow lemur be called *Loris*, *Stenops*, or *Nycticebus?* Should Sowerby's whale be placed in the genus *Physeter*, *Delphinus*, *Delphinorhynchus*, *Heterodon*, *Diodon*, *Aodon*, *Nodus*, *Ziphius*, *Micropterus*, *Micropteron*, *Mesodiodon*, *Dioplodon*, or *Mesoplodon*, in all of which it may be found in various systematic lists? Should one of the largest and best known of the Cetaceans of our seas be called *Balænoptera musculus*, *Physalus antiquorum*, or *Pterobalæna communis*, all names used by authors of high

authority? Should the smallest British seal be called *Phoca hispida*, *fœtida*, or *annellata*?

I might go on indefinitely multiplying instances which will be answered differently by different naturalists, the arguments for one or the other name being often nicely balanced. What is wanted, therefore, is some kind of judicial authority for deciding what should in future be used. If a committee of eminent naturalists, selected from various nations and divided into several sections according to the subjects with which each member is most familiar, could be prevailed upon to take up the task of revising the whole of our existing nomenclature upon the basis of the laws issued by the Association in 1842, occasionally tempering their strictly legal decisions with a little discretion and common sense, and with a view, as much as possible, of avoiding confusion, and promoting general convenience; and if the working zoologists of the world generally would agree to accept the decisions of such a committee as final, we should dispose of many of the difficulties with which we are now troubled. There seems to me no more reason why the nomenclature of such a committee, if it were composed of men in whose judgment their fellow-workers would have confidence, should not be as universally accepted as is the nomenclature of the last edition of the "Systema Naturæ" of Linnæus. We have agreed not to look beyond that work for evidence of priority, and why should we not agree in the same way to accept decisions which would probably be arrived at with even fuller knowledge and greater sense of responsibility?

Whether this suggestion will be received with favour or not it appeared to me that it was one not inappropriate for the consideration of this section which has already dealt with the question in a manner so advantageous to science, and also for this year, which has witnessed the hundredth anniversary of the death of the great teacher of systematic zoology.

Our knowledge of the living inhabitants of the earth has indeed changed since that time. Our views of their relations to the universe, to each other, and to ourselves, have undergone great revolutions. The knowledge of Linnæus far surpassed that of any of his contemporaries; but yet of what we now know he knew but an infinitesimal amount. Much that he thought he knew we now deem false. Nevertheless, some of the oldest words to be found in all his writings contain sentiments which still claim a response in the hearts of many. Although we are less accustomed to see such words in works of science, that is no proof that their significance has been impaired by the marvellous progress of knowledge. With the words which Linnæus selected to place at the head of his great work I will conclude—

> "O Jehova,
> Quam ampla sunt tua opera!
> Quam sapienter ea fecisti!
> Quam plena est terra possessione tua!"

NOTES

THE International Congress of Meteorology, to be held in Paris from August 24 to 28 next, at the Trocadéro, will discuss a long series of questions having an important bearing on the progress of meteorology, and especially on combined action on the part of the meteorologists of various countries. Sixteen subjects are down for discussion relating to the study of storms in Europe and America, the means of carrying on and recording meteorological investigations on a uniform plan, the origin and propagation of cyclones, meteorology and aeronautics, terrestrial magnetism, sun-spots and meteorology, influence of the configuration and nature of soil, and other physical conditions on climate, earthquakes, the measures to be adopted in observatories to hasten the progress of meteorology &c. This is certainly a comprehensive programme, and we trust there will be a good attendance of competent meteorologists of all nations, and that some good practical results will be the outcome of the meeting.

WE notice that Prof. Fuller has resigned the professorship of mathematics at Aberdeen, which he has held with so much distinction. All of the long list of Senior Wranglers who have come from the University of Aberdeen within the last twenty-five years have been his pupils. Prof. Fuller was tutor of Peterhouse before his appointment to Aberdeen. The chair will probably be filled up in September by the University Court.

THE *Journal Officiel* has published the dates of three new congresses to be held at the Trocadéro and Tuileries:—Weights and Measures, September 2, 3, 4; Silk-culture, September 5, 6, 7, 9, 11; Legal Medicine, August 11, 13, 14.

WE are informed that a course of six lectures on meteorology will be given under the auspices of the council of the Meteorological Society, commencing in October next. The subjects of the lectures will be:—"The Nature and Physical Properties of the Atmosphere;" "Air Temperature, its Distribution and Range;" "Atmospheric Pressure, Wind, and Storms;" "Clouds and Weather Signs;" "Rain, Snow, Hail, and Electricity;" and "The Nature, Methods, and General Objects of Meteorology." It is intended that these lectures shall give a concise account of the present state of knowledge on the above subjects. The lectures will be open to the public, admission being by ticket, to be obtained at the office of the Society, 30, Great George Street, Westminster, S.W. Further particulars, giving full information as to the time, place, &c., will be duly announced.

SCIENTIFIC study does not yet appear to have attracted a superfluity of women, to judge by the numbers of candidates at the Cambridge Higher Local Examinations recently held. Only about thirty out of 500 took the science subjects; twenty-one took botany, one failed, and three obtained distinction; twenty-six geology and physical geography, of whom two failed, and seven were distinguished; seven geology, one failed, three distinguished; nine chemistry, three failed, none distinguished. Ten of the science candidates sat at Cambridge, and among them they gained ten out of fourteen of the distinctions given. Miss E. M. Clarke, of Cambridge, was distinguished in geology, zoology, and botany, and passed in chemistry. Mathematics got only twenty-three candidates, of whom four failed; only two, however, were placed in the first class (being Cambridge students), and two in the second. We are glad to learn that two new subjects are to be set in the science group next year, namely, physics and physiology, the latter so much needed in all girls' schools. Also, students will be allowed to take this group without having to pass Group A (literature and history) first, although it will be required for a full certificate.

SIR SAMUEL BAKER, in a letter to a contemporary, advocates the establishment of a botanical garden in Cyprus, similar to that in Ceylon, under the charge of a competent official, by whom experiments will be made, and the trees most suitable for the climate and varying altitudes of mountain ranges be selected.

A PERUVIAN newspaper, the *Bolsa*, we learn from the *Colonies*, says that extraordinary phenomena have been observed in connection with the "Corpuna" volcano in the province of Castilla, which have caused great alarm among the population. The immense banks of snow which have crowned its summit from time immemorial have suddenly melted away with such rapidity as to cause torrents to rush down the sides of the mountain, washing out immense quantities of stones and earth. The river below, being unable to contain the great body of water so suddenly added to it, overflowed its banks, causing great damage and distress. A great chasm or lateral crater next opened on one side, throwing out volumes of smoke and steam as well as tongues of flame, which were distinctly visible at night, accompanied with loud subterranean rumblings. It had never been supposed that the Corpuna was or could be a volcano. and there is no tradition that it was ever in a state of eruption, Nor within the memory of man has its crown of snow ever been absent.

THE British Medical Association concluded its meeting at Bath on Friday. The Association meets next year at Cork, Dr. O'Connor being the president elect.

EXPERIMENTS have been made at Paris with the telephone between the Exhibition building and Versailles, and they proved very successful. It is intended to make use of the telephone during the great military manœuvres.

THE Paper Exhibition which we intimated some time ago was to be held in Berlin is now opened, and an instructive and interesting account of the variety of objects exhibited—from flimsy to paper carpets, chairs, and even boats—will be found in the *Times* of the 13th. Both the articles manufactured and the materials, chemical and other, connected with the manufacture of paper, are exhibited, and judging from the description, the exhibition althogether must be at least as attractive as the Caxton Exhibition of last year. One of the principal exhibitors is Prince Bismarck, who is a partner in a large firm at Varzin; one of his specialties is paper slips for the Morse telegraph apparatus. The following interesting statistics are given by the *Times*' Correspondent from the Catalogue :—

	Number of Inhabitants.	Kilos consumed.	Kilos per head.
United States	39,000,000	535,000,000	14
Germany	43,000,000	244,000,000	6
England	33,000,000	168,000,000	5
France	37,000,000	138,000,000	3·6
Austria-Hungary	36,000,000	92,000,000	2·5
Russia	27,000,000	67,000,000	0·9
Italy	28,000,000	38,000,000	1·4
Scandinavia	6,000,000	3,000,000	0·5
Belgium	5,500,000	27,000,000	5·1
Switzerland	2,500,000	17,000,000	6·3

According to inscriptions put up in the hall and illustrated by paste-board cubes of different size, 600,000,000 men employ Chinese paper, while 366,000,000 use the European and 130,000,000 the Arabian article; 24,000,000 write on leaves, bark, and wood, 280,000,000 dispensing with writing and reading, and consequently taking no interest in this enlightened exhibition.

THE excursionists who went to the Boulonnais last week under the auspices of the Geologists' Association, led by Prof. Morris, seem to have had a good time of it. Their intention having been communicated to M. A. Huguet, Senator, Maire of Boulogne, he convoked the local geologists, the members of the Société Académique de Boulogne, the Société Médicale, the committees of the Public Library and of the Museum, and invitations were at the same time sent to the Presidents of the Geological Societies of Paris, Lille, &c., to meet their scientific brethren from across the Channel and give them a proper welcome on this the first occasion of English geologists in any number visiting France. Among those assembled to meet the excursionists on their arrival at Boulogne were M. Edmond Pellat, ex-president of the French Geological Society, Dr. Ch. Barrois, Vice-president of the North of France Geological Society, Prof. Giard of the Zoological Laboratory at Wimereux, the British Vice-Consul and other persons of note. The Maire bade the party welcome, and hoped he would have the pleasure of welcoming other deputations of scientific inquirers. Indeed the occasion seems to have assumed somewhat of an international character, and we should not be surprised if the Maire of Boulogne has to welcome many such deputations. Perhaps, should the North of France Geological Society organise an excursion to the London Basin, the Geologists Association may succeed in getting the Lord Mayor to return the pleasant compliment paid the English geologists by the civic chief of Boulogne; a Mansion House dinner might even be possible.

M. BISCHOFSHEIM, the well-known generous Parisian banker, having recently visited Montsouris Naval Observatory, and found the canvas roof of the equatorial house in wretched condition, Admiral Mouchez told him, in answer to his inquiry, that the budget of the observatory was not rich enough to meet an expense of 50*l*. required for the construction of a comfortable zinc cover. Next morning Admiral Mouchez received a cheque for 120*l*. from his visitor of the previous evening.

WE believe it is deeply regretted in France that M. André and M. Angot, the two French astronomers sent out to America to observe the recent transit of Mercury, did not stay to observe the solar eclipse of July 29, although the two phenomena were visible from the same place. M. André is now in Lyons, and M. Angot arrived in Paris almost on the day when the telegram announced the detection of Vulcan by Prof. Watson.

THE new hotel of the Paris Geographical Society is fast approaching completion, and the date for the opening will be determined soon.

WE regret to state that M. Cochery, the director of the French Postal Telegraph Department, has announced his intention of charging the several communes receiving the daily telegrams of the Agricultural Service a sum of 4*l*. a year. M. Mascart, the new director of the Central Bureau, has obtained the postponement of this step for a month. If M. Cochery carries his resolution into effect it is pretty certain that the greater number of rural communes will refuse to pay, and the organisation which M. Leverrier had so well organised will be thrown into a state of disorganisation less than a year after his death. It is certain that the Meteorological Congress, which is to meet at the Trocadéro ten days hence, will interfere and lay the case before the public.

M. BARDOUX has sent to the prefects of the several departments of France a circular, asking them to collect information on the resources, working, and composition of the meteorological commissions established by Leverrier. The minister intends to give to these boards a uniform organisation.

IT is stated in the French papers that Mr. Edison is to have no reward whatever at the Paris Exhibition for his phonograph. The reasons alleged for this apparent denial of justice are somewhat amusing. The jury of the class of instruments of precision declared that the phonograph could not be considered as at all an instrument of precision, but merely a toy; consequently they sent it to the class of telegraphy to be rewarded. But the telegraphists replied that it was of no use whatever in telegraphy, and refused to examine it. The consequence is that the most wonderful invention, probably, in the Exhibition, will be passed by unmentioned and unrewarded.

ON Friday, August 9, the Société Française d'Hygiène held a banquet at the Continental Hotel, Paris, for the reception of the Sanitary Institute of Great Britain. Dr. Ricord was in the chair, assisted by M. de Lesseps and Anatole de la Forge, the Director of the Press Department in the Home Office. The toast of the Sanitary Institute was replied to by Mr. Chadwick, who handed to the Société Française d'Hygiène a diploma of affiliation signed by the Duke of Northumberland and Dr. Richardson. Some members of the English Ladies' Sanitary Association were also present at the banquet.

IT is not generally known, we are told by the *China Mail* of Hongkong, that official rank is, to a certain extent, hereditary in China. Thus, when an officer of the first rank dies—the four grand secretaries, viceroys, and chief presidents of the "boards" at Peking, for instance—his sons inherit the "full fifth" rank, and are entitled to commence their public career as *Langchung*, or junior lord, of one of the boards. The sons of officers of the "full-second" rank—such as vice-presidents of the boards and governors—may enter public life as assistant

secretaries, with the "half-fifth" rank. The sons of officers of the "half-second" rank become under-assistants, with "full-sixth" rank. The sons of officers of the third rank obtain the honorary degree of bachelor of arts, carrying the seventh rank. The sons of deceased officers of lower grades inherit no rank.

THE thirty-ninth anniversary meeting of the Royal Botanic Society was held in the Gardens, Regent's Park, on Saturday, Mr. James Heywood, F.R.S., in the chair. The annual reports of the council, auditors, and secretary were read. From these reports it appears that the affairs of the society are in a satisfactory state; the receipts in each of the several items had exceeded those of 1877, the balance being some 600*l*. better. The number of new Fellows elected was 112. Four hundred and eighty-one free students' orders for terms of two to six months each had been issued, including sixty-three to artists. The number of cut specimens given to students, professors, and teachers at the several medical and other schools was 63,414—an increase of 20,000 over last year, and 40,000 more than in 1871. The usual exchange of plants and seeds has been maintained with vigour; valuable contributions to the Society's collections were received from correspondents, including the Botanic Gardens of South Australia, Mauritius, Dublin, &c., and also from the Royal Gardens, Kew.

THE additions to the Zoological Society's Gardens during the past week include two European Lynx (*Felis lynx*) from Norway, presented by Major Chadwick; a Hobby (*Hypotriorchis subbuteo*), British Isles, presented by Mr. Howel Scratton; a Solitary Thrush (*Monticola cyanea*), European, presented by Mr. W. Verner; a Common Marmoset (*Hapale jacchus*), a Weeper Capuchin (*Cebus capucinus*), three Bluish Finches (*Spermophila cærulescens*) from Brazil, a Coati (*Nasua nasica*), a Common Boa (*Boa constrictor*), a Common Teguexin (*Teius teguexin*) from South America, a Red and Blue Macaw (*Ara macao*) from Central America, deposited; a Black-footed Penguin (*Spheniscus demersus*) from South Africa, two Common Kingfishers (*Alcedo ispida*), British Isles, purchased; a Wapiti Deer (*Cervus canadensis*), born in the Gardens; a Black-crested Cardinal (*Gubernatrix cristatella*), two Talpacoti Ground Doves (*Chamæpelia talpacoti*), bred in the Gardens.

THE NORWEGIAN NORTH ATLANTIC EXPEDITION

THE Expedition left Hammerfest on July 13 on its trip westwards. The depths sounded on the following days were not great, the greatest being 1,440 fathoms in 72° 16′ N. lat. and 8° 9′ E. long. On the 17th, late in the evening, we unexpectedly met the ice of the Greenland Arctic current, in 73° 10′ N. lat. and 3° 22′ W. long., and were obliged to turn eastwards and northwards. Our next cross section commenced in 75° 16′ N. lat. and 0° 54′ W. long., and went along the 75th parallel to a point north-east of Bear Island. On the westernmost point we had a depth of 1,985 fathoms, but later only smaller depths. The bank west of Bear Island lies much more easterly and nearer this island than shown in the charts, there being a depth of 1,100 fathoms in the place where the French Expedition with *La Recherche* in 1839 gives 259 fathoms. The weather was in the beginning favourable, but under the ice we had strong northerly winds with a temperature of 2° C. and a heavy swell from the south-east. Having passed the meridian of Bear Island the temperature of the water fell to 0·2° C. on the Spitzbergen Bank. From our last sounding and dredging station north-east of Bear Island we sailed down to the east side of the island and stopped outside the south-east side, but the heavy wind and sea did not allow us to go on shore. With the glass we saw distinctly the Russian hut and its environs, but were not able to see our flag, which we had planted by the Dutch port, nor any column or cairn which could show us that the Dutchmen had been there. At 10 P.M. on July 23 we made sail for Norway, and with a fair but rather heavy gale and storm the *Vöringen* went on, heavily rolling, till we, twenty-four hours afterwards, made the Norwegian coast in thick rainy weather. At 4 A.M. on the 24th we dropped our anchor in Hammerfest.

Our soundings show that there is a sort of ridge 1,200–1,300 fathoms deep, between Bear Island and Jan Mayen. The trawl has brought up from the ice-cold water in these depths many rare and several new fishes, particularly of the genus Lycodes. A good many other new animals have been secured by our zoologists. The line of 0° C. lies in our last two cross-sections still at a depth of 500 fathoms. In the eastern part of our sea, the boundary line of the polar current, where 0° is to be found is less than 30 fathoms, and the surface water is less than 5°, lies in lat. 72½° N., long. 6° E., and in lat. 75° N., long. 7½° E. North and east of Bear Island the ice-cold Spitzbergen current runs southward over a very shallow bottom, the depths being only some 20 fathoms. The border of the Greenland Arctic current shows the same phenomenon as that observed in our northern fjords, viz., a minimum of temperature in, say, 40 fathoms depth, a second maximum about 0° C. in, say, 100 fathoms, and the absolute minimum of − 1·4° at the sea bottom.

We are now fitting out for our last trip, to the sea west of Spitzbergen, and expect to be ready to sail on the 30th.

At this moment we have news from the Dutch expedition, whose commander has left a letter, sent us from Vardö. He tells us that they made Jan Mayen on the east side but were not able to land in the heavy sea. From Jan Mayen they proceeded along the edge of the ice to Hackluyt's Headland, North Spitzbergen, which they reached on June 19. North of Spitzbergen they spent some fourteen days, and went as far east as to the Verlegin Hook. Thence they sailed southwards, called at Kobbe Bay, and found our mail at Bear Island, but were obliged to leave that place in a hurry, the wind being rather strong. Their letters home have probably been taken by a Norwegian fisherman off Vardö. Prof. Nordenskjöld passed the North Cape some days ago.

Hammerfest, July 27 H. MOHN

THE ECLIPSE OF THE SUN

ON another page we print an article, just received from Mr. Lockyer, on the recent eclipse, which, from its title and date, it will be seen was written before the event. The following telegrams from the *New York Tribune* of July 31 will perhaps enable our readers, in the meantime, to form a more complete idea of the results achieved.

A telegram to the *Tribune* from Lebanon, Mo., July 30, states that the Fort Worth party of observers, consisting of L. Waldo and R. W. Willson, of Harvard University, Prof. J. K. Ries and W. H. Pulsifer, of St. Louis, and Mr. F. E. Seagrave, of Providence, R. I., had fine weather for their observations, and met with general success. The four contacts were observed both with and without spectroscopes. The reversion of the spectral lines at totality, and the corona and its spectrum were studied, and five photographs, two of them polariscopic, were secured during totality. A number of sketches by local observers, for extent and form of corona, were made. The observers stationed by this party at McKinney, Allen, Cleburne, Waco and Dallas, were also generally successful in observing the duration of totality.

The following official telegrams were received at the U.S. Naval Observatory from the Naval Professors who were in charge of parties sent out to observe the eclipse :—

"*Las Junta, Col., July* 29.

"Good observations of the eclipse at Las Junta. Complete set of photographs. "ASAPH HALL"

"*Creton, W.T., July* 29

"Sky cloudless and observations perfectly successful. Six photographs of corona. Four polariscope photographs of corona and a fine drawing obtained. No ultra violet spectrum visible during totality. "W. HARKNESS"

"*Central City, Col., July* 29

"Whole eclipse perfectly observed. I find no Vulcan as large as sixth magnitude. Hastings finds consistent tangential polarisation. Drawings and photographs of corona. Diffraction shade bands observed. "E. J. HOLDEN"

"*Separation, W.T., July* 29

"Observations here very successful. Saw wings of light, supposed to be zodiacal light, extending 6° on each side of the moon in the direction of the ecliptic. Commander Sampson, U.S.N., found no dark lines in continuous spectrum of corona. Line 1,474 seen near sun's limb. No bright lines visible a few seconds after totality. "S. NEWCOMB"

"*Pike's Peak, Col., July* 29

"Fair weather after a week's storm. Observations successful in a marvellously clear sky. Corona resembling zodiacal light followed in one direction twelve diameters from the sun. "S. P. LANGLEY."

"Eclipse successfully observed at Dallas, Texas. All four contacts satisfactory. No inter-Mercurial planet seen with comet-seeker. Thin clouds. No stars seen near the sun. Corona very brilliant. Several drawings secured and photographs taken. "D. P. TODD"

It is telegraphed to the *Tribune* from—

"*Havana*, July 30.—Yesterday the total eclipse of the sun was visible in this latitude. The sky was perfectly clear, and complete observations were made. A report of the results obtained was expected to-day, but the scientific commission which took observations at Mariel, where the meridian passes, has not returned yet."

"*Quebec*, July 30.—The eclipse of the sun yesterday was witnessed under the most favourable circumstances."

"*Washington*, July 30.—The Signal Service Observer at Virginia City, Mont., reports to the Chief Signal Officer as follows:—'Our four telescopic stations have got all the contacts nicely, and three sketches of the corona.'"

OUR ASTRONOMICAL COLUMN

THE SATURNIAN SATELLITE HYPERION.—The following ephemeris of Hyperion is deduced from the elements which were calculated by Prof. Asaph Hall, upon his observations in 1875 with the Washington 26-inch refractor.

AT GREENWICH MIDNIGHT.

	Position.	Distance.		Position.	Distance.
	°	"		°	"
Aug. 24	268	147	Sept. 4	89	125
,, 25	263	93	,, 5	82	53
,, 26	243	37	,, 6	301	30
,, 27	125	36	,, 7	281	99
,, 28	105	93	,, 8	277	167
,, 29	100	146	,, 9	276	213
,, 30	97	189	,, 10	274	239
,, 31	96	218	,, 11	273	244
Sept. 1	94	228	,, 12	272	231
,, 2	93	216	,, 13	271	201
,, 3	92	181	,, 14	269	158

The plane of the orbit of the satellite is assumed to coincide with that of the ring, and as the earth has passed through the plane since the period included by the ephemerides which appeared in this column last year, the apparent motion of the satellite is now reversed, or the angles of position diminish. The above ephemeris includes an entire revolution of the satellite, and will serve, if necessary, to afford an idea of its position at any time during the present opposition, remarking that the satellite will be at its peri-saturnium at the following times: August 15·0939, September 5·4052, September 26·7165, October 18·0278, and November 8·3391.

OLBERS' STAR NEAR γ PEGASI.—On September 27, 1820, Olbers remarked a star of 6·7 m. not entered upon Harding's map, and which Harding had not seen during two comparisons of it with the heavens. It was somewhat brighter than 39 and nearly equal to 40 Piscium. Olbers accounted for it not having been observed on the meridian by the fact of its culminating within a few seconds of γ Pegasi. He watched it during the remainder of the season without noticing any change in brightness, and it was from the circumstance of his attention being thus directed to this quarter of the sky that he made an independent discovery of the comet of 1821. The star is in the *Durchmusterung* as 7·5 m., and was observed once by Argelander (October 24, 1861), the resulting position for 1855·0 being R.A. 0h. 5m. 41·8s., N.P.D. 73° 52′ 56″. Bessel did not observe it, but it is suspicious that he has a star 9m. only, preceding Argelander's position of Olbers' star 7·0s. and 3′ 58″ to the north, where the *Durchmusterung* has no star. It is a case for some one of those observers who are occupied with the variable stars to explain.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

A New Mineral

MR. BARNETT, of Chyandour, near Penzance, sent to the British Museum some time ago a mineral which appears to be new.

It is white with a slight tint of blue or greenish blue, and occurs as a layer sometimes of a quarter of an inch thickness, generally of a uniform fibrous structure, lining hollows or encasing quartz and other minerals. It is associated with earthy chlorite and quartz; iron pyrites, some copper pyrites, and mispickel being disseminated in the lode-material. Scorodite in boss-like aggregations also occurs with it, and in at least one instance the interior of the bosses of scorodite is filled with the mineral in question.

Dr. Flight has analysed it, and, though the mode in which the water is present has to be established more certainly, the general result of the analysis may be stated to be the assigning to the mineral a formula expressed, in "old style," as

$$3R_2O_3, As_2O_5 + 16H_2O,$$

R_2O_3 representing alumina with a notable amount of ferric oxide. The tint seems due to about one per cent. of copper, and a small amount of a sulphate is also present. The presence of the sulphate and general character of the composition would lead one to place the mineral with Pitticite, or "iron-sinter." But the formula, as Dr. Flight has pointed out, is so nearly that of an arsenical (instead of a phosphatic) Evansite that the true place of the mineral seems to be near the Evansite of David Forbes. I propose to call it Liskeardite, and to describe it more precisely hereafter. N. S. MASKELYNE

British Museum, Mineral Department, August 12

The Colouring of Birds' Eggs

WITHIN the last few months several notices have appeared in both England and Germany of supposed newly-discovered

colouring matters in the shells of birds' eggs. Permit me to call attention, through your journal, to a long and detailed paper on this subject published by me, about four years ago, in the *Proceedings* of the Zoological Society of London. Apparently the authors who have lately treated on this subject have not been aware of the existence of this paper. As far as they go, the facts they have described fully confirm my conclusions; but, as I have shown, different birds' eggs contain at least five perfectly distinct coloured substances, and not, as Liebermann says, only two. One of these is closely related to a product of the decomposition of hæmoglobin and another to the bile pigments, it and the bile of birds yielding the same well-marked product on oxidisation. H. C. SORBY

Kingstown, Dublin, August 6

The Limbs

IN his interesting papers on "The Genesis of the Limbs," which have recently appeared in NATURE, Mr. Mivart mentions that I have represented the limbs as modified portions of a primitively continuous inferior azygos fin.

That view was stated by me at length in a paper in the *Journal of Anatomy and Physiology*, 1871, vol. v., p. 59. It was formed from the following considerations among others—First, that the mesial, or azygos fins, are essentially double organs, being formed from the coalesced elements of the dorsal and ventral plates of the two sides, and being furnished with muscles, nerves, and blood-vessels, from the two sides. They may therefore be the representatives of organs which remain double, that is, of organs in which the lateral elements do not coalesce. Secondly, the limbs and the azygos fins do not co-exist in the same region. The limbs are formed where the ventral plates are kept apart and expanded by the presence of the visceral cavity, so that the elements which in the dorsal and postanal regions meet and unite into azygos fins, are here separated and grow out as lateral limbs. The ventral plates are, moreover, continued onwards beyond the outgrowing line of the limbs, and form the median portion of the visceral wall which lies beneath and between the limbs on the two sides. Thirdly, there is such a marked resemblance between the ventral fins in some fishes and the anal fins, that the transition from the one to the other is easy; the dagger-shaped pelvic bones being the representatives of two or more coalesced intraspinous bones and the ray bones of the one set being, in like manner, the representatives of the ray bones of the other.

Mr. Balfour in his admirable papers on "The Development of Elasmobranch Fishes," in the *Journal of Anatomy and Physiology* shows (vol. xi. p. 133), that the limbs are the remnants of continuous lateral fins, and that the ridges from which they are developed are in every way like the folds from which the unpaired fins are formed, but the development and growth are confined to two special points on each side instead of being continued, as in the case of the dorsal and anal fins, along a greater length of the fold. He further remarks that, externally, they closely resemble the unpaired fins, and both their position and nervous supply indicate that they do not belong to one special segment of the body. The lateral ridges, from which they are developed, I conceive to be the continuations of the diverging lateral halves of the essentially double ridge of the caudal fin kept asunder by the presence of the visceral cavity. These are but little separated in the position of the ventral fins, and are more so in the position of the pectoral fins. If this be so, the limbs are specialised differentiations of primitively continuous lateral folds—of portions, that is, of the diverging plates of the median fold from which the caudal fin is developed.

I cannot, however, assent to Mr. Mivart's view that the limbs are mere appendages to the axial system, or admit that either they or the limb girdles are the result of centripetal growth, "due to the in growth of originally superficial structures—exoskeletal hardenings which have grown inwards and become endoskeletal." The limb girdles are found in the same plane of the mesoblast as the ribs, and are, as I have shown in the "Anatomy of the Cryptobranch" (*Journal of Anatomy*, vol. vi. p. 9, and *Observations on Myology*), though not necessarily the serial homologues of the ribs, yet like them the result of ossification in the ventral transverse intermuscular septa. As they grow out they carry before them envelopes not only of skin, but of muscle, derived from the body-wall, which become differentiated according to the requirements. Moreover, although the ridges from which they are primarily developed may appear at first as epiblastic projections, these are soon supplemented by accumulations of mesoblastic tissue in which the components of the limbs are chiefly formed. G. M. HUMPHRY

Cambridge, August 1

The Darkness of Caverns

THE impenetrable darkness of caverns has been for a very long time a recognised fact, without its cause having been satisfactorily explained. This darkness vanishes but partially before torch-light, and that only in a very limited radius. I, in my explorations in the caverns of Spain, had also noticed this circumstance, and now that I have verified it in others in Switzerland, I venture to think that I have found the explanation of this phenomenon.

The walls as well as the roof and floor of caverns are continually covered with moisture, which works without interruption in condensing the corpuscles that float in that circumscribed space. It thus performs the same function that the glycerine does which varnishes the sides of the crystal box by means of which Prof. Tyndall obtains an optical vacuum, the light diffusing itself imperfectly from want of those atoms which act as reflecting bodies. I have had occasion to verify my supposition by scattering around the torch very fine dust of different substances. The brightness diffused itself regularly all the time that the dust maintained itself in the required state of closeness and fluctuation, and vanished again slowly as the dust spread or deposited itself. The earth or common dust is the one which, in my experience, has produced the best effect.

SALVADOR CALDERON

Scent and Colour in Flowers

THE extension of our perceptive faculties of sight and hearing by various optical and acoustical instruments may enable us to comprehend the possibility of these faculties existing in other creatures to a degree so far surpassing ours as to seem a difference almost of kind. So the sight of the vulture would seem to be paralleled by the faculty of smell in moths, as evidenced by the detection of distant females by males. It would seem probable that the sense of smell may guide insects at a far greater distance than that of vision; for a consideration of the structure of the eyes of insects leads to the belief that they are not capable of forming clear images of distant objects. While, then, the scent of its blossoms may attract insects to a plant, their colour will act as a subsequent guide to the individual flowers, just as variegations undoubtedly act as honey-guides when the insect reaches the flower. This view is borne out, firstly, by the undoubted connection between perfume and pollination, shown by Morren in the case of the orchid *Maxillaria*, whose aromatic perfume lasts till pollination; and, secondly, by the well known connection of odour both with colour and with natural groups, white flowers being mostly sweet-scented, brown and orange ones most fetid. The insect could thus identify species before seeing them. Mr. Wallace has been, perhaps justly, blamed by a writer in the *Gardener's Chronicle* for saying that brightly-coloured flowers are seldom scented, and Dr. Taylor by "J. S. G." (NATURE, vol. xviii. p. 277), for saying that white flowers open mostly at night. It would, I think, be truer to say that few flowers are both variegated and scented, *i.e.*, that scented flowers are mostly monochromous, and that the majority of night-blowing flowers are white. The latter is a very different matter from saying that the majority of white flowers are night-blowing. We can perceive with difficulty that one part of a flower is more scented than another, yet scent may replace the dots and point-indicating lines of variegation to the senses of an insect. Nature not only often effects one purpose by divers means, but also uses one means for divers ends; so just as colour exists in plants, not only to attract insects, we can understand it being absent in some white flowers simply as a phenomenon of degradation and not as one of specialisation. The dog-rose, white convolvulus, and daisy, mentioned by Mr. Gardner as closing at eventide, are all scentless. The first, according to Dr. Hermann Müller, is visited by six hymenoptera, two diptera, and twelve coleoptera. The convolvulus does not close till between eight and ten P.M., and re-opens by moonlight. It is visited by two diptera, *Podura*, *Thrips*, one coleopteron, two hymenoptera, and the *Sphinx con-*

volvuli, L. This is a dusk-loving hawk-moth, which also visits the honeysuckle. The daisy is visited by nine hymenoptera, thirteen diptera, three coleoptera, and two lepidoptera, viz., the least meadow brown and the common blue butterflies. Many flowers, like *Lychnis vespertina*, remain open without exhaling their perfume, and I think Mr. Gardner will find that most of the subduedly-coloured flowers which are open at night give off most perfume, and are visited and fertilised by moths rather at dusk than in the dark, whilst the white ones remain fragrant still later. The clearly-cut discs of white of *Lychnis vespertina* are the last objects our eyes can often discern on a midsummer night's ramble. Of course variegation on the moths themselves would be as useless, from the point of view of sexual selection, as on the flowers from that of insect-fertilisation. Though it is to a certain extent true that like causes produce like effects, in investigations into phenomena so complex in their etiology as those of biology we must, I think, be more mindful that the converse that like effects are the result of like causes by no means necessarily follows. G. S. BOULGER

11, Burlington Road, Westbourne Park, W.

SCIENTIFIC SERIALS

Rendiconto delle Sessioni dell' Accademia delle Scienze dell' Istituto di Bologna.—The more important papers read at the Academy during the academical year 1877-8 were the following:—On the metamorphosis of plants, by Prof. G. B. Ercolani. The author specially refers to the transformation of a cryptogamous plant of the genus *Uromyces* into a phanerogamous dicotyledon, *Cuscuta europæa* L., and the return to the primitive cryptogamous form apparent in the seeds and branches of the *Cuscuta*.—On the velocity of light in transparent magnetised bodies, by Prof. A. Righi.—On the concentration of a magnetic solution near the pole of a magnet, by the same.—On the curves with equal normal principals, by Prof. A. Fais.—On some researches to ascertain whether from cadaverous matter, from albumen, or yolk of egg, volatile phosphoretted products are evolved, and on an excellent means of discovering free phosphorus in minute quantities, by Prof. Francesco Selmi.—Crystallographical researches, by Prof. Carlo Maragoni. The author describes some theoretical and experimental researches, and their application to the natural history of crystallised minerals.—Contributions to the flora of the Bolognese province, by Prof. Girolamo Cocconi.—Researches on the varying nature of the caloric emitted by various bodies heated to 100° C., by Prof. E. Villari.—On four species of noxious insects, inhabiting pines and birches, by G. Bertoloni.—On the calcareo-siliceous conglomerates of Sasso Cardo and of the Rio Fonti, and on the origin of pyrites, by Prof. Domenico Santagata.—Critical observations regarding some recent Italian crystallographical publications, by Prof. Luigi Bombicci.—On comparative psychogeny and the attempts to establish a zoopsychological classification, by Prof. Siciliani.—Note on a theorem in the theory of binary forms, by Prof. Francesco d'Arcais.—Observations regarding the existence of rudiments of upper canine teeth and incisors in the embryos of oxen and sheep, by Dr. G. P. Piana.—Anatomy and physiology of *Surilla Neapolitana*, by Prof. Salvatore Trinchese.—On a problem in undetermined analysis occurring in the geometrical theory of the transformation of plane figures, by Prof. F. P. Ruffini.—Geometrical studies on the molecular equilibrium, by S. Canevazzi.—On some gigantic bird-remains, probably belonging to *Aepyornis* or *Ruck*, by Prof. G. Bianconi.—On the internal texture of the eye of *Sphinx*, by Prof. G. V. Ciaccio.—On the origin and structure of the humor vitreous, particularly in the embryos of the two first classes of vertebrates, by the same.—On the whale of Taranto and the *Macleayius* of the Paris Museum, by Prof. G. Cappellini.—On the emery from S. Lucca and Paderno, and its fossils, by Dr. Lodovico Foresti.—On the reticular structure of the red corpuscles in the blood of *Torpedo*, and of the nerve substance of frogs, by Prof. Salvatore Trinchese.—Researches on the central nervous system of *Squilla Mentis*, by Dr. G. Bellonci.—Results of experiments made at the Royal Botanical Gardens of Bologna upon some species of *Eucalyptus*, and upon a new grass recently introduced in Italy, by A. Bertoloni.—On the nerve ends in the skin of bat's wings, by Dr. Agostino Rossi.—On the formation of protoxide of iron in the metallic state in the wet way, by Prof. Francesco Selmi.

SOCIETIES AND ACADEMIES

MANCHESTER

Literary and Philosophical Society, April 2.—E. W. Binney, F.R.S., F.G.S., president, in the chair.—On aurin, by R. S. Dale, B.A., and C. Schorlemmer, F.R.S.—The origin of some ores of copper. Part II., by Charles A. Burghardt, Ph.D.

April 16.—Note on the occurrence of dioptase on Chrysocolla, from Peru, by Charles A. Burghardt, Ph.D.—On the internal cohesion of liquids and the suspension of a column of mercury to a height more than double that of the barometer, by Prof. Osborne Reynolds, F.R.S. The object of this communication is in the first place to show that certain facts already fully established afford grounds for believing that almost all liquids, and particularly mercury and water, are capable of offering resistance to rupture commensurate with the resistance offered by solid materials; in the second place to describe certain experimental results which, as far as they go, completely verify these conclusions and subvert the general ideas previously mentioned as to the limits to the height to which mercury can be ssupended in a tube or water raised by suction; and, in conclusion to explain the nature of the circumstances which have resulted in the practical limits to these phenomena.—On the estimation of hyposulphites and sulphites, by J. Grossmann, Ph.D.—Note on the action of iodine trichloride upon carbon bisulphide, by J. B. Hannay, F.R.S.E., F.C.S.

PARIS

Academy of Sciences, August 5.—M. Peligot in the chair.—Probable new observation of the planet Vulcan by Prof. Watson, by M. E. Mouchez.—On the orbito-ocular phenomena produced in mammals by excitement of the central end of the sciatic nerve, after excision of the superior cervical ganglion and the superior thoracic ganglion, by M. A. Vulpian.—New note on the progress of phylloxera in the two departments of Charente, in connection with the last communication of M. de la Vergne, by M. Bouillaud.—Rate of propagation of excitations in the motor nerves of the red muscles, abstracted from the power of the will, by M. A. Chauveau.—On the fundamental covariants of a cubo-quadratic binary system, by Prof. Sylvester.—On the baking of plaster, and on the manufacture of plasters by slow coagulation, by M. Ed. Landrin.—No mycelium intervenes in the formation and in the normal destruction of swellings developed under the influence of phylloxera, by M. Maxime Cornu.—On the abnormal solubility of certain bodies in soaps and alcaline resinates, by M. Ach. Livache.—On the vibratory forms of solid bodies and of liquids, by M. C. Decharme.—Note on the intra-Mercurial planet, by M. Gaillot.—Results of solar observations during the second quarter of 1878, by M. Tacchini.—Action of chloride of zinc on methylic alcohol; hexamethylbenzine, by MM. Le Bel and Greene.—Researches on the connections which exist between the weight of various bones of the Biscayan whale (*Balæna biscayensis*), by M. S. de Luca.—On *Prosopistoma punctifrons*, Latr., by MM. E. Joly and A. Vayssière.—On the influence of atmospheric electricity on vegetation, by M. L. Grandeau.—Age of the Mont-Dol bed; constitution and formation of the low plain called Marais de Dol, by M. Sirodot.

CONTENTS

THURSDAY, AUGUST 22, 1878

BRITISH BARROWS

British Barrows. A Record of the Examination of Sepulchral Mounds in various Parts of England. By William Greenwell, M.A., F.S.A. Together with *Description of Figures of Skulls, General Remarks, Pre-historic Crania, and an Appendix.* By George Rolleston, M.D., F.R.S. (Oxford: Clarendon Press, 1877.)

THE pre-historic inhabitants of Europe are now exciting an interest in the minds of thoughtful men which, twenty years ago, would have seemed impossible, and which can no longer be ignored by the historian. The story of man in Great Britain is rapidly being unfolded, principally by the careful and scientific exploration of the various remains which are eloquent of the condition of things that passed away before the art of letters was known in the north; and, among those who have been mainly instrumental in bringing this about, Mr. Greenwell will ever deserve a foremost place. He has devoted years of patient labour to the accumulation of facts; and in the present work he records the results of the examination of upwards of 230 burial mounds, the greater number being in the wolds of eastern Yorkshire. He has also had the advantage of the aid of Prof. Rolleston, by whom the human remains—fortunately now safe from dispersal, in the Oxford Museum—have been classified and described, in the latter part of the book.

The general results of the exploration are thrown into an introduction of 130 pages, carefully written, and highly suggestive of new lines of thought. The barrows vary in size and shape very much as the graves and tombs in our own graveyards, where the rich man's memory is preserved by the large mausoleum, while the poor man's resting-place is marked merely by the little mound of earth, soon to be lost in the general surface. Those in the Yorkshire wolds are either circular or "long," the former being the more abundant, and are frequently surrounded by a ramp or a ditch. In some cases this was within the base of the barrow, and very generally it was incomplete. "This very remarkable feature," writes Mr. Greenwell, "in connection with the inclosing circles, is also found to occur in the case of other remains which belong to the same period and people as the barrow. The sculptured markings engraved upon rocks, and also upon stones forming the covers of urns or cists, consist in the main of two types—cup-shaped hollows, and circles, more or less in number, surrounding in most cases a central cup In almost every instance the circle is imperfect, its continuity being sometimes broken by a duct leading out from the central cup; at other times by the hollowed line of the circle stopping short when about to join at each end. The connection of the sculptured stones, if so they may be termed, with places of sepulture brings them at once into close relationship with the inclosing circles of barrows, and it is scarcely possible to imagine but that the same idea, whatever that may have been, is signified by the incomplete circle in both cases. The rings of gold and bronze, of various shapes, some of which in their construction show that the penannular form is not caused by the requirements of their use, appear to represent the same incomplete circle. In fact, if some of the gold rings were figured upon stone, they would appear in the very similitude of the circular rock sculptures." Our author suggests that it may have been intended to prevent the exit of the spirits of those buried, though in that case it is hard to see why the spirit should not have found its way out through the opening. It seems more probable that if the barrow represented the hut inhabited by the living that the circle round it would represent the trench, or the inclosure of the hut, and that this would necessarily be incomplete, to allow of access to the habitation.

The dead were buried in the barrows of the wolds very generally in the condition and clothing in which they died, the proportion of cases of inhumation to those of cremation being as 301 to 378, or about 80 per cent. In all probability both customs were carried on simultaneously, as was the case in ancient Rome, where, however, inhumation was mainly confined to the lower classes. Where inhumation had been practised the body was buried in the crouching posture in which life had departed, and which would be natural where the sleeping place was not well protected against the cold, and the covering was scanty. This interpretation, due to the ingenuity of Mr. Evans, is most likely true.

The burnt and broken bones of various animals used for food in the barrows are probably the remains of funeral feasts held at the time of the interment, or from time to time afterwards, or they may be the remains of food offered to the dead. Splinters and various manufactured implements of flint, and fragments of pottery, also occur sometimes in great abundance, and probably symbolise some religious idea. Fragments of flint were used in interments at least as late as the fourth and fifth centuries after Christ in this country; for they were found in considerable quantities inside the oaken coffins in the Romano-British cemetery, referable to the above date, explored at Hardham, Sussex, in 1866.

Where cremation was practised the funeral pile was sometimes kindled upon the spot, which was afterwards occupied by the barrow, but at other times the ashes of the dead were collected and deposited somewhere else. In several barrows curious perforated vessels of pottery, or "incense cups," were met with, which may have been used to convey the sacred fire to the pile. The ashes of the dead were placed in urns sometimes highly ornamented, and those things which delighted the dead most or were most useful to him, were deposited in the tomb. Flint scrapers, flakes, arrow-heads, beads, hammer-axes, celts, domestic pottery, and a few bronze articles. The number of objects buried in each barrow varied according to the wealth of the dead and the estimation in which he was held by the survivors.

The animal remains in these barrows prove that the ancient inhabitants of the Wolds were no rude savages living mainly on the chase. They possessed flocks and herds, consisting of well-known domestic breeds—the small Celtic shorthorn, now represented by the mountain cattle of Wales and Scotland, the pig, the horned sheep or goat, the horse and the dog, the two last being

the rarest. They also ate venison of the stag (*Cervus elaphus*). Their place in the archæological scale of culture is fixed by the few and simple forms of bronze articles in the barrows, the simple wedge-shaped axe, and the short broad dagger, in association with various articles of stone, coupled with the absence of the higher bronze types, such as the sword. They belong to the early bronze age. The absence of the sword is also noticed in the tumuli of France, referred by Mr. Chantre to the same horizon. At this time the knowledge of bronze was gradually finding its way northwards from the Mediterranean centres, and the simpler forms preceded the more complex and elaborate.

Nor are we left in doubt as to the ethnical relations of these ancient Yorkshiremen. Prof. Rolleston's elaborate examination of the crania and skeletons reveals the fact that the two types—the small long-headed or "Iberian," and the tall robust round heads, or "Celtic," which have been traced by Thurman, Huxley, Busk, and myself, from Scotland to the Mediterranean, and from the Rhine to the Pillars of Hercules, occur in the round barrows side by side in intimate association. The former of these, "the Silurian" of Prof. Rolleston, is considered in this work as the older of the two. According to Dr. Thurman it was dormant in Britain in the Neolithic age, at the close of which it was invaded by "the Celtic" or "the Cimbric" of Prof. Rolleston. The truth of this view is confirmed by the fact that the dead of these two races rest peacefully together in the round barrows of the wolds referable to the early bronze age. In concluding this review it remains merely to say that this valuable work fills a void in the archæological record of Great Britain, and that it contains a larger mass of accurately observed facts than any book hitherto published relating to the Bronze Age in this country.

W. BOYD DAWKINS

THE ECLIPSE OF THE SUN

THE following letter appears in the *Daily News* of Tuesday, from its Special Correspondent, dated Manitou, Foot of Pike's Peak, Colorado, August 2:—

Since the eclipse I have had no easy time of it. To gain accurate information concerning the work done along the long north and south line, on which, as your readers already know, the stations were located, it was necessary to return over the crest of the Rocky Mountains at Sherman (8,200 feet) down to Cheyenne, and then go as far south, by the Colorado Central line, as Colorado springs, passing Denver on the way. Manitou, whence this letter is written, is about six miles from the latter place on the old and famous Ute trail, a road much improved in these later days for the needs of the enormous traffic connected with the mines at Leadville. The place itself is lovely—so lovely, indeed, that although it is over 2,000 miles from the eastern seaboard, the hotels and baths (iron and alkaline) formed but a nucleus for a large encampment, tents dotting the slopes in all directions. The newly-published geological map of this region with which Prof. Hayden, with his wonted generosity and forethought, had supplied some of the astronomers, enabled them to revel with more or less success in the wonders of the famous "Garden of the Gods," and Glen Eyrie, where the rocks put on everything they can in the way of uncommon arrangement and colour. Vertical beds of limestone and sandstone 300 feet high, and not many yards thick, even at the base—each spire excelling the other in some glorious colour, including all those we see in an English sunset—is a sight never to be forgotten, even if Pike's Peak, with its patches of snow, were not seen through such gigantic and weird portals.

Here, then, we met the Pike's Peak and the Las Animas parties, all rejoicing over their success. Two of the former—General Myer, the creator and chief of the wonderful meteorological signal surface, and Prof. Cleveland Abbe—have suffered somewhat from their sudden transference from the plains to a height of above 14,000 feet. The latter, indeed, had to be transferred on a litter, by the General's order, to a station at mid-height before the eclipse. But the journey to Manitou does not exhaust my wanderings. Much was expected from Prof. Holden's party at Central City, easily reached from Golden City by one of the most wonderful railways in the world. To Central City, therefore, I went up to a point called Black Hawk. The railroad is filched from the bed of a stream at the bottom of a true cañon. Here rail and river together are not fifteen yards broad, and precipitous cliffs 1,000 feet high shut out the light. Here, again, the cliffs recede, the river bed widens, and miners—American and Chinese—are engaged in extracting the precious gold from the turbid water and its bed, some of the miners making in this way 1,500 dollars a week. From Black Hawk to Central City there is no possible approach except up the side of a steep hill. The railway accepts the condition, and zigzags up—now engine, now cars, foremost—till the nest of stamping mills and smelting furnaces at Black Hawk looks like a speck below.

The party at Central City had been as fortunate as the rest in the matter of weather, and their station, on the top of Teller's Hotel, in the middle of this strange mountain community, had proved a very convenient one. After my last letter to you, I became acquainted with the fact that Mr. Burnham, so well known for his work on double stars, and whose eye seems to be keen as was formerly Dawes's among us, was going to observe the eclipse on behalf of the *Chicago Times*—a piece of newspaper enterprise worthy of imitation. I tried to find him when I passed through Denver to Prof. Young's camp, but was unsuccessful, though I was fortunate enough to see Prof. Young himself, whose party was second to none either in *personnel* or in their means at the disposal for securing good results.

My telegram to you, immediately after the eclipse, was necessarily based upon the most meagre materials, and laid emphasis upon those parts of the combined attack which were most strongly represented at the northern stations. Still, now that the complete information is before me, I have very little to alter, so singularly and exceptionally do the observations support each other in the main; and I say in the main, because on some points there is enough disaccord to make the astronomers already look forward to the next eclipse. Without going too much into detail I will go over the ground in the light obtained by personal communication with the chiefs of the different parties, with the single exception of Prof. Hall, who is a most skilful and competent observer, and to whose opinion, as the discoverer of the satellites of Mars, the greatest weight is to be attached. The corona was much less brilliant than usual. Those who have observed the greatest number of eclipses are strongest on this point. The contrast, perhaps, is most striking between this eclipse and that of 1871 observed in India. Now that this fact is recognised, the naturalness of it is apparent to everybody. We know that the sun's activity and the various meteorological and magnetical conditions on our own earth which depend upon or are connected with it wax and wane every eleven years or so. This is termed the sun-spot period. Thus the sun was very active in 1871, and it will be again very active in 1882. It is very sluggish now, and it will be sluggish again in 1889. In 1871 we had many spots, many prominences, many mag-

netic storms and auroræ, heavy rainfall, and, let me add, no famines to speak of. Associated with these we had a large corona. This year there are no spots, the prominences are rare, the magnets were never so quiet, there are no auroræ, and we are passing through a famine period. Associated with these we had a small corona. Hence it is that the astronomers agree that this year another connecting link has been added to the chain which binds together the solar changes.

On another point mentioned in my telegram the evidence is overwhelming. Since the use of the spectroscope in this branch of inquiry the spectrum of the corona has been observed to be a mixed one—that is, bright lines as well as a continuous spectrum have been traced in it. In 1869 and 1871 these lines were very bright to the eye—much brighter than the continuous spectrum, while in 1875, when the spectrum was photographed for the first time, the record showed the lines to be much brighter than the continous spectrum. Now, as it generally happens that when in our laboratories we study gaseous spectra there is a faint continuous spectrum accompanying the lines, it was thought that the spectrum of the solar corona might have a gaseous origin exclusively. This year's observations have quite dispelled this idea, for the two elements of the corona spectrum, to which attention has been drawn, instead of varying directly, vary inversely. The continuous spectrum has been seen and photographed by itself, without any bright lines. Now for the interpretation of this hieroglyphic language. The gases which were high up in the sun's atmosphere at the last epoch of maximum sun-spots (1871), have almost entirely retreated to a lower level; as these gases are to a large extent carriers of heat from the interior to the exterior of the sun, the exterior is cooler in their absence, and indeed cool enough to allow it to hold in suspension a larger percentage of solid and liquid particles to which the continuous spectrum is due. A natural suggestion touching these particles is that they consist of meteoritic matter, and all the phenomena to which attention has been drawn can be explained by supposing that these meteorites surround the sun, and that at the minimum sun-spot period they can exist lower down in consequence of the reduced temperature of the sun's atmosphere at that time. If this suggestion, which is the one in favour out here, be endorsed we have a partial cause of reduced solar radiation at that time. I cannot quit this part of the subject without referring to the wealth of astronomical resources which have been brought to bear upon it. Paper astronomers have been now for a long time, I doubt not, very lucratively employed in proving that it was impossible to do exactly what has been done. Dr. Draper, who ranks deservedly high among solar observers, bears off the palm by the strength of his attack. He used a camera of six inches aperture and of only twenty-one inches focal length, and, by means of a Rutherfurd grating two inches square, obtained a photograph of the corona and two of its spectra with the same instrument. The plates were exposed during the whole of totality, and it is encouraging for future work to know that much less exposure would have sufficed to secure the precious records, although the continuous spectrum recorded is the most difficult to obtain. Prof. Harkness used an instrument of nearly equal power, though of slightly different arrangement. The smallest instruments employed—ordinary portrait cameras with a grating in front—also gave the same result, though, of course, on a much smaller scale.

Next comes the endorsement of the fact that the coronal light is due partly to solar light—that all the light it sends to us is not its own. Prof. Barker, with Dr. Draper's party at Rawlings, though he saw no bright lines, saw dark ones in abundance; and Prof. Morton got distinct traces of radial polarisation, thereby endorsing the result obtained in 1871. The object of these observations by the polariscope is to determine whether and in what plane light is reflected; and for the light to be reflected to us by particles in the sun's atmosphere we must imagine our eye, the centre of the sun, and the particle to lie in one plane. If, further, we imagine an infinite series of particles surrounding the sun, we shall have an infinite series of these planes, and each radius of the sun will lie in one of them. I have been particular in stating this, because Prof. Hastings, a great authority in optical matters, declares that the reflection takes place at right angles to these radial planes; in other words, he declares that the polarisation is tangential and not radial. As has been well remarked, this result is easily explained by supposing ice crystals to be present in the sun's atmosphere, and no other solution lies on the surface. I believe no one is more astonished than Prof. Hastings at his observation; and here again we see the necessity of relaxing no efforts and letting slip no opportunity of garnering the observations with which eclipses alone supply us.

Finally, as to the construction of the corona. Numberless records of this, both visual and photographic, have been secured, and as usual, though there is almost perfect agreement as to the structure of the lower portion, the rays and streamers have been very variously observed. The upper and lower portions of the sun were graced by the most exquisite tracery, bending over right and left like plumes of ostrich feathers, the intervals between them being of a delicate blue. Near the solar equator the structure was not so obvious. Still, it was there, for Prof. Bass saw it come out *and pulsate* after he had fixed his eye on one portion for two minutes. The structure was distinctly less filamentous than in 1871. With regard to the streamers there appears to have been two sets—one along the ecliptic, giving rise to the appearance of a wind vane seen by one set of observers; another, at right angles to this, seen by another set. Neither of these systems of streamers was visible in a telescope, though the base of the ecliptical ones appears on the photographs. On this point the greatest weight must be given to the observers on Pike's Peak, 14,147 feet high. Most fortunately the weather there as elsewhere was superb, and the corona was seen as it was never seen before. The clearness of the sky in this region when the weather is good is simply wonderful. The Milky Way seems to have deep holes in it, and the individual stars shine out. Even at Rawlings Prof. Watson could see the satellites of Jupiter with the naked eye. At Pike's Peak General Myer saw the corona for fully five minutes after totality was over, while in India, in 1871, the much brighter corona was seen at sea-level for only three minutes after the eclipse was over. Prof. Cleveland Abbe, who, as already stated, observed below the Peak, saw the ecliptic streamers extending to a distance of twelve solar diameters, while he saw nothing of the north and south ones, pretty conclusive evidence in favour of their subjectivity, and from their appearance he has little doubt of their being meteor streams. Prof. Newcomb saw the ecliptic streamers almost equally well at Separation by the help of a novel contrivance. He had a disc erected on a high pole at some distance from his telescope so that he could momentarily cover the dark moon and corona and observe the external phenomena with the naked eye. In this way he saw the streamers extending nearly as far as Prof. Abbe did. Still he explains them differently. He considers that they indicate the true zodiacal light. No doubt there are difficulties surrounding both suggestions, and here again we see the need for future thought and work. The regions in which the various parties were located enabled many of the connected phenomena to be observed as they had never been before. From Pike's Peak, with its horizon of 150 miles on all sides, the shadow of the moon sweeping rapidly through the air was a very tangible thing and seemed to

be solid enough to sweep all before it. It was noticed on the air and on the very buttresses of the Peak that the boundary of the shadow was strongly coloured in the prismatic order. The wide extent of Alcali Plains round Separation and Preston—plains broken by nothing save the wonderful avenue of telegraph poles along the railway, and the solitary water tank and telegraph operator's house, enabled these and the associated phenomena to be seen well there also, and there were just sufficient clouds above the eastern horizon to bring out into strong relief the retreat of the shadow through the air, while during the eclipse the leaden light on the desolate plains gave rise to an intense feeling of loneliness and weirdness. The daring genius of Edison has left its mark on this eclipse. So soon as he had completed his tasimeter he saw its applicability to eclipse work in determining the presence of heat waves in the radiation from the corona. I had the rare privilege of seeing the great inventor at work gradually increasing the sensitiveness of his wonderful instrument with the most consummate knowledge of principles and contempt for elaboration, until at length during the eclipse he was rewarded by seeing the speck of light on the attached Thomson galvanometer give a decided swing from its zero on the dark moon when the image of the corona was brought on the fine slit in the plate which shielded the tasimeter from its surroundings. The instrument was too new to succeed in other hands, even those of Prof. Young; but he was not to be beaten. Driven from one instrument he took up another—the thermo-electric pile—and was rewarded by finding a heat line in the ultra-red. This opens out another new line of work in future eclipses; and so science advances. Rumour here says that one distinguished astronomer remained away because he considered eclipses "played out." What a lesson he has learned ere this touching the need of that receptivity to which I referred in my former letter!

Mindful of your space I must here conclude my statement of the more salient solar phenomena observed, the detailed discussion of which will occupy the astronomical world for some time to come. I shall have been very unfortunate and unworthy of my post if I have not succeeded in convincing your readers that several important advances have been made, and that the work done on the eclipse of 1878 will rank high in astronomical records. I have not yet, however, quite finished the story I have to tell. Students of solar physics may congratulate themselves that the gravitational astronomers will in the future insist upon having their finger in the eclipse pie. In my telegram I was enabled to announce the position of the body observed by Prof. Watson near the sun. Since the telegram was dispatched the matter has been seriously discussed by Prof. Holden and others, and little doubt remains that a new major planet has been discovered, if, indeed, Vulcan has not been refound. Prof. Watson's work has been acknowledged on all hands to be a veritable *tour de force*. The Naval Observatory instructions contained a map of all stars near the sun down to the seventh magnitude. Prof. Watson determined to review all these, and provided his equatorial with paper circles on which to mark the difference of right ascension and declination between any body not marked on the chart and the sun. After the eclipse, and before the instrument was dismounted, Prof. Newcomb and Mr. Lockyer saw the precious record *in situ*. It was here at Manitou that Prof. Watson again reviewed his work, and despatched a telegram to the Smithsonian Institution corroborating the one I had previously sent to you.

The English observers are full of appreciation of the reception they have met with from their American *confrères*. Prof. Thorpe and Dr. Schuster were the guests of Prof. Hall's party at Las Animas. Mr. Lockyer, invited by General Myer to Pike's Peak, by Dr. Draper to Rawlings, Prof. Newcomb to Separation, and Prof. Wright to Las Animas, decided for Rawlings, where Dr. Draper placed all the resources of his observatory at his disposal. A thing unknown in England—that is, a travelling railway photographic car—being, however, placed at the disposal of the astronomers by its proprietor, Mr. Silvis, it was decided at the last moment to establish another station, and on the morning of the eclipse Prof. Watson and Mr. Lockyer proceeded to Separation, a station on the Union Pacific Railway, between the eclipse camps there and at Preston.

The *Times* correspondent, writing from Pueblo, South Colorado, July 31, states that Dr. Schuster brought out with him a couple of fluorescent eye-pieces, with a view of re-determining the position of the lines he observed in 1875; but a too-confiding faith in the tender mercies of the baggage-men in charge of the instruments led to the utter destruction of one of the eye-pieces, while the other was so injured that it was impossible to get it into order in time for the eclipse. Indeed, nearly all the instruments, in spite of most careful packing, suffered more or less damage, either during their canter—for such it can only be called—along the Western railroads, or at the hands of the "baggage-smashers" who took charge of them at the depôts. The baggage arrangements of American railroads are doubtless perfect in theory; but the practical application of them is simply ruinous to scientific apparatus.

The newspapers are filled with allusions to the phenomenon; its effect on animals, on the colours of objects, and on their visibility, was noted everywhere. The darkness was far from being so great as was anticipated; the decrease of temperature was, however, very considerable. At La Junta it fell from 96° to 80°; at the time of last contact it had again risen to 93°. In Pueblo the fall was as great as 23½°—*i.e.*, from 103° to 79·5°.

The following interesting and amusing account of the eclipse observations of Prof. Lewis Swift, by a reporter of the *Rochester Democrat*, appears in the *New York Tribune*.

Prof. Lewis Swift returned from Denver, Col., Saturday evening, bringing splendid trophies of his skill as a searcher of the heavens. That his discovery was not duly reported by the Associated Press is chiefly owing to Prof. Swift's modesty in heralding the results of his labours and his desire to carefully determine the significance of his observations before making public announcement. Yesterday afternoon we visited Prof. Swift at his pleasant home in Ambrose Street, and learnt from him the story of his visit to Denver and observations of the total eclipse. Prof. Swift said:—"Up to the time of the eclipse the prospects for a clear day were very poor. The nights were clear, but it was cloudy, and rainy every afternoon. Sunday afternoon there was a clearing storm, with hail and drenching rain. Up to Sunday afternoon an unprecedented amount of rain had fallen in the region. On Monday morning there was not a cloud in the sky, and all predicted a clear day, and we had it. The final preparations were made as rapidly as possible. A. C. Thomas had arrived at the eleventh hour from Chicago with a telescope and spectroscope attachment. Prof. Hough had brought a small telescope with fine lines stretched across the object-glass, and a micrometer eyepiece, for the purpose of measuring the corona. I had secured from the Mayor the services of a police-officer to keep the grounds about the instruments clear from spectators. According to Washington predictions the eclipse was to commence at 4h. 7m. 50·4s. According to Prof. Colbert's calculations it would begin at 4h. 10m. 50s., Washington time—a difference of three minutes. Prof. Colbert's prediction, was proved to be very nearly correct. About half an hour before the eclipse was to

begin our party took their positions. I was about twenty feet to the south of Prof. Hough, who was in a group consisting of himself, Prof. Colbert, and Prof. Easterday, who used the telescope for observing the corona. To the west was a class of fifteen young ladies from Denver, instructed by Prof. Colbert to sketch the corona. South-east of my position Mr. Thomas was stationed.

"When arranging my instrument I made the post very low, exciting much comment by my companions. I told them I intended to lie on the ground during the observations, this being a position I had found the easiest in my experience of twenty years' comet-seeking. I spread a carpet on the earth and had a great advantage over the other members of the party, who were obliged to assume constrained positions, which tended to unsteadiness of vision. Seated by me was Daniel Drummond, with an accurate time-piece, set by the chronometer a few minutes before the first contact. E. D. Smith, an old acquaintance, whom I met in Denver, recorded the time of each event as I called it. Mr. Drummond is an experienced engineer, and counted the seconds with great accuracy.

"As I arranged my telescope for the first event the wind was blowing in fitful gusts from the south-east, shaking our instruments. To prevent my instrument from swaying I tied a long stick to it, about a foot above the eyepiece, the other end being braced against the ground and free to move only in one direction. This was a blunder to which I owe the discovery of a stranger, which I am inclined to think is Vulcan. As the sun moved the eye end of the telescope moved to the east. The stick would not allow any backward movement, and when I attempted to observe the sky to the east of the sun I could not. This confined my area of vision to a small distance west of the sun. But to return to the observation. My observation of the first contact was four seconds later than the observation of Prof. Colbert. The following is my record of events by Washington time:—

	h.	m.	s.
First contact	4	11	18
Bailey's beads	5	20	22
Beginning of totality	5	20	38
Corona first seen	5	23	17
End of totality	5	23	26
End of eclipse	6	26	35

"The watch was one second slow of the chronometer at the first contact, two seconds at the end of totality, and four seconds at the end of the eclipse. Before the eclipse began I had made up my mind to observe the general phenomena, the corona, protuberances, and Bailey's beads for about half a minute at the beginning of totality. I designed a minute and a half for a search for Vulcan, and the remainder, some forty odd seconds, to observe the phenomena at the end of totality.

"About one minute after totality two stars caught my eye about three degrees, by estimation, south-west of the sun. I saw them twice, and attempted a third observation, but a small cloud obscured the locality. The stars were both of the fifth magnitude, and but one is on the chart of the heavens. This star I recognised as Theta in Cancer. The two stars were about eight minutes apart. There is no such configuration of stars in the constellation of Cancer. I have no doubt that the unknown star is an intra-Mercurial planet, and am also inclined to believe that there may be more than one such planet. In 1859 the French astronomer Lescarbault claimed that he had seen an intra-Mercurial planet crossing the sun's disc. He related his discovery to Leverrier, who became a firm believer in the existence of such a planet. The perturbations of Mercury's orbit demand such a planet as Leverrier named Vulcan. The star I saw may have been the same that was seen by Prof. Watson, who was located at Rawlings, Wy. T.

"I possessed a comet eye-piece of very flat and large field and distinct to the very edge. It was made in this city, and to it and my blunder in failing to untie my instrument I owe my success. Prof. Colbert, of our party, also searched for Vulcan, but his field was not large. I saw but two protuberances, and those just at the end of totality. The advancing moon uncovered them. I had a view for at least two seconds of the sun's chromosphere at the same time. The chromosphere, by my measurement, is 2,000 miles in thickness. It is a layer of red-hot hydrogen surrounding the sun. The protuberances are projected from it.

"The corona was unusually extensive. It had never been seen so far extended. The greatest prolongation was in the direction of the moon's path across the sun, and as drawn by some of the parties extended on each side of the sun to a distance of more than three million miles. The pencils of light were radial mostly, though some of them were curved. I came away so quickly from Denver that I did not learn of the success of the other parties. In comparing notes with our party, Prof. Hough agreed with me in the measurement of the chromosphere. This measurement is made by calculating the time it takes the moon to pass over it. I learned of Prof. Watson's discovery the day after the eclipse. I have not seen him since he made the observation."

OUR ASTRONOMICAL COLUMN

WATSON'S SUSPECTED PLANET.—At the instance of M. Mouchez, the director of the Bureau des Calculs of the Observatory at Paris, M. Gaillot, who so long assisted Leverrier in the formation of his planetary tables, has examined how far the position of the object seen by Prof. Watson will accord with the more probable of the orbits which Leverrier inferred for a hypothetical planet, from the observations of suspicious spots in transit over the sun's disc. It may be remembered that their discussion led to a general formula, which was thus expressed by Leverrier; ν being the heliocentric longitude of the planet, k an indeterminate which might have values positive or negative, but necessarily whole numbers, and j the number of days reckoned from the beginning of the year 1750:—

$$\nu = 139^{\circ}\cdot94 + 214^{\circ}\cdot18\,k + (10^{\circ}\cdot901252 - 1^{\circ}\cdot972472\,k)\,j + (-5^{\circ}\cdot3 + 5^{\circ}\cdot5\,k)\cos.\,\nu.$$

M. Gaillot has found that, of the four possible orbits retained by Leverrier, corresponding to $k = -2, -1, 0,$ and $+1$, respectively, the first agrees the closest with the observation. With this value of k the diurnal motion is 14°·8462, the semi-axis major 0·164, and the period of revolution 24·25 days—less than the period of the sun's rotation. When the question of eccentricity is introduced, it is remarked that in the preferable orbit it is already very considerable, and comparable with that of the orbit of Mercury, and it is easy to demonstrate, to use M. Gaillot's words, "qu'il peut y avoir identité absolue entre la position observée et la position prévue." In fact, he finds that the agreement will be perfect if the eccentricity is assumed 0·14, and the longitude of the perihelion 74°. With regard to the inclination of the orbit to the ecliptic, M. Gaillot, from further consideration, supposes it may not exceed 7°. He notes that the most serious objection which opposes itself to the identification of the object observed, with a planet moving in the orbit indicated by Leverrier's formula, is that we should see but a very small part of the disc illuminated, and without denying that there is reason in this objection, M. Gaillot adds that Prof. Watson describes "as being of the fourth magnitude, a star the diameter of which may be comparable with that of Mercury, and which, in superior conjunction, may appear of the first magnitude." He further remarks that while it is not possible to decide with certainty upon the identity of

Prof. Watson's planet with that of which Leverrier has indicated the track, he believes he has shown that there is no incompatibility between the observed and hypothetical objects. If only one such planet exist between Mercury and the sun M. Gaillot points out that, in order to account for the accelerated motion in the perihelion of Mercury, its mass must be nearly equal to that of the latter—an inference drawn from Leverrier's table in vol. v. of the Paris *Annales*. An ephemeris extending to September 1 is appended to M. Gaillot's communication in the *Comptes Rendus* of August 5. Remarking that the assumed sidereal period of Prof. Watson's planet is 24·25 days, the synodical period is nearly twenty-six days, and accordingly we find by the ephemeris that the body should pass nearly at the same distance in longitude and latitude from the sun on August 24. But considering that this must hold during the next revolution whatever the period of any possible intra-Mercurial planet may be, it may be suggested that the most effectual plan of search will be to watch daily the vicinity so indicated with our larger instruments beyond the period at which the hypothetical planet should pass according to M. Gaillot's ephemeris. To set the equatorial it will be sufficient to subtract 9m. 50s. from the sun's right ascension at the proposed time of search, and to add to the sun's N.P.D. a quantity varying from 23′ on August 22, to 17′ on September 10.

A COMPANION OF α LYRÆ.—On several occasions during the last ten years, to our knowledge, attention has been directed to a star near α Lyræ in the *n.f.* quadrant, and suspicion of variability entertained, from the observer not having distinctly remarked it previously. An inquiry on the same point was lately addressed by a correspondent to Prof. Winnecke. The star is on an angle of about 42°, distance 139″. In October, 1870, it was a full magnitude fainter than the well-known Herschelian companion. Possibly some reader interested in the variable stars may be able to say if there is any reason to include the more distant star in this class of objects. In due course the direction of the proper motion of the large star will bring it immediately upon this *comes*, supposing there be no physical connection.

SCHMIDT'S "CHARTE DER GEBIRGE DES MONDES."—We hope next week to give some account of this most laborious and valuable work, which has been produced, through the liberality and scientific spirit of the Prussian government, in a style and with a perfection of arrangement that reflect the highest credit on all concerned. Probably no astronomical work could possess a greater degree of interest for amateurs generally, and—considering the attention paid to the examination of the moon's surface in this country—to British amateurs especially.

GEOGRAPHICAL NOTES

THE Arctic exploring ship *Alert* is being again fitted out for active duty, under the command of her old captain, Sir George Nares. She is intended for a voyage of surveying service principally in the South Pacific. Her first work will be an examination of the inner water leading from the Straits of Magellan to the Gulf of Peñas, along the seaboard of Chili; from this she will stretch across the South Pacific Ocean towards Fiji adding (*en route*) as far as practicable to our knowledge of the hydrography of the Low Archipelago, Society and Friendly Islands. After a few months spent in the neighbourhood of Fiji and in an examination of dangers lying in the track of navigation between that group and the Colony of New Zealand, she will, for the latter part of her voyage, be employed off the North Western Coast of Australia, principally in ascertaining the positions of, and as far as necessary charting, the various reefs and islets lying off the Australian continent, and between it and the ports of the Dutch Indies, at many of which reefs, &c., traffic has been for some time increasing in the search for trepang, pearls, and guano.

THE *Mittheilungen* of the Vienna Geographical Society, Nos. 6 and 7, contains a valuable "Culture-Map" of Asia Minor, exhibiting in a satisfactory manner the various zones of vegetation which mark that region recently brought into such intimate relations with this country. The map is by A. v. Schweiger-Lerchenfeld, who contributes also the explanatory text. Dr. Ziegler describes the important works carried on during 1877-8 by the Swiss correspondents of the Society, and Prof. Schmick contributes a paper on Ocean Currents.

FROM America we have No. 2, 1878, of the always interesting *Bulletin* of the American Geographical Society. A paper on "Japan, Geographical and Social," by the Rev. W. E. Griffis, contains the results of much research, as well as of personal observation, and is an important contribution to our knowledge of that country. Dr. Wright Hawkes discusses in an able and unprejudiced manner "The So-called Celtic Monuments of Britanny," his conclusion being that the evidence as to their origin is very conflicting. Mr. Jess Young, who was astronomer to Giles's trans-Australian expedition, gives an account of the results of his observations while crossing the great Australian desert.

THE Geographical Society of St. Petersburg intends to publish Karl Ritter's works in Russian in celebration of his jubilee.

WE learn that a new branch of the Russian Geographical Society, independent of those of Orenburg and Western Siberia, will shortly be opened at Tashkent.

WE have received from Williams and Norgate a neat and well-executed map of Cyprus, by Kiepert of Berlin, upon a sufficiently large scale to show distinctly the chief features of the island.

NOTES

PROF. MENDÉLEEFF is to be absent from his post in the St. Petersburg University for a year for the purpose of visiting Western Europe, where he will devote his time to the preparation of a large work on aëronautics. The work will contain a historical sketch of the subject, and expound its present condition from a scientific point of view.

THE appearance is announced of a biography of the late Prof. von Baer, by Dr. Stida, Professor in the Dorpat University. The autobiography of Baer appeared some years before his death, but embraced only his childhood and youth. The work of Dr. Stida is chiefly devoted to the scientific life of Baer, and contains a complete review of his works.

WE are glad to see that the *Times* is beginning to recognise the national importance of science-teaching in schools, and the necessity for our legislators being able to estimate the bearings of the various problems in physical science which are involved in the measures that come before them, in which the national welfare is involved. In a leading article on the meeting of the British Association the *Times* says that "We are living in a time when legislation is busy with physical matters, and is likely to become more so. The tendency of unscientific persons, especially when they are politicians, is to ignore the certainties which physical science furnishes, and hence to suppose that legislation about physical matters may properly be conducted upon a basis of compromise, like legislation about matters of opinion. It is very important that people who are not scientific themselves, and who never will be, should yet possess enough scientific knowledge to understand the difference which separates questions on which compromise is proper or expedient from those in which it would be fatal to the attainment of the desired result." The *Times* seems to us in-

clined to rate too highly the educational influences of the British Association on our legislators and on the general public. The rational conclusion to be drawn from the admissions in the passage we have quoted is that there ought to be some kind of scientific council which Government could consult in regard to measures involving questions of science. The British Association has done good service in helping to draw the attention of the public to the real nature of science, but it can never be a substitute for such a council as that which the most eminent of our men of science maintain would be the only effectual guarantee for enlightened legislation in scientific matters, and the establishment of which was recommended by the Duke of Devonshire's Commission. It is gratifying to find that the *Times* admits that "physical science affords an admirable means of mental training in schools," and we trust the day is not distant when it will be placed on a footing of equality with other branches of education. From another leader on Prof. Huxley's address we may infer that this chief representative of average British opinion has advanced from its obstinate opposition to the doctrines associated with the name of Darwin; indeed, the tone of the article to which we refer seems even more "advanced" than the address which was the occasion of it. It is comforting to find that, in scientific matters, at least, a better spirit is beginning to pervade the paper which is both an index and a leader of middle-class public opinion.

THE time of meeting of the German Naturalists and Physicians at Cassel has been changed from September 18–24 to September 11–18. This change has been caused by the fact that during the time originally fixed a series of military manœuvres are to take place which will cause a great influx of military men into Cassel, the lodging resources of which, it is feared, would not be commensurate with the requirements of a simultaneous meeting of the men of science and of war.

THE meeting of the French Association will not take place at the Palais des Beaux Arts, as it was originally contemplated, but in the rooms of the Lycée Saint Louis. The session begins to-day.

A SOMEWHAT curious discussion has been recently occupying the attention of the Paris Academy of Sciences. It originated in the publication by M. Berthelot, in the *Revue Scientifique* of July 20, of a series of laboratory notes by the late Claude Bernard, on alcoholic fermentation, in which the great physiologist came to conclusions opposed to those so long advocated by M. Pasteur. It was natural that M. Pasteur should endeavour to weaken the force of M. Bernard's experiments and conclusions. In the *séance* of July 22 he drew attention to the publication of the notes, and explained their apparent opposition to his theory by the statement that Bernard's method of working was to proceed to the investigation of every theory as if its opposite were true, thus submitting every point to a crucial test. He declared his intention of taking up Bernard's series of experiments and working them out from the latter's standpoint, in the confidence that the result would be entirely in favour of his (Pasteur's) theory of fermentation. At the next sitting of the Academy M. Pasteur stated that he had found that many changes had been made for typographical and other reasons in the notes as they appeared in the *Revue* as compared with the original MSS., though these changes do not seem materially to alter the sense of the "Notes." M. Berthelot rejoices that M. Pasteur intends to repeat Bernard's experiments, and whatever may be the result, science is likely to be a gainer. There at present the matter rests.

WE notice the death in Jena, on July 25, of the well-known botanist Prof. Christian Eduard Langethal. He was born at Erfurt in 1806. After completing his botanical studies at Jena he was for some time assistant to the famous agriculturist Prof. Schulze, and in 1835 teacher of natural sciences at Eldena. In 1839 he was called to a professorship at Jena, which he occupied up to the date of his death. Prof. Langethal was the author of several standard works, among others, "Geschichte der deutschen Landwirthschaft" (1846-56); "Lehrbuch der landwirthshaftlichen Pflanzenkunde" (1841-45; fifth edition, 4 vols. 1876); "Beschreibung der Gewächse Deutschlands" (1858); "Terminologie der beschreibenden Botanik;" and "Flora von Thüringen," issued in conjunction with Schenk and Schlechtendal.

THE model of the Gauss monument has just been finished by Prof. Schaper. The design is made after an original portrait of Gauss, in the possession of Göttingen University, and the monument itself is now to be executed in bronze at the atelier of Herr Gladenbeck, of Berlin. It will be erected at Brunswick, the birthplace of the great mathematician.

THE Royal Academy of Sciences at Munich has just elected to membership Prof. Krehl, of Leipzig, and Mr. Charles Darwin.

MEASURES are being taken for the foundation of a geological institute at St. Petersburg, which shall accomplish for the Russian empire what the Imperial Institute at Vienna has done for Austria. At present geological work is attempted only at the instigation of mining companies and the learned societies, and the want of unity in the efforts made for the development of Russian geology has long been painfully felt.

THE French Minister of Public Instruction has authorised the director of the newly created Central Meteorological Bureau to hire a hotel in the Rue de Varenne, in a populous part of Paris, where no observations can be taken, in which to establish the offices of his administration, which, within a few days, will be removed from the observatory. It is fair to state that Admiral Mouchez will not discontinue the taking of meteorological observations with the instruments established by Arago. It is pretty certain that the magnetical instruments established at a great expense by Leverrier will not be removed at a period so important in the history of terrestrial magnetism. We can state that new observers will be paid for that purpose, if necessary.

THE Meteorological Commission of Vaucluse again this year ascended Mont Ventoux. M. Mascart, the new director of the Meteorological Central Bureau, was one of the party, having come from Paris for that purpose. Simultaneous observations were taken at Orange, Carpentras, Avignon, and Apt. But the principal object of this scientific excursion was to determine the best manner of erecting the contemplated observatory at the top of this mountain, which is 1,919 metres above the level of the sea, and 1,692 above Apt, the nearest meteorological station.

THE Giffard captive balloon has become a great favourite amongst the visitors to Paris, as well as the Parisians themselves. The greatest number of ascents in one day has been seventeen, and the money taken has been 6,000*l.* in eighteen days, including three in which ascents could not take place, owing to the boisterous state of the weather. The charges are one franc for witnessing the ascents, and twenty francs for ascending.

THE Lords of the Committee of Council on Education have determined to award bronze medals to students who obtain a first-class in honours in any subject of science at the May examinations.

IN an Appendix to the Annual Report (for 1878) of the Lyceum of Valparaiso, Mr. Edwyn C. Reed gives an account of the progress he has made in commencing a Museum of Natural History for this Institution, which, if we understand rightly, is not intended to be simply for the instruction of the scholars of

the Lyceum, but for the City of Valparaiso generally. The new Museum does not appear to be well off for funds, but Mr. Reed has many friends and correspondents in this country who will be able to serve him by exchanges.

THE second marine excursion of the Birmingham Natural History and Microscopical Society to the Island of Arran, in the month of July last, proved most successful. Twenty-eight members, including six ladies, formed the party, who travelled by Pullman cars and family carriage by Midland railway, going by night and returning by day. A small steam yacht—the *Lizzie*—was chartered for a week. There were also botanical and geological excursions daily to the many interesting parts of the island. The results of the dredgings were most satisfactory, and a beautiful series of specimens was taken, including *Luidia fragillisima* and two or three Nudibranchs new to the locality. The towing-net—on an improved principle, devised by Mr. Henry Allport—was also used most successfully, and many interesting forms of marine life taken, notably *Bipinnaria* and *Pluteus*. The examination of these and other microscopic objects in the evenings in the ladies' drawing-room proved a great attraction. Preliminary reports have been made to the society and the specimens exhibited:—General, by Mr. Edmund Tonks, B.C.L., president, and Mr. Sam. Timmins, F.R.S.L.; Botanical, by Mr. John Morley, hon. sec.; Dredging Arrangements, Mr. John F. Goode; and Marine Zoology, by Mr. W. R. Hughes, F.L.S. The Geological Report, by the Rev. George Deane, D.Sc., F.G.S., was deferred. A full account of the proceedings will appear in an early number of the *Midland Naturalist*. The excursion extended from July 19 to 27, and a most interesting and enjoyable week was passed. A resolution was unanimously passed suggesting to the society that the next excursion should be to Falmouth.

WE have on several occasions drawn attention to the interesting department in the Paris Exhibition devoted to school equipments, and it is hopeful to find that in connection therewith, a *Times* Paris correspondent has discovered the success of the French in their efforts to impart to the nation a technical education. "One of the most interesting and instructive departments of the Exposition," the *Times* correspondent writes, "is that devoted to the illustration of the working and results of the system of French popular education both in Paris and in the Provinces. The foreign visitor who observes with admiration throughout the country the evidences of the general artistic and technical skill of the French workmen of every class will see in this Educational Department of the Exposition the key to the secret of that success. When will the corporation of London be able to match the interesting educational results here displayed by the sister municipality of Paris?"

UNDER the presidentship of Lord Hardwicke the opening meeting of the thirty-fifth annual congress of the British Archæological Association began on Monday at the ancient town and port of Wisbech.

ZOOLOGISTS will be glad to know that the "Rules for Zoological Nomenclature," drawn up by the late H. E. Strickland, F.R.S., at the instance of the British Association, have been reprinted. The "Notes" were prepared after consultation with many zoologists, British and foreign, and are now brought out under the care of Mr. P. L. Sclater. The publisher is Mr. Murray.

PROF. BROCA opened the International Congress of Anthropology at the Paris Exhibition by a short address, in which he pointed out the necessity of rigid observation as the only means of obtaining trustworthy data on which to build the science.

WE are glad to learn that the French balloon service has not een disorganised by the resignation of Col. Laussedat. The new head of the service is General Farr, who distinguished himself in the last Franco-German war in the northern part of France.

DR. ERNST, of Carácas, writes us that by an oversight in his note on the earthquake of Cúa (NATURE, vol. xviii. p. 130) the direction of the shock was not given. It came from E.N.E., or more exactly E. 15° N.

THE article on the Elasmotherium from which our description was obtained (vol. xviii. p. 387) appeared in No. 23 of the Russian journal *Niwa* for 1878, p. 411.

THE additions to the Zoological Society's Gardens during the past week include a Lion (*Felis leo*), a Patas Monkey (*Cercopithecus ruber*) from West Africa, presented by Mr. W. H. Wylde, s. s. *Agra*; a common Paradoxure (*Paradoxurus typus*) from India, presented by Mr. Edwin Etty Sass; two Common Buzzards (*Buteo vulgaris*), European, presented by Master Valentine Marks; two Herring Gulls (*Larus argentatus*), European, presented by Mr. Thomas Landseer; a Crested Ground Parrakeet (*Calopsitta novæ-hollandiæ*) from Australia, presented by Mrs. Parker; a many-zoned Hawk (*Melierax polyzonus*) from East Africa, presented by Mr. C. H. Fisher; a Copper-head Snake (*Cenchris centortrix*) from Pittsburg, U. S. A., presented by Dr. F. Painter, F.Z.S.; a Macaque Monkey (*Macacus cynomolgus*) from India, two Barbary Apes (*Macacus inuus*) from North Africa, two Beautiful Parrakeets (*Psephotus pulcherrimus*) from Australia, thirteen Greek Land Tortoises (*Testudo græca*) European, deposited; a Bladder-nosed Seal (*Cystophora cristata*) from the North Atlantic, six Common Kingfishers (*Alcedo ispida*) British Isles, purchased.

THE BRITISH ASSOCIATION

DUBLIN, *Tuesday*.

SINCE we last wrote the British Association week has come and gone with even more than its wonted rush and whirl of incessant engagements. That the meeting has been a successful one in every respect is unquestionable. Though no very original papers have been read or any great sensation excited in any section, as at Plymouth, by the telephone, yet the meetings have been above the average in general interest, the evening lectures and entertainments having been specially attractive, and the attendance extremely good. This meeting will always be memorable for the splendid address of the president, Dr. Spottiswoode.

The proceedings have been very fairly and fully reported in the local papers. Is it too much to hope that, ere long, the Association may see its way to utilising the energy now invariably displayed by the provincial press in giving to its members an early, full, and revised report of the proceedings of the meeting in a more convenient form than in the columns of a newspaper? We are aware that this question has often been mooted, and indeed it has not been untried, but for its permanent success a well-considered scheme is necessary, and doubtless the local press, as well as the sectional secretaries (more especially if some honorarium were attached to one secretary in each section) would gladly lend their aid. If we may judge by the constant inquiries for such a report that reach our ears at each meeting of the Association, it would seem to be a widely-felt want. Our own columns have, to a large extent, been opened to meet this need; but it is obvious that the constant pressure upon our space only permits a partial report of the proceedings, whilst the discussions upon the papers, often extremely valuable, have no permanent record, even in a condensed form.

There is another point upon which we may venture to say a word or two, and that is, the desirability of giving a little more attention to the fact that a large majority of

those present at the meetings of the Association have had no special scientific training, and to whom, therefore, almost the whole of the sectional proceedings are one continuous riddle. Would the actual usefulness of the Association to scientific men be in any way lessened by devoting a certain portion of the proceedings of each section at a specified time to papers specially prepared for an audience, not of specialists, but simply of intelligent men and women, and to reports on the progress of the various branches of science, couched in untechnical language? Would not such a course be a distinct gain in the diffusion of science, and in its permanent effect upon the town where the annual meetings are held; and would it not enable the general public to give a more hearty and sympathetic support to the scientific worker?

Let us now put on record a brief epitome of the principal events in the meeting which has just closed. On Wednesday night, August 14, Dr. Spottiswoode delivered his presidential address to a crowded audience, but, unfortunately, in a room only suited to the strongest lungs. The concert-hall of the Exhibition Palace was the only room available for the purpose, and the Association were fortunate in having chosen lecturers with such powerful voices as Mr. Romanes and Prof. Dewar, who subsequently delivered their discourses in the same place.

The first lecture, by Mr. Romanes, on Animal Intelligence, was admirably delivered and excited general interest by its lucid and masterly exposition of the psychological affinities between the mind of man and that of the lower animals.

The lecture by Prof. Dewar, on "Dissociation, or Modern Ideas of Chemical Action," delivered on the 19th, will be memorable for its magnificent display of experimental illustrations; the prodigious labour involved in the preparation for the successful execution and for rendering visible to an immense audience (as was the case) a multitude of novel and delicate experiments, generally seen only on a small scale, but here magnified to astonishing proportions, can only be known to those who have gone through a like, though lesser, toil.

Numerous *conversazioni* and private entertainments must leave upon the minds of the members a not unjust impression of the gaiety and hospitality of the citizens of Dublin in their corporate as well as in their private capacity. First there came an unusually brilliant *soirée* given by the Royal Dublin Society, many of the objects of interest in which were reproductions lent by the South Kensington Museum from the valuable loan collection of 1876. To the untiring exertions of Mr. J. H. Wigham, of the firm of Messrs. Edmundson, both on this occasion as well as on others, the hearty thanks of the Association are due. All the powerful machinery in actual use in first-class lighthouses for the purposes of illumination, as well as for signalling in fogs, was erected for the use of the Association in the spacious premises of the Royal Dublin Society, at the sole expense of the firm Mr. Wigham has made so well known.

On Friday evening the Rector and Vice-Rector of the Catholic University gave another *conversazione*. The brilliancy of the electric lights, derived from a Gramme machine, exhibited here on this and on subsequent occasions, delighted everyone. The phonograph was exhibited in the physical lecture-hall, songs were sung and poetry recited by this instrument with the most perfect success. In one instance a song which had been sung into the phonograph by a well-known Dublin vocalist was turned on, after the lapse of an hour, in the presence of a new audience, who had not heard the original, and not only were the words and the melody easily recognised, but those who were familiar with the style of the singer were able at once to tell whose voice it was that came forth thus wonderfully from the rotating cylinder. In another hall, specially darkened for the purpose, Mr. Spottiswoode's vacuum tubes were exhibited, to great advantage by means of a splendid induction coil made by Mr. H. Yeates, of London. It was shown, too, how the stratification of the electric light, in these tubes, may be unravelled by the aid of a rotating mirror, the character of the stratification changing most remarkably with every change in the strength of the current. These experiments were greatly facilitated by the use of a new form of Bunsen's bichromate of potash battery, lately devised by Dr. Molloy, Professor of Physics in the Catholic University, for general use in the laboratory. This battery, which consists of six large cells, is always ready for immediate use, and is equal in power to at least eight quart cells of Grove's battery.

The great hall of the University was laid out with refreshments on one side, while on the other a varied and most successful series of interesting and brilliant experiments, illustrating the phenomena of heat, light, sound, and electricity, offered an intellectual feast even more attractive to the great body of the visitors. Upwards of 700 guests were received during the evening, but, owing to the admirable arrangements, all were able to make their way through the several halls without any inconvenient pressure. Amongst the guests we noticed most of the distinguished English and foreign *savans*, the President of the Association and Mrs. Spottiswoode, Sir John and Lady Lubbock, Sir Joseph Hooker, Prof. Roscoe, Prof. Gladstone, &c.

We understand that it is to Dr. Molloy, the genial and accomplished Professor of Physics at, and Vice-Rector of the Catholic University, the Association is indebted for one of the pleasantest evenings during this meeting.

On Monday the President and Fellows of the Royal College of Surgeons gave a reception to the chief members of the Association, and on Tuesday evening an interesting *soirée*, open to the Association generally, was given by the Royal Irish Academy. The Lord Lieutenant and the Duchess of Marlborough were present both on Thursday evening at the Royal Dublin Society and on Tuesday at the Royal Irish Academy.

Saturday, as well as Thursday, was devoted to excursions, particulars of which we gave in a former impression.

At the general committee on Monday, Mr. Sorby, and other delegates, attended to invite the Association to meet at Sheffield, an invitation which was cordially and unanimously accepted. Dr. Allman was chosen president, and Swansea was fixed upon as the place of meeting for 1880. The officers of the Association were re-appointed, and the election of Mr. J. E. H. Gordon as assistant general secretary was confirmed. The retirement of Mr. Griffith from this post which he has so long and admirably filled, caused universal regret. We hope it may not be considered impertinent if, in conclusion, we also express our sense of the great loss which the Association has sustained in the resignation of Mr. Griffith, who, in spite of the many demands upon his time and patience, has combined a systematic and energetic discharge of his duties with the most calm and courteous demeanour during the fifteen years that he has filled the arduous post.

To-day the University of Dublin conferred the honorary degree of LL.D. on the following members of the Association:—Mr. W. Spottiswoode, Prof. H. J. S. Smith, Dr. Janssen, Prof. Maxwell Simpson, Prof. E. Roscoe, Prof. Alexander W. Williamson, Mr. Evans, Sir John Lubbock, Sir J. D. Hooker, Prof. W. H. Flower, Prof. Huxley, Sir Wyville Thomson, and Prof. J. Thomson.

(*By Telegram.*)

DUBLIN, *Wednesday*.

The attendance at the concluding general meeting has been considerably above the average, the total number present being 2,577, of which 1,959 had either Associa-

tion or ladies' tickets, whilst 110 new members joined. The total sum received by the treasurer for tickets was 2,605*l.* Last year the attendance was 1,229, and the sum taken 1,268*l.*

The following grants were made :—

	£
Mathematics and Physics.	
Cayley, Prof.—Calculation of Factor Tables for the Fifth and Sixth Millions (re-appointed)	150
Sylvester, Prof.—Tables of Fundamental Invariants of Algebraic Forms	50
Forbes, Prof. G.—Observation of Atmospheric Electricity at Madeira (renewed)	15
Haughton, Rev. Dr.—Tables of Sun-Heat Coefficients	30
Joule, Dr.—Determination of Mechanical Equivalent of Heat (renewed)	65
Forbes, Prof.—Instrument for Detecting Presence of Fire-damp in Mines	30
Ayrton, Mr.—Specific Inductive Capacity of a good Sprengel Vacuum	40
Glaisher, Mr.—Luminous Meteors	20
Gill, Mr. D.—Improvements in Astronomical Clocks	30
Chemistry.	
Roberts, Mr.—Composition and Structure of some of the less known Alkaloids (re-appointed)	25
Wallace, Dr.—Development of Light from Coal-gas of different qualities (re-appointed)	10
Adams, Prof. W. G.—Electrolysis of Metallic Solutions and Solutions of Compound Salts	25
Geology.	
Evans, Mr. John.—Exploration of Caves in Borneo	50
Hull, Prof.—Circulation of Underground Waters (re-appointed)	15
Godwin Austen, Mr.—Kentish Boring Exploration (renewed)	100
Evans, Mr. John.—Kent's Cavern Exploration (re-appointed)	100
Evans, Mr. John.—Record of Progress of Geology (re-appointed)	100
Haughton, Rev. Dr.—Fermanagh Caves Exploration (re-appointed)	5
Close, Rev. Maxwell.—Miocene Flora of Basalt of North of Ireland	20
Biology.	
Bate, Mr. Spence C.—Marine Zoology of South Devon	20
Stainton, Mr.—Record of Zoological Literature (re-appointed)	100
Foster, Dr. M.—Table at the Zoological Station, Naples (re-appointed)	75
Brooke, Sir Victor.—Illustrations for a Monograph on the Mammoth	17
Sclater, Mr. P. L.—Natural History of Socotra	100
Rolleston, Prof.—Exploration of Bone Caves in South Wales (partly renewed)	50
Fox, General Lane.—Exploration of Ancient Earthworks (re-appointed)	25
Fox, General Lane.—Excavation at Port Stewart, and elsewhere in the North of Ireland	15
Statistics and Economic Science.	
Farr, Dr.—Anthropometric Committee (re-appointed)	50
Mechanics.	
Thomson, Sir W.—Datum Level of the Ordnance Survey (re-appointed)	10
Froude, Mr. W.—Instruments for Measuring the Speed of Ships (renewed)	50
Napier, Mr. J. R.—Steering of Screw Steamers (re-appointed)	10
Thomson, Sir W.—Tidal Observations in the English Channel (re-appointed)	10

REPORTS.

Report of the Committee, consisting of Prof. Cayley, Dr. Farr, Mr. J. W. L. Glaisher, Dr. Pole, Prof. Fuller, Prof. A. B. W. Kennedy, Prof. Clifford, and Mr. C. W. Merrifield, appointed to consider the Advisability and to estimate the Expense of constructing Mr. Babbage's Analytical Machine, and of printing Tables by its means. Drawn up by Mr. Merrifield.—We desire in the first place to record our obligations to General Henry Babbage for the frank and liberal manner in which he has assisted the Committee, not only by placing at their disposal all the information within his reach, but by exhibiting and explaining to them, at no small loss of time and sacrifice of personal convenience, the machinery and papers left by his father, the late Mr. Babbage. Without the valuable aid thus kindly rendered to them by General Babbage it would have been simply impossible for the Committee to have come to any definite conclusions, or to present any useful report.

We refer to the chapter in Mr. Babbage's "Passages from the Life of a Philosopher," and to General Menabrea's paper, translated and annotated by Lady Lovelace, in the third volume of "Taylor's Scientific Memoirs," for a general description of the analytical engine.

The Report then, in Section I., contains an account of the general principles of calculating engines, and proceeds :—

II. *Special Characteristics of Mr. Babbage's Analytical Engine.*—1. *The Mill.*—The fundamental operation of Mr. Babbage's analytical engine is simple addition. This and the other elementary rules of subtraction, multiplication, and division, and all combinations of these, are performed in what is called "*the mill.*" All the shifts which have to take place, such as changing addition into subtraction by throwing a reversing train into gear, or the shift of the decimal place, carrying and borrowing, and so forth, are effected by a system of rotating cams acting upon or actuated by bell-cranks, tangs, and other similar devices commonly used in shifting machinery, sometimes under the name of clutches or escapements. These clutches and bell-cranks control the purely additive and carrying processes effected in the additive trains described in the note to Section I., and, *being themselves suitably directed*, secure that the proper processes shall be performed upon the proper subject-matter of operation, and duly recorded, or used, as may be required.

2. *The Store.*—A series of columns, each containing a series of wheels, constitutes the store. This store, which may be in three or more dimensions, both receives the results of operations performed in the mill, and serves as a store for the numbers which are to be used in the mill, whether as original or as fresh subjects of operation in it. Each column in the store corresponds to a definite number, to which it is set either automatically or by hand, and the number of digits in this number is limited by the number of wheels carried on the shaft of the column. The wheels gear into a series of racks, which can be thrown into or out of gear by means of the cards.

3. *Variable Cards.*—All the numbers which are the subject of operation in the mill, whether they are the result of previous operations therein, or new numbers to be operated upon for the first time, are introduced to it in the form of Jacquard[1] cards, such as are used in weaving. One set of wires or axes transfers the numbers on these cards to the subject of operation in the mill, exactly as similar cards direct which of the warp threads are to be pushed up, and which down, in the Jacquard loom. The mill itself punches such cards when required.

4. *Operation Cards.*—A different set of cards selects and prescribes the sequence of operations. These act, not upon the number wheels of the mill or store, but upon the cams and clutches which direct the gearing of these wheels and trains. Thus, in such an operation as $(a\,b + c)\,d$, we should require :—

1st, four variable cards with the numbers a, b, c, d.

2nd, an operation card directing the machine to multiply a and b together.

3rd, a record of the result, namely, the product $a\,b = p$, as a fifth variable card.

4th, an operation card directing the addition of p and c.

5th, a record of the result, namely, the sum $p + c = q$, as a sixth variable card.

[1] In a letter written by Mr. Babbage to Arago in December, 1839, the following explanation of the use of these cards is given. It probably conveys the idea in the fewest words possible. It is only necessary to add that their twofold employment embodies the separation of the symbols of operation from those of quantity. "You are aware that the system of cards which Jacquard invented are the means by which we can communicate to a very ordinary loom orders to weave any pattern that may be desired. Availing myself of the same beautiful invention, I have by similar means communicated to my calculating engine orders to calculate any formula, however complicated; but I have also advanced one stage further, and I have communicated through the same means orders to follow certain laws in the use of those cards, and thus the calculating engine can solve any equations, eliminate between any number of variables, and perform the highest operations of analysis."

6th, an operation card directing the machine to multiply q and d together.

7th, a record of the result, namely, the product $q\,d = p_2$, either printed as a final result or punched in a seventh variable card.

III. *Capability of the Engine.*—It has already been remarked that the direct work of the engine is a combination and repetition of the processes of addition and subtraction. But in leading up to any given datum by these combinations, there is no difficulty in ascertaining tentatively when this datum is reached or about to be reached. This is strictly a tentative process, and it appears probable that each such *tentamen* requires to be specially provided for, so as to be duly noted in the subsequent operations of the machine. There is, however, no necessary restriction to any particular process, such as division; but any direct combination of arithmetic, such as the formation of a polynomial, can be made to lead up to a given value in such a manner as to yield the solution of the corresponding equation. In any such process, however, it is evident that there can be only (to choose a simile from mechanism) one degree of freedom; otherwise the problem would yield a locus, indeterminate alike in common arithmetic, and as regards the capabilities of the machine. The possibility of several roots would be a difficulty of exactly the same character as that which presents itself in Horner's solution of equations, and the same may be said of imaginary roots differing but little from equality. These, however, are extreme cases, with which it is usually possible to deal specially as they arise, and they need not be considered as detracting materially from the value of the engine. Theoretically, the grasp of the engine appears to include the whole synthesis of arithmetic, together with one degree of freedom tentatively. Its capability thus extends to any system of operations or equations which leads to a single numerical result.

It appears to have been primarily designed with the following general object in view: to be coextensive with numerical synthesis and solution, without any special adaptation to a particular class of work, such as we see in the difference engine. It includes that, *à majori*, and it can either calculate any single result or tabulate any consecutive series of results just as well. But the absence of any specialty of adaptation is one of the leading features of the design.

Mr. Babbage has also considered the indication of the passage through infinity as well as through zero, and also the approach to imaginary roots. For details upon these points we must refer to his "Passages from the Life of a Philosopher."

IV. *Present State of the Design.*—The only part of the analytical engine which has yet been put together is a small portion of "the mill," sufficient to show the methods of addition and subtraction, and of what Mr. Babbage calls his "anticipating carriage." It is understood that Gen. Babbage will (independently of this report) publish a full account of this method. No further mention of it will therefore be made here.

V. *Probable Cost.*—Without attempting any exact estimate, we may say that it would surprise us very much if it were found possible to obtain tenders for less than 10,000*l.*, while it would pretty certainly cost a considerable sum to put the design in a fit state for obtaining tenders. On the other hand it would not surprise us if the cost were to reach three or four times the amount above suggested.

Section VI. refers to *Strength and Durability*, and VII. to *Probable Utilisation of the Analytical Engine.*

VIII. *Possible Modification of the Engine.*—Without prejudging the general question referred to us as to the advisability of completing Mr. Babbage's engine in the exact shape in which it exists in the machinery and designs left by its inventor, it is open to consideration whether some modification of it, to the sacrifice of some portion of its generality, would not reduce the cost and simplify the machinery so as to bring it within the range of both commercial and mechanical certainty. The "mill," for example, is an exceedingly good mechanical arrangement for the operations of addition and subtraction, and with a slight modification, with or without store-columns, for multiplication. We have already called attention to the imperfection of the existing machines, which show weakness and occasional uncertainty. It is at least worth consideration whether a portion of the analytical engine might not thus be advantageously specialised so as to furnish a better multiplying machine than we at present possess. This, we have reason to believe, is a great desideratum both in public and private offices, as well as in aid of mathematical calculators.

Another important desideratum to which the machine might be adapted without the introduction of any tentative processes (out of which the complications of the machinery chiefly arise) is the solution of simultaneous equations containing many variables. This would include a large part of the calculations involved in the practical application of the method of least squares. The solution of such equations can always be expressed as the quotient of two determinants, and the obtaining this quotient is a final operation, which may be left to the operator to perform by ordinary arithmetic, or which may be the subject of a separate piece of machinery, so that the more direct work of forming the determinant, which is a mere combination of the three direct operations of addition, subtraction, and multiplication, may be entirely freed from the tentative process of division, which may thus be prevented from complicating the direct machinery. In the absence of a special engine for the purpose, the solution of large sets of simultaneous equations is a most laborious task, and a very expensive process indeed, when it has to be paid for, in the cases in which the result is imperatively needed. An engine that would do this work at moderate cost would place a new and most valuable computing power at the disposal of analysts and physicists.

Other special modifications of the engine might also find a fair field for reproductive employment. We do not think it necessary to go into these questions at any great length, because they involve a departure, in the way of restriction and specialisation, from Mr. Babbage's idea, of which generality was the leading feature. Nevertheless, we think that we should be guilty of an omission if we were to fail to suggest them for consideration.

IX. *General Conclusions, and Recommendation.*—1. We are of opinion that the labours of Mr. Babbage, firstly on his Difference Engine, and secondly on his Analytical Engine, are a marvel of mechanical ingenuity and resource.

2. We entertain no doubt as to the utility of such an engine as was in his contemplation when he undertook the invention of his analytical engine, supposing it to be successfully constructed and maintained in efficiency.

3. We do not consider that the possibilities of its misuse are any serious drawback to its use or value.

4. Apart from the question of its saving labour in operations now possible, we think the existence of such an instrument would place within reach much which, if not actually impossible, has been too close to the limits of human skill and endurance to be practically available.

5. We have come to the conclusion that in the present state of the design of the engine it is not possible for us to form any reasonable estimate of its cost, or of its strength and durability.

6. We are also of opinion that, in the present state of the design, it is not more than a theoretical possibility; that is to say, we do not consider it a certainty that it could be constructed and put together so as to run smoothly and correctly, and to do the work expected of it.

7. We think that there remains much detail to be worked out, and possibly some further invention needed, before the design can be brought into a state in which it would be possible to judge whether it would really so work.

8. We think that a further cost would have to be incurred in order to bring the design to this stage, and that it is just possible that a mechanical failure might cause this expenditure to be lost.

9. While we are unable to frame any exact estimates, we have reason to think that the cost of the engine, after the drawings are completed, would be expressed in tens of thousands of pounds at least.

10. We think there is even less possibility of forming an opinion as to its strength and durability than as to its feasibility or cost.

11. Having regard to all these considerations, we have come, not without reluctance, to the conclusion that we cannot advise the British Association to take any steps, either by way of recommendation or otherwise, to procure the construction of Mr. Babbage's analytical engine and the printing tables by its means.

12. We think it, however, a question for further consideration whether some specialised modification of the engine might not be worth construction, to serve as a simple multiplying machine, and another modification of it arranged for the calculation of determinants, so as to serve for the solution of simultaneous equations. This, however, inasmuch as it involves a departure from the general idea of the inventor, we regard as lying outside the terms of reference, and therefore perhaps rather for the consideration of Mr. Babbage's representatives than ours.

We accordingly confine ourselves to the mere mention of it by way of suggestion.

Third Report of the Committee, consisting of Dr. Joule, Prof. Sir W. Thomson, Prof. Tait, Prof. Balfour Stewart, and Prof. Maxwell for the Determination of the Mechanical Equivalent of Heat.—Dr. Joule has published a paper giving *in extenso* the experiments summarised in the last two reports, in the *Philosophical Transactions* of the Royal Society, where was published his former paper in 1850. The new result which confirms the old one, gives 772·55 foot pounds as the equivalent at the sea-level, and the latitude of Greenwich of the heat which can raise a pound of water, weighed *in vacuo*, from 60° to 61° F. of the mercurial thermometer where the permanent freezing point is called 32°, and the permanent boiling point of water under a barometrical pressure of 30 inches of mercury raised to 60° F. is 212°. The work at present in hand is a more accurate investigation of the true position of the freezing and boiling points of the thermometer when cleared from the effects of the imperfect elasticity of the glass of which they are constructed. The correction to the above equivalent which may thus accrue, is not expected to be of considerable amount.

Report of the Committee appointed for the purpose of inquiring into the possibility of establishing a "Close Time" for Indigenous Animals.—The Committee being dissatisfied with certain points in the Report of the Scottish Fishery Commissioners, addressed a letter to the Home Secretary, calling attention to the following points:—

"I. That conclusions Nos. 2 and 3 of the Commissioners, viz., that 'Legislation in past periods has had no appreciable effect,' and that 'Nothing that man has yet done, and nothing that man is likely to do, has diminished, or is likely to diminish, the general stock of herrings in the sea,' if correct, are absolutely contradicted by conclusion No. 13, which recommends that 'The Sea-Birds Preservation Act, protecting gannets and other predaceous birds which cause a vast annual destruction of herrings, should be repealed in so far as it applies to Scotland.'

"II. That conclusion No. 1, stating that 'The herring-fishery on the coast of Scotland as a whole has increased and is increasing,' clearly shows that there can be no necessity for the step recommended in conclusion No. 13 as above cited.

"III. That conclusion No. 13 seems to have been arrived at from exaggerated or incorrect information, as will appear from the following considerations:—The number of gannets on Ailsa is estimated ('Report,' p. xi.) at 10,000, and a yearly consumption of 21,600,000 herrings is assigned to them; while the Commissioners assume that there are '50 gannets in the rest of Scotland for every one on Alisa,' and on that assumption declare that the total destruction of herrings by Scottish gannets is more than 1,110,000,000 per annum. This is evidently a miscalculation; for, on the premises, this last number should be 1,101,600,000, a difference of more than 8,000,000.

"But, more than this, supposing the figures at the outset are right, it appears to the Close-Time Committee that the succeeding assumption of the Commissioners must be altogether wrong; at any rate, there is no evidence adduced in its support, and some that is contradictory of it.

"The number of breeding-places of the gannet in the Scottish seas has long been known to be five only, as, indeed, is admitted by one of the Commissioners (Appendix No. 2, p. 171; and the evidence of Capt. M'Donald, which is quoted in a note to the same passage, while estimating the Ailsa gannets at 12,000 in 1869 (not 1859, as printed), puts the whole number of Scottish gannets at 324,000 instead of 510,000, which there would be at the rate of 50 in the rest of Scotland for one on Ailsa, according to the Commissioners' assumption.

"Moreover, 50,000 of these 324,000 birds, or nearly one-sixth, are admitted by this same Commissioner to be 'of great value to the inhabitants' of St. Kilda, and, indeed, they are of far greater value to them than any number of herrings, since it is perfectly well known that the people of St. Kilda could hardly live without their birds; therefore this 50,000 must be omitted from any estimate of detriment. Deducting, then, 50,000 from Capt. M'Donald's 324,000, we have 274,000, and these, at the Commissioner's estimate, would consume 600,060,000 herrings instead of the 1,110,000,000 alleged by the report, and, therefore, nearly 200,000,000 fewer than the Commissioners' estimate of the annual take of the Scottish fisheries (800,000,000)—25 per cent. less instead of 37 per cent. more.

"Hitherto the supposition of the report, that the gannets frequent the Scottish seas all the year round, has been followed; but the Close-Time Committee begs leave to observe that, as a matter of fact, these birds are not there in force for more than half the year.

"This, then, will require another abatement to be made. Not to exaggerate the case, the Committee assumes them to frequent these waters seven months, or seven-twelfths of a year. This will make their annual capture of herrings 350,350,000, instead of the more than 1,110,000,000 of the Commissioners, being nearly 700,000,000, or much less than one-third, fewer.

"IV. That in all the evidence received and published by the Commissioners only two witnesses allege that any harm has resulted to the fisheries from the Sea-Birds Protection Act. Of these, the first, Robert M'Connell, presented a petition from the fishermen of Girvan, in which it is stated (p. 145) that 'no legislation is called for or required;' while another witness from the same place, John Melville (a fishery officer), declares (at p. 146) that 'The fishery has very much increased this last year. Recent years have also shown a gradual increase. The increase is partly due to the increased machinery and partly to the increase in the number of herrings.'

"The second witness unfavourable to the Act, John M'William (an Inspector of Poor), speaks (pp. 147-49) only from personal knowledge acquired between 1833 and 1853, when he ceased to be a fisherman, and not from any recent experience. He can therefore scarcely be held competent to give an opinion of his own as to whether the Sea Birds Protection Act (passed in 1869) has injured the fisheries. Another witness recommends the repeal of this Act; but he, Hugh MacLachlan, expressly states (p. 143) that he 'thinks the cause of the decrease [in the numbers of herrings taken] is the catching immature fish;' and the remedy he proposes is the adoption of a strict Close Time.

"V. That, on the other hand, the utility of sea-birds in pointing out the situation of shoals of herrings and other fish is not only generally notorious, but is even admitted in the Report (pp. 57 and 175).

"VI. That if the Sea-Birds Act be repealed on the grounds alleged for Scotland, its repeal for England and Ireland must logically follow; and this Committee trusts that no steps may be taken to repeal the Act for Scotland."

In view of any proceedings which may be taken in the Session of 1879 in regard to the recommendations of the Scottish Herring-Fishery Commissioners, as well as on general grounds, the Committee urges its reappointment.

SECTION A.—MATHEMATICAL AND PHYSICAL.

Note on the Pedetic Action of Soap, by Prof. W. Stanley Jevons.—Since the publication in the *Quarterly Journal of Science* for April, 1878, of my paper on Pedesis, or the so-called Brownian movement of microscopic particles, it has been suggested to me that soap would form a good critical substance for experiment in relation to this phenomenon. It is the opinion of Prof. Barrett, and some other physicists, that the movement is due to surface tension, whereas, I believe that chemical and electromotive actions can alone explain the long-continued and extraordinary motions exhibited by minute particles of almost all substances under proper conditions. Soap considerably reduces the tension of water in which it is dissolved, without much affecting (as is said) its electric conductibility. If, then, pedesis be due to surface-tension, we should expect the motion to be killed, or much lessened when soap is added to water.

Having tried the experiment, I find that the result is of the opposite character to what Prof. Barrett anticipated. With a solution of common soap the pedetic motion becomes considerably more marked than before. I have observed this result not only with china clay and some other silicates, but also with such comparatively inert substances, as the red oxide of iron, chalk, and even the heavy powder of barium carbonate. The last-named substance, one of those which we should least expect to dance about of its own accord, gave a beautiful exhibition of the movement when mixed with a solution of about 1 per cent. of soap, and viewed with a magnifying power of 500 or 1,000 diameters.

The correctness of this result was also tested by observing the suspending power of solutions of soap-solution compared with water. If a little china clay be diffused through common impure water, that, for instance, of the London Water Companies, the greater part of the clay will soon be seen to collect together in small flocks and fall to the bottom in two or three hours, the water being almost clear. However, if about 1 per

cent. be dissolved in the water, the behaviour of the clay is quite different. The larger particles soon subside, but the smaller ones remain diffused through the liquid for a long time, giving it a milky appearance, quite different from the flocky and grainy appearance of the common water; if 1 per cent. of sodium carbonate be dissolved in common water, and china clay be mixed therewith, the subsidence of the clay is still more rapid, owing, as I have explained, to the increase in the electric conductivity of the fluid, and the consequent decrease of pedesis. But I now find that if soap be added at the same time, pedesis is not destroyed but considerably increased, and the clay remains a long time in suspension, two or three days at least.

These facts give a complete explanation of the detergent power of soap. It has long seemed to me unaccountable that for cleansing purposes the comparatively neutral soap should be better than the alkaline carbonate by itself; we are told that the alkali is but feebly combined with the stearic or other fatty acids. But why combine it at all if we need only the alkaline power of the base? The fact is that the detergent action of soap is due to pedesis, by which minute particles are loosened and diffused through the water so as to be readily carried off. Pure rain or distilled water has a high cleansing power, because it produces pedesis in a high degree. The hardness of impure water arises from the vast decrease of pedesis due to the salts in solution. Hence the inferior cleansing power of such water. If alkaline salts be added, dissolved in water, it becomes capable of acting upon oleaginous matter, but the pedetic power is lessened, not increased. But if soap be added also, we have the advantage both of the alkali dissolving power, and of the pedetic cleansing power. At the same time we have a clear explanation why silicate of soda is now largely used in making soap; for I have shown, in the paper referred to, that silicated soda is one of the few universal substances which increase the pedetic and suspensive power of water.

I believe that the detergent power of soap and water is one of the many important phenomena which may be explained by the study of pedesis, and I propose to follow up the investigation of this movement in regard to the several substances which tend to increase it.

Motions produced by Dilute Acids on some Amalgam Surfaces, by Robert Sabine.—The author finds, when a drop of very dilute acid is placed upon the clean and newly filtered surface of a rather rich amalgam of some metal which is positive to mercury, that the drop does not lie still as it would do upon pure mercury, but sets itself into an irregular jerky motion. This is the case with copper, zinc, antimony, tin, and lead amalgams. But if instead of these amalgams those of platinum, gold, and silver are used—these latter metals being negative to mercury—the drop of acid water lies quite still. The acids tried were sulphuric, hydrochloric, oxalic, and acetic, which behaved similarly but in different degrees. When the experiment is made in an atmosphere of oxygen the movements upon the amalgams of the positive metals are increased; but in hydrogen, carbonic acid, nitrogen, and coal-gas the motions are instantly arrested.

The author concludes that the motions result from an alternate play of deoxidation of the mercury underneath the acid by electrolysis, due to the currents of small floating particles of the positive metal causing the drop to contract, and of oxidation of the surface outside the acid-drop causing it to re-expand.

On Certain Phenomena accompanying Rainbows, by Prof. Silvanus P. Thompson.—The author narrated several instances of rainbows seen chiefly in Switzerland, where radial streaks of light devoid of colour were observed within the primary and without the secondary bow. The explanation suggested was as follows:—The wedge-shaped radial streaks are beams of sunlight, which become visible by diffuse reflection from particles of matter in their path, just as the apparently divergent beams of sunrise or sunset become visible. These "beams" being practically parallel to one another, appear to converge in the point exactly opposite to the sun by perspective, or, in fact, just as the parallel beams of sunset appear divergent. Since the rainbow has for its centre the point opposite the sun, such beams must have positions radial with respect to the bow. They resemble, therefore, the *rayons du crépuscule* occasionally seen in the east at sunset; they had never been observed crossing the dark span between the primary and secondary bows. A similar phenomenon of rays might sometimes be seen in sunlight, when the shadow of the observer fell upon a slightly turbid lake or river.

SECTION B.

CHEMICAL SCIENCE.

OPENING ADDRESS BY THE PRESIDENT, PROF. MAXWELL SIMPSON, M.D., F.R.S.

MY position here is a highly honourable, but by no means a comfortable one. Naturally you expect to hear from me something new about the science which occupies the attention of this section, and I have the miserable feeling that I must disappoint you. How can I possibly find a fact in chemistry with which you are not already acquainted? If, in order to cater for you, I go to France, Germany, Russia, or America, I find the abstractors of the Chemical Society have been there before me, and have swept everything of value into their journal. Chemists are now kept perfectly acquainted with the progress of science in every part of the world, and therefore the *raison d'être* of this address, so far as announcing the discoveries of the year is concerned, has passed away. I therefore propose instead of giving you a concentrated essence of the last twelve numbers of the *Journal* of the Chemical Society, to bring before you the claims of this science to a place in general education, and the claims of original research to a place in the curriculum for higher degrees in our universities.

I have been devoted to chemistry all my life. It has been my business and my pleasure. The longer I live the more deeply am I impressed with the advantages to be derived from its study, and I am anxious that these advantages should be shared by the rising generation.

Whether we take into account the value of the knowledge acquired, the discipline of the intellectual faculties in acquiring that knowledge, or the effect on the character, surely we have a right to give the study of this science a prominent place in our schools and colleges. It would be difficult to over-estimate the value and extent of the knowledge we derive from chemistry. Without it we can know nothing about the air we breathe, the water we drink, or the food we eat; we cannot understand the processes of combustion, respiration, fermentation, putrefaction, or the endless chemical changes which are continually in operation around us, and which affect our lives for good or for evil. In a word, the whole of the phenomena of nature must for ever remain to us, more or less, an inscrutable mystery.

Again, is it not desirable that we should have some acquaintance with the chemical arts, from which we derive so many of our comforts and luxuries? Should we not know something of the arts of photography, dyeing, metallurgy—something of the manufacture of glass and china, and of the thousand beautiful things that are constantly in our hands? Not only is the knowledge we obtain from chemistry very considerable in itself, but it furnishes us with a key which enables us to unlock vast stores of knowledge contained in several other sciences—these are, physics, geology, mineralogy, physiology, and I may now add, astronomy. Physics and chemistry are so intimately connected that it is difficult to say where the one begins and the other ends. The help that chemistry gives to physics is shown by the numbers of chemists who have distinguished themselves as physicists. I may mention a few belonging to our own time—Andrews, Bunsen, Faraday, Frankland, Graham, Guthrie, and Regnault.

With regard to mental discipline, the mind of the student is exercised in both the inductive and deductive methods of reasoning. His original faculties are stimulated by the consciousness that he can in many cases readily test the worth of his ideas by experiment. With inexpensive apparatus and a good balance, the intelligent student can make out for himself some of the laws and many of the facts of the science, and, it may be, also add to them. He glides insensibly from the known to the unknown. Indeed his spirit of inquiry demands, in most cases, to be curbed rather than spurred. Some students are constantly finding out new methods of analysis or discovering the precious metals in impossible places.

The readiness with which we can cross over into the *terra incognita* of chemistry and make little explorations there, constitutes, in my opinion, the great charm of this science, and, to a great extent, its value as an educational agent. What I wish to insist upon is that the student of chemistry can reach the field of original work sooner than the student of most other sciences. Once he commences original research the development of his intellectual faculties rapidly progresses. His imagination is daily exercised in propounding new theories and devising experiments in order to ascertain their truth or

falsehood. And what more valuable intellectual training can there be than the habit of subjecting our ideas to the test of inexorable experiment? In the world outside chemistry we are, alas! too ready to take things for granted. The chemist's motto is *prove all things*. The ancients adopted a different method: they assumed certain principles and reasoned from them. They therefore did little in science.

Chemistry promotes in a remarkable manner accuracy, thoroughness, and circumspection. An organic analysis requires six weighings: if any one of these is inaccurate, the results are worthless. A qualitative test carelessly applied may cause us, in a research, to waste months in the pursuit of a phantom or Will-o'-the-Wisp which can have no corporeal existence. If we have to employ absolute alcohol in our experiments, we must not be satisfied with going through the ceremony of making it absolute, but we must assure ourselves that it *is* absolute. Unless we are sure of every step in our research, our results become doubtful, and therefore of no value.

On the circumspection, also, of the original worker large demands are made. The avenues by which error may creep in and vitiate his results are very numerous. These he must foresee, and endeavour to close up. Laboratory work teaches us to use our senses aright, sharpens our powers of observation, and prevents us from reasoning rashly from appearances. It also promotes manual dexterity, and trains the hands to work in subordination to the head.

Perhaps in no other science is the student so deeply impressed with the order and economy of nature, the immutability of her laws, and the exactness of her operations. These impressions will, no doubt, in after life impart seriousness to his character, and save him from the adoption of many a wild theory.

I come now to the effect of original work on the character. Many virtues are necessary to the chemist—courage, resolution, truthfulness, and patience. He is often obliged to perform experiments which are attended with great danger, and no man can hope to fight long with the elements without carrying away many a scar. Sometimes fatal accidents occur. Many years ago Mr. Hennel, of the Apothecaries' Hall, London, lost his life by the explosion of a fulminating powder which he was preparing for the East India Company. And many of us recollect the sad death of young Mr. Chapman, a distinguished chemist whom I had the pleasure of knowing, who was literally blown to atoms while working in the Hartz Mountains on a new dynamite which he had himself discovered. I must tell the ladies, however, that accidents are not always so disastrous, but that often one may escape with merely the loss of an eye. But the chemist must not be discouraged by fear of accident, neither must he be disheartened by the temporary failure of his experiments, nor at the slowness of his processes. Bunsen was obliged to evaporate forty-four tons of the waters of the Dürcheim springs in order to obtain 200 grains of his new metal, cæsium. It took Berthelot several months to form, by a series of synthetical operations, an appreciable quantity of alcohol from water and carbon, derived from carbonate of baryta. Many years ago, in the laboratory of Wurtz—my honoured master—a poor student, whom I knew, was carrying from one room to another a glass globe which contained the product of a month's continuous labour, when the bottom of the globe fell out, and the contents were lost. Nothing daunted, he recommenced his month's work, and brought his research to a successful issue.

Above all things, the chemist must be *true*. He must not allow his wishes to bias his judgment or prevent him from seeing his researches in their true light. He must not be satisfied that his results appear true, but he must believe them to be true; and having faithfully performed his experiments, he must record them faithfully. He may often be obliged to chronicle his own failures and describe operations that tell against his own theories, but this hard test of his truthfulness he must not shrink from.

But I must not weary you with the virtues of the chemist. If I have succeeded in showing that the pursuit of this science tends largely to develop the intellect and discipline the character, I think I have done something for chemistry. We are told by Bishop Butler that "habits of virtue acquired by discipline are improvement in virtue, and improvement in virtue must be advancement in happiness."

I am glad to see that the importance of original research as a part of higher education is at last beginning to be recognised in this country. The Royal University Commission at Oxford has recently recommended that candidates for the higher degrees in science shall in that university be required in future to work out an original investigation. In Germany, where education has been so long and so well understood, original work has been, for at least the last half century, a *sine quâ non* for a degree. Another admirable rule exists in that country, the adoption of which in Great Britain might go far to wash out the stain from our islands, of not having contributed our fair quota to the advancement of human knowledge. It is this—the Germans make a point of securing invariably that their scientific chairs shall be filled by men who have already distinguished themselves by their discoveries. The professor, on his appointment, naturally desires to continue his investigations, and endeavours to secure, and usually succeeds in securing, the assistance of his pupils. This is a mutual advantage. The professor is able to do more work for science, and the student, on his part, learns to conduct for himself an original investigation. Hence there is always a rising generation of original workers in Germany, who turn out papers more or less meritorious with the rapidity of a Walter's press. They are stimulated by the hope of one day arriving themselves at a professor's chair, the path to which they are well assured is only through the toilsome field of original work. But I must not wrong the German student by the implication of a purely selfish motive in his work. His labour is one of love, and his ambition, for the time at least, is bounded by the desire to *do something* for science. And from a multitude of such enthusiasts the great professors come. Great mountains are only found in mountainous countries.

I find myself insensibly led to speak of the encouragement of research in this country, and, although it has been very largely discussed in scientific circles, I will venture to add a few words. To promote original work here, I believe it is indispensable that our professors should be well paid. It would save them from the necessity of supplementing their incomes by commercial analyses, and thus enable them to devote their spare time to original work. And to secure that they shall have spare time, I would like to see in every laboratory a competent assistant, who would be able occasionally to take up the professor's lectures, should he be engaged in important work. There are many around me who know how very exacting original investigation is, and how necessary it is, at times, to be able to work on without interruption, bits and scraps of time being of no value. I am glad to see that the Oxford Commission also recommends the appointment of well-paid assistants. Well-paid professorships and well-paid assistantships would be attractive prizes for our students to work up to; and if it were clearly understood that the only way to these prizes was through original investigation, we should very soon have an army of zealous and competent workers.

The plan of appointing a staff of original workers unconnected with teaching has been proposed; but I do not approve of it. The original worker is, as a rule, the best teacher, and the rising generation of students should not be deprived of the advantage of his instruction. Moreover, as I said before, the professor may be greatly assisted by his pupils.

No doubt the Government Grant Fund does a good deal for science, but the field of its operations is, under present conditions, limited. Professors, as a rule, are so occupied with teaching that they cannot avail themselves of the fund; and of those students who might be competent and willing, very few can afford to do so. Instead of trusting to the precarious and insufficient support of the fund they must endeavour to settle themselves permanently in life.

It is much to be regretted that the universities of Oxford and Cambridge, with such splendid revenues at their disposal, should contribute so little to the advancement of physical science. I hope the day is not far distant when the Fellowships—or at least a few of them—which now go to reward young men for merely passing a good examination, shall be given *without examination* to men who shall have advanced human knowledge in any department. At present, a Fellowship of 250*l*. or 300*l*. a-year, lasting ten or twelve years, and in some cases for life, may be obtained on showing proof of a good memory—or, at most, a capacity for assimilating other men's ideas. To make discoveries—to follow out a new train of thought, and establish it by experiments specially devised to that end, has been left not only without reward, but almost without recognition, in our two principal seats of learning. Is it to be so always? The world at large, ignorant as it is, has a sounder instinct on this subject, and the man who makes the humblest addition to the stock of knowledge in the world rarely fails to receive the world's respect and honour.

The suggestions I have ventured to make could not, of course, be well carried out, unless the government take into its own hands the appointment to all scientific chairs. Of this I think I see indications. I believe that sooner or later the government will assume the supreme direction of education in this country. It has already taken primary education under its control, and quite recently, here in Ireland, intermediate education to a great extent. And does the appointment of so many university commissions not show a disposition on the part of the government to assume the direction of higher education also?

SECTION C.—GEOLOGY.

The Origin and the Succession of the Crystalline Rocks, by Prof. J. Sterry Hunt, LL.D., F.R.S.—As a preliminary to a statement of the results of many years of study of the crystalline rocks in North America, the author proceeded to consider the question of their origin, which is still a subject of debate between plutonists and neptunists. The crystalline silicate rocks naturally divide themselves into three groups, namely, those indigenous stratified formations which have been called primitive or primary, those masses to which, from their relations to contiguous rocks, geologists assign an exotic origin, and in accordance with a generally-accepted theory, have agreed to call igneous or plutonic; and a third and distinct group of rock-masses which, though like the last, clearly posterior to those encasing them, are now, by most geologists, admitted to be of aqueous origin. This third group includes metalliferous lodes and various other crystalline veinstones, and is conveniently designated endogenous. It is not always easy to distinguish between the rocks of these three groups; there are not wanting those who have assigned an igneous origin to metalliferous lodes, and many still confound endogenous granitic veins with the mineralogically similar plutonic granites. In like manner the distinction between the latter and the stratified granitoid gneisses is frequently not very apparent. That the movement of flow in extravasated plutonic rocks may give to their constituent minerals a stratiform arrangement, is a fact of which both exotic granites and doleritic dykes and masses afford illustrations. Moreover, the arrangement due to successive depositions upon the walls of a fissure may give to an endogenous mass a structure which simulates that of a sedimentary rock, and imparting to granitic veinstones a resemblance to gneiss; while a laminated structure sometimes results from the arrangement of the crystals developed in a cooling mass. Hence there are not wanting those who include under the head of plutonic rocks not only the clearly marked exotic granites, dolerites, and diorites, but the granitoid gneisses, the massive bedded greenstones, and likewise the more schistose rocks with which these gneisses and greenstones are often so intimately associated that it is difficult to separate them. According to those who hold this plutonic view, the crystalline rocks represent the igneous crust of the globe, and their frequent stratiform structure is due to agencies in great part anterior to the production of sedimentary rocks. In opposition to this view is that of the neptunist, who, starting from the fact that the elements of an aqueous sediment may, through the action of chemical and crystallogenic forces, pass into new combinations and acquire a new structure, argues not only that all indigenous crystalline rocks have had an aqueous origin, but that the exotic masses themselves represent the last stages of this process of alteration or metamorphosis of sedimentary beds.

Further inquiry into the chemical and lithological composition of the crystalline rocks, however, brings to light difficulties in the way of both of these hypotheses. To begin with the plutonist view, volcanic rocks, both ancient and modern, are more or less nearly related in composition to the gneisses and the stratified greenstones, but we seek in vain among undoubted volcanic or igneous rocks for the chemical representatives of the masses of serpentine, olivine, steatite, chlorite, quartzite, magnetite, oligist, and limestone, which appear in the primary formations, and have, all of them, by geologists of the school in question, been regarded as of igneous or plutonic origin. To account for the presence of such rocks among the more or less feldspathic aggregates—chiefly gneisses and greenstones—which make up the greater portion of the crystalline formations, three hypotheses have been imagined by plutonists. According to the first of these the earth's interior is a reservoir from which, at times, have been ejected not only basic and acidic feldspathic rocks, but molten masses of olivine, iron-oxyde, quartz, and limestone. Other geologists of this school have sought to account for the presence of some of these exceptional rocks by a process of so-called segregation, which would assimilate them to endogenous masses. The chemical and geognostical difficulties in the way of both of these hypotheses have, however, led to their general rejection for the third, which supposes these rocks to have been formed by a subsequent local alteration of portions of the ordinary plutonic rocks.

From acknowledged cases of alteration or replacement in mineral species which result in pseudomorphs, and from the more frequent cases of envelopment and of isomorphism, which have been taken for examples of pseudomorphism, it was argued that many species are capable of being changed into others by the loss or addition of certain elements, so that the resulting body often contains no portion of its original constituents. Extending this view from single crystals to rock-masses, it was maintained that different portions of an igneous or plutonic formation, whether basic or acidic, might be transformed into serpentine, chlorite, or limestone. These changes were supposed to depend on the action of water, which, aided by heat, was regarded as the efficient agent in the local alterations of plutonic rocks. At the same time the adjacent sedimentary strata were supposed to share in these changes, thus giving rise to what have been called contact-formations. In their latest form these doctrines have been well set forth by von Lasaulx and by Knop. This third hypothesis, then, proposes to account for the presence of various exceptional varieties of rock among ordinary plutonic formations by supposing that limited portions of these have, at different times, been the subject of very unlike chemical processes, resulting in their complete change into new forms of rock by what has been called pseudomorphic alteration, or metamorphosis. As, however, such a conversion involves a change not only of form but of substance, it has been more properly designated a metasomatosis.

We have next to consider the neptunean view as ordinarily expounded. This, while it accounts by sedimentation for the stratiform arrangement of the crystalline rocks and explains the existence therein of beds of iron ores and limestones, still presents many of the difficulties which are encountered in the plutonist view. If, as most neptunists maintain, the great crystalline series have been derived from the alteration of uncrystalline ones, which were not only similar to those of palæozoic and more recent times, but are, in fact, portions of these which in adjacent regions are still known to us in their original unchanged condition, how are we to explain the genesis of the feldspathic and hornblendic rocks which predominate in these crystalline formations? The sandstones and shales from which, in this view, they are supposed to be formed, could never, by themselves, give rise to the rocks in question, since they are deficient in the alkalis, and to a greater or less extent in the other bases required for the production of the constituent silicates. To explain their origin, therefore, it becomes necessary to admit the introduction of these various bases from without, and to suppose a series of metasomatic processes more wonderful than those imagined by the plutonist. The latter, by his hypothesis, has already at hand feldspathic and hornblendic rocks which are to be the subjects of metasomatosis, while the neptunist has only the products of their decay.

In either hypothesis, we have to account for the presence, in the primary formations, of beds and interstratified masses of a great number of exceptional silicated rocks very distinct in composition from any mechanically-formed sediments, including not only silicates like serpentine, olivine, steatite, chlorite, pinite, garnet, epidote, and hornblende, but of pure orthoclase, as well as of triclinic feldspars. Each of these species would require, for its production from any ordinary igneous or aqueous rock, a separate and independent metasomatic process—involving the addition of certain elements and the abstraction of others—until the whole heterogeneous crystalline series was complete. The author illustrated these views by examples from recent writers, and concluded that the hypothesis of metasomatosis, as maintained both by plutonists and neptunists, supposes the operation in solid rocks of processes of circulation, absorption, elimination, selection, and aggregation scarcely to be equalled in the economy of highly-organised beings, and not easily imagined in the masses of the mineral kingdom.

Certain geologists suppose the existence of two classes of crystalline stratified rocks: the one neptunean, and consisting

of altered portions of palæozoic or more recent sediments, and the other—more ancient—which may be either neptunean or plutonic in origin. The history of geology gives many examples of crystalline formations which have been, in turn, assigned to various geological horizons from the cainozoic to the base of the palæozoic, but have since been found to belong to a pre-palæozoic period. In the opinion of the author we have no good and sufficient reason for believing in the present existence of any uncrystalline representative of these crystalline formations, or of any such formation which is not pre-Silurian, if not pre-Cambrian, in age. There are, however, many examples of local alterations of later sediments by hydro-thermal action, which has developed in these many crystalline minerals identical with those found in the more ancient rocks. The advocates of the neptunean hypothesis have, for the most part, sought for the origin of the crystalline rocks in sediments of a later date, of which the uncrystalline representatives are still to be found. There are, however, reasons for believing that in eozoic, or pre-Cambrian times, there prevailed chemical activities, dependent upon greater subterranean temperature, different atmospheric conditions, and abundance of thermal waters, and that under these circumstances were deposited the materials for the crystalline rocks. There have not been wanting those who have sought in similar hypothetical conditions for the origin of these rocks. De la Beche, in 1834, imagined them to be chemical deposits, due to the action of the heated ocean upon the earth's primeval crust before the dawn of life.

The author's researches into the composition and structure of the crystalline rocks, conjoined with his studies of the chemistry of natural waters, led him, in 1860, to reject the hitherto received view of the epigenic or metasomatic origin of serpentine, steatite, chlorite, and similar rocks, and to maintain their derivation from silicates formed by chemical processes and deposited in the water of lakes or seas. This view he soon after extended to the various other exceptional rocks found in crystalline formations, which it was in 1864 asserted, had been "formed by a crystalline and molecular re-arrangement of silicates generated by chemical processes in waters at the earth's surface." In elucidation of this view the author referred to the insoluble silicates now separated in the evaporation of many natural waters, to the formation from the earliest times to the present of deposits of serpentine, sepiolite, glauconite, and of aluminous silicates allied to chlorite, which are found either forming beds or filling the cavities of various marine organic forms from the foraminifers of to-day to the crinoids of palæozoic time, and the eozoon of the Laurentian. The formation in modern times of crystalline zeolites and quartz in thermal waters was also cited in illustration of this view of the generation of various mineral silicates by causes now in operation, which, it is believed, were far more active in eozoic times. This was not, as had been already suggested by others, a process confined to a seething primeval ocean before the advent of life, but was continued through long ages under varying chemical conditions, and was contemporaneous with the deposition of successive strata of limestone and detrital matters. The argillaceous portions of these, it is conceived, may have taken part in the reactions with thermal waters.

We have thus, in the opinion of the author, a reasonable mode of accounting for the origin of the various rocks of the crystalline formation, and a consistent and complete neptunean theory, which does not invoke the aid of metasomatosis. It has, since it was proposed eighteen years ago, met with the approval of many whose studies have made them the fittest judges of its reasonableness. Among those who have either formally given their adhesion to it, or have enunciated similar views, may be mentioned the names of Delesse, Renard, Gümbel, Credner, Alphonse Favre, and Gastaldi.

The chemical activities concerned in the production of the various silicates have doubtless suffered gradual change and diminution through the successive ages of eozoic time, from which have resulted mineralogical and lithological differences in the crystalline terranes. Each of these includes quartzites and limestones, in which latter certain silicates, such as serpentine, hornblende, and micas, are occasionally found. It is in those aluminiferous rocks, which are without lime or magnesia, that are seen the essential and characteristic differences dependent, as long ago pointed out by the author, upon a decrease in the proportion of alkalis. As we pass from the older to the younger of the eozoic terranes, the feldspar, orthoclase, and albite, become partially or wholly replaced by silicates like muscovite, damonite, and paragonite, and finally by andalusite, fibrolite, cyanite, and pyrophyllite.

The author alluded briefly to the changes by which the ancient aqueous deposits were transformed into crystalline stratified rocks by what Gümbel has designated diagenesis, as distinguished from their supposed origin by epigenesis or metasomatic change. The question of the relation of the indigenous crystalline rocks to the endogenous and exotic masses included in them was noticed, the author alluding to the hypothesis, which he has elsewhere maintained, that the source of all exotic or eruptive rocks is to be found in the displacement or extravasation of ancient deposits of neptunean origin.

Coming to the second division of his subject, the author asserted that the study of the crystalline rocks of North America shows the existence of several distinct groups or terraces.

The Laurentian, which is the most ancient, includes in its lower part a mass of unknown thickness of granitoid gneiss, often hornblendic (Ottawa gneiss), succeeded—perhaps unconformably—by what has been called the Grenville series, consisting of similar gneisses and hornblendic rocks with intercalated quartzites and iron ores. These two divisions make up together the lower Laurentian of Logan, of which the thickness in Canada may greatly exceed 20,000 feet.

The Norian, which is the upper Laurentian or Labradorian of Logan, rests unconformably upon the Laurentian, and is remarkable for a great development of rocks composed chiefly of labradorite or related plagioclase feldspars, which have been called labradorite-rock or norite. The interstratified gneisses, quartzites, and limestones of the Norian are not unlike those of the Laurentian. This series, which abounds in great beds of titanic iron-ore, has a great volume which may exceed the thickness of 10,000 feet assigned to it by Logan.

The Laurentian is in many parts unconformably overlaid by the Huronian series, which is characterised by a great development of greenstones, generally hornblendic, with epidote, chlorite, steatite, serpentine, and soft hydrous mica-schists, often called talcose, besides argillites, quartzites, and limestones, generally magnesian. It abounds in metalliferous deposits, including magnetic and specular iron-ores, chrome, and sulphurets of copper, iron, and nickel, and has had assigned to it in different regions a thickness of from ten to twenty thousand feet.

In many parts of North America there exists a great development of rocks characterised by the predominance of orthofelsite or petrosilex, often becoming a quartziferous porphyry. This, which is apparently the *hälleflinta* of Sweden, was regarded as eruptive until, in 1869, the author showed it to be a stratified series with some associated quartzites and schists, and then included it in the lower part of the Huronian. Hitchcock, who has since studied these rocks in New Hampshire, has called them lower Huronian. From their absence in many localities at the base of the typical Huronian, it is conjectured that they may belong to a more ancient and distinct series.

The Montalban or White Mountain series is characterised by micaceous gneisses, generally called granites, which pass into quartzose and feldspathic mica-schists, often abounding in garnet, staurolite, fibrolite, and cyanite. Great masses of dark green gneissoid hornblendic rock, very distinct from the Huronian greenstones, abound in the Montalban, which also includes beds of a peculiar olivine rock, besides quartzites and crystalline limestones. This series abounds in endogenous granitic veins, containing muscovite, beryl, tourmaline, apatite, and oxide of tin. It probably equals the Huronian in thickness, and is supposed to overlie it.

The Taconian series includes a great volume of characteristic mica-schists, often quartzose, but seldom distinctly feldspathic, and frequently consisting in large part of damonite, or of pyrophyllite. Some of these, like the schists of the Montalban, include garnet and chiastolite. They are associated with quartzites and with dolomites and limestones, all of which are also frequently micaceous. Associated with these are found serpentines and granular hornblendic rocks of a peculiar type, very unlike those of the preceding groups and much less crystalline. The quartzites are in large part detrital rocks. This series, which yields the statuary marbles of North America, has a thickness of about 5,000 feet and is the lower Taconic of Emmons. It is found reposing alike on the Laurentian, Huronian, and Montalban, and is overlaid, in apparent unconformity, by the upper Taconic, which is identical with the Quebec group of Logan. This, which consists of many thousand feet of sandstones and argillites, with some limestones, includes

among its strata organic forms belonging to various divisions of the Cambrian up to the Arenig. The Taconian, although containing an undescribed linguloid shell and a so-called scolithus, is by the author considered provisionally as distinct from the Cambrian. It has yielded in Ontario, besides scolithus, the Eozoon Canadense, and may perhaps be regarded as the connecting link between the eozoic and palæozoic ages.

The upper Taconic, or so-called Quebec group in Eastern North America, is separated by a stratigraphical break from the succeeding portion of the Cambrian, the Bala group (Trenton, Utica, and Loraine), while, on the contrary, the supposed discordance in the regions just mentioned at the summit of the latter, corresponding to the division between Cambrian and Silurian in Wales, appears to have been based on a misconception. There is, however, an important palæontological break at this horizon, connected with a great deposit of barren detrital rocks, which marked the close of the Cambrian period, and the author records his opinion that the name of lower Silurian, as well as that of Siluro-Cambrian, which he, with others, has applied to the Bala or upper division of the Cambrian is to be rejected as being historically incorrect and as tending to perpetuate false views of the palæontological relations between these and the succeeding rocks.

The early advocates in North America of the metamorphism of palæozoic rocks taught, in the first place, the stratigraphical equivalence of the upper Taconic (or lower and middle Cambrian) with the upper Cambrian, and further maintained that these rocks had suffered various degrees and various kinds of metamorphism, as the result of which they had assumed, in different areas, the characters of the Taconian, the Montalban, and Huronian, and the Laurentian; the lithological differences between these several series being regarded as marks of the greater or less alteration which, it was supposed, these uncrystalline Cambrian sediments had undergone. Other geologists have imagined portions of these same crystalline formations in North America to be altered strata of Silurian, Devonian, and even of triassic age.

The great groups of eozoic rocks already described constitute, however, in the author's opinion, as many great stratified series which, before the Cambrian time, existed in their present crystalline condition, and had been successively subjected to the accidents of uplift, contortion, and denudation, so that the newer eozoic groups were, at the beginning of the paleozoic period, distributed irregularly over the floor of fundamental Laurentian gneiss. These various crystalline groups are found, with a singular persistence and uniformity of lithological character, from Alabama to Newfoundland, along the Atlantic belt, and thence westward through Canada to the great lakes, and beyond, in the vast regions of the Cordilleras to the Pacific slope.

The author had some years since pointed out the remarkable similarity between these various crystalline groups of North America and the crystalline rocks of the British Islands, and had lately been able, by new observations, to confirm his conclusions. Among the crystalline formations of Donegal he had indicated representatives of Laurentian, Montalban, and Huronian, and the latter he had recently observed largely developed in Argyleshire and Perthshire. To the Huronian also he refers the green schists of Anglesea and Carnarvonshire, in both of which regions the orthofelsite or hälleflinta series at the base of the Huronian (the so-called porphyries), and likewise the more ancient gneisses, are well represented. He would, however, leave this subject to his friend Dr. Henry Hicks, who has so happily mastered the obscure problems of the pre-Cambrian geology of Wales. The studies of Gastaldi and others enable us to assert that similar series of ancient rocks occur in the same order in the Alps; and we infer that the chemical and physical conditions which presided over the production of the crystalline stratified rocks were world-wide.

SECTION D.

BIOLOGY.

Department of Anatomy and Physiology.

ADDRESS BY R. MCDONNELL, M.D., F.R.S., VICE-PRESIDENT OF THE SECTION.

SINCE this Association met twelve months ago the science of physiology has suffered an irreparable loss. In February last Claude Bernard died in the sixty-fifth year of his age. He was interred with a degree of pomp never in this country, and rarely even in France, accorded to men of science. His country showed how justly and how highly they estimated the merit of a man who, gentle, unobtrusive, modest, by the greatness of his genius and the brilliancy of his many discoveries shed a lustre on the land which gave him birth. It was my privilege to have been at one time a pupil of this illustrious physiologist. It will be my pride if I can show to a thoughtful and cultivated audience, such as I have now the honour to address, that the discoveries of my honoured master, although of necessity made by experiment on animals, have added much to that stock of knowledge which has conferred the greatest benefits upon mankind. In an address like this—limited to a short time—it would not be possible to give a detailed account of the work accomplished by Bernard. To do so would be to give a history of the progress of physiology for the last thirty-five years. His researches were so extended, and some of his discoveries so vast, that by comparison they seem to make others appear small, as the gigantic Californian pine seems to dwarf a goodly-sized oak which grows alongside it. Hence, we speak of Bernard's less important researches—of his minor discoveries, although of sufficient magnitude to have seemed great if made by another. Of these I cannot speak at length, yet some of my hearers will know that the services which Bernard has rendered to science by his researches on the pneumogastric nerves, the fifth pair, the chorda tympani, the facial, &c., are not small. Assuredly, the same may be said for his observations on "recurrent sensibility;" on the blood pressure and the gases of the blood; on the variations of colour of this fluid, according to the active or passive condition of the functions of the organ traversed by it; on the variations of temperature during these conditions of functional activity or inactivity; on the elective elimination by the glands of substances introduced into the economy, or of those which, as morbid products, accumulate in the system as the result of certain morbid states; on the special character and action of the varieties of the salivary secretions; upon the influence of the nervous centres on the secretion of saliva; on the electric phenomena manifested in nerve and muscle; on albuminuria connected with lesions of the nervous system; and (notably in its important practical bearings on uræmia) on the modifications of the secretions of the stomach and intestines after arrest of the elimination of urea through the natural channels. Claude Bernard, in truth, left his mark deeply on every aspect of physiology on which he touched. His discoveries, however, as regards the functions of the pancreas, of the liver, and concerning the vaso-motor system of nerves, are those on which his fame will ever chiefly rest.

Dr. McDonnell then dwelt in detail on the importance of Bernard's researches from a medical point of view.

Department of Anthropology.

The business of this department was opened by the following address from Prof. Huxley.

When I undertook, with the greatest possible pleasure, to act as a lieutenant of my friend the president of this Section, I steadfastly purposed to confine myself to the modest and useful duties of that position. For reasons, with which it is not worth while to trouble you, I did not propose to follow the custom which has grown up in the Association of delivering an address upon the occasion of taking the chair of a section or department. In clear memory of the admirable addresses which you have had the privilege of hearing from Prof. Flower, and just now from Dr. M'Donnell, I cannot doubt that that practice is a very good one; but I would venture to say, to use a term of philosophy, that it looks very much better from an objective than from a subjective point of view. But I found that my resolution, like a great many good resolutions that I have made in the course of my life, came to very little, and that it was thought desirable that I should address you in some way. But I must beg of you to understand that this is no formal address. I have simply announced it as a few introductory remarks, and I must ask you to forgive whatever of crudity and imperfection there may be in the mode of expression of what I have to say, although naturally I shall do my best to take care that there is neither crudity nor inaccuracy in the substance of it. It has occurred to me that I might address myself to a point in connection with the business of this department which forces itself more or less upon the attention of everybody, and which, unless the bellicose instincts of human nature are less marked on this side of St. George's Channel than on the other, may possibly have something to do with the large audiences we are always accustomed to see in the anthropological

department. In the Geological Section I have no doubt it will be pointed out to you, or, at any rate, such knowledge may crop up incidentally, that there are on the earth's surface what are called *loci* of disturbance, where, for long ages, cataclysms and outbursts of lava and the like take place. Then everything subsides into quietude; but a similar disturbance is set up elsewhere. In Antrim, at the middle of the tertiary epoch, there was such a great centre of physical disturbance. We all know that at the present time the earth's crust, at any rate, is quiet in Antrim, while the great centres of local disturbance are in Sicily, in Southern Italy, in the Andes, and elsewhere. My experience of the British Association does not extend quite over a geological epoch, but it does go back rather longer than I care to think about; and when I first knew the British Association, the *locus* of disturbance in it was the Geological Section. All sorts of terrible things about the antiquity of the earth, and I know not what else, were being said there, which gave rise to terrible apprehensions. The whole world, it was thought, was coming to an end, just as I have no doubt that, if there were any human inhabitants of Antrim in the middle of the tertiary epoch, when those great lava streams burst out, they would not have had the smallest question that the whole universe was going to pieces. Well, the universe has not gone to pieces. Antrim is, geologically speaking, a very quiet place now, as well cultivated a place as one need see, and yielding abundance of excellent produce; and so, if we turn to the Geological Section, nothing can be milder than the proceedings of that admirable body. All the difficulties that they seemed to have encountered at first have died away, and statements that were the horrible paradoxes of that generation are now the commonplaces of schoolboys. At present the *locus* of disturbance is to be found in the Biological Section, and more particularly in the anthropological department of that Section. History repeats itself, and precisely the same terrible apprehensions which were expressed by the aborigines of the Geological Section, in long far back time, is at present expressed by those who attend our deliberations. The world is coming to an end, the basis of morality is being shaken, and I don't know what is not to happen if certain conclusions which appear probable are to be verified. Well, now, whoever may be here thirty years hence—I certainly cannot—but, depend upon it, whoever may be speaking at the meeting of this department of the British Association thirty years hence will find, exactly as the members of the Geological Section have found, on looking back thirty years, that the very paradoxes and conclusions, and other horrible things that are now thought to be going to shake the foundations of the world will by that time have become parts of every-day knowledge and will be taught in our schools as accepted truth, and nobody will be one whit the worse.

The considerations which I think it desirable to put before you in order to show the foundations of the conclusions at which I have very confidently arrived, are of two kinds. The first is a reason based entirely upon philosophical considerations, namely, this—that the region of pure physical science, and the region of those questions which specially interest ordinary humanity, are apart, and that the conclusions reached in the one have no direct effect in the other. If you acquaint yourself with the history of philosophy, and with the endless variations of human opinion therein recorded, you will find that there is not a single one of those speculative difficulties which at the present time torment many minds as being the direct product of scientific thought, which is not as old as the times of Greek philosophy, and which did not then exist as strongly and as clearly as they do now, though they arose out of arguments based upon merely philosophical ideas. Whoever admits these two things—as everybody who looks about him must do—whoever takes into account the existence of evil in this world and the law of causation—has before him all the difficulties that can be raised by any form of scientific speculation. And these two difficulties have been occupying the minds of men ever since man began to think. The other consideration I have to put before you is that, whatever may be the results at which physical science as applied to man shall arrive, those results are inevitable—I mean that they arise out of the necessary progress of scientific thought as applied to man. You all, I hope, had the opportunity of hearing the excellent address which was given by our president yesterday, in which he traced out the marvellous progress of our knowledge of the higher animals which has been effected since the time of Linnæus. It is no exaggeration to say that at this present time the merest tyro knows a thousand times as much on the subject as is contained in the work of Linnæus, which was then the standard authority. Now how has that been brought about? If you consider what zoology, or the study of animals, signifies, you will see that it means an endeavour to ascertain all that can be studied, all the answers that can be given respecting any animal under four possible points of view. The first of these embraces considerations of structure. An animal has a certain structure, a certain mode of development, which means a series of stages in that structure. In the second place, every animal exhibits a great number of active powers, the knowledge of which constitutes its physiology; and under those active powers we have, as physiologists, not only to include such matters as have been referred to by Dr. M'Donnell in his observations, but to take into account other kinds of activity. I see it announced that the Zoological Section of to-day is to have a highly interesting paper by Sir John Lubbock on the habits of ants. Ants have a polity, and exhibit a certain amount of intelligence, and all these matters are proper subjects for the study of the zoologist as far as he deals with the ant. There is yet a third point of view in which you may regard every animal. It has a distribution. Not only is it to be found somewhere on the earth's surface, but palæontology tells us, if we go back in time, that the great majority of animals have had a past history—that they occurred in epochs of the world's history far removed from the present. And when we have acquired all that knowledge which we may enumerate under the heads of anatomy, physiology, and distribution, there remains still the problem of problems to the zoologist, which is the study of the causes of those phenomena, in order that we may know how those things came about. All these different forms of knowledge and inquiry are legitimate subjects for science, there being no subject which is an illegitimate subject for scientific inquiry, except such as involves a contradiction in terms, or is itself absurd. Indeed, I don't know that I ought to go quite so far as this at present, for, undoubtedly, there are many benighted persons who have been in the habit of calling by no less hard names conceptions which our president tells us must be regarded with much respect. If we have four dimensions of space we may have forty dimensions, and that would be a long way beyond that which is conceivable by ordinary powers of imagination. I should, therefore, not like to draw too closely the limits as to what may be contradiction to the best established principles. Now, let us turn to a proposition which no one can possibly deny—namely, that there is a distinct sense in which man is an animal. There is not the smallest doubt of that proposition. If anybody entertains a misgiving on that point he has simply to walk through the museum close by in order to see that man has a structure and a framework which may be compared, point for point and bone for bone, with those of the lower animals. There is not the smallest doubt moreover that, as to the manner of his becoming, man is developed, step by step, in exactly the same way as they are. There is not the smallest doubt that his activities—not only his mere bodily functions, but his other functions—are just as much the subjects of scientific study as are those of ants or bees. What we call the phenomena of intelligence, for example (as to what else there may be in them, the anthropologist makes no assertion)—are phenomena following a definite causal order just as capable of scientific examination, and of being reduced to definite law, as are all those phenomena which we call physical. And just as ants form a polity and a social state, and just as these are the proper and legitimate study of the zoologist, so far as he deals with ants, so do men organise themselves into a social state, and though the province of politics is of course outside that of anthropology, yet the consideration of man, so far as his instincts lead him to construct a social economy, is a legitimate and proper part of anthropology, precisely in the same way as the study of the social state of ants is a legitimate object of zoology. So with regard to other and more subtle phenomena. It has often been disputed whether in animals there is any trace of the religious sentiment. That is a legitimate subject of dispute and of inquiry; and if it were possible for my friend Sir John Lubbock to point out to you that ants manifest such sentiments he would have made a very great and interesting discovery, and no one could doubt that the ascertainment of such a fact was completely within the province of zoology. Anthropology has nothing to do with the truth or falsehood of religion—it holds itself absolutely and entirely aloof from such questions—but the natural history of religion, and the origin and the

growth of the religions entertained by the different kinds of the human race are within its proper and legitimate province. I now go a step farther, and pass to the distribution of man. Here, of course, the anthropologist is in his special region. He endeavours to ascertain how various modifications of the human stock are arranged upon the earth's surface. He looks back to the past, and inquires how far the remains of man can be traced. It is just as legitimate to ascertain how far the human race goes back in time as it is to ascertain how far the horse goes back in time; the kind of evidence that is good in the one case is good in the other; and the conclusions that are forced on us in the one case are forced on us in the other also. Finally, we come to the question of the causes of all these phenomena, which, if permissible in the case of other animals, is permissible in the animal man. Whatever evidence, whatever chain of reasoning justifies us in concluding that the horse, for example, has come into existence in a certain fashion in time, the same evidence and the same canons of logic justify us to precisely the same extent in drawing the same kind of conclusions with regard to man. And it is the business of the anthropologist to be as severe in his criticism of those matters in respect to the origin of man as it is the business of the palæontologist to be strict in regard to the origin of the horse; but for the scientific man there is neither more nor less reason for dealing critically with the one case than with the other. Whatever evidence is satisfactory in one case is satisfactory in the other; and if any one should travel outside the lines of scientific evidence, and endeavour either to support or oppose conclusions which are based upon distinctly scientific truths, by considerations which are not in any way based upon scientific logic or scientific truth—whether that mode of advocacy was in favour of a given position, or whether it was against it, I, occupying the chair of the Section, should, most undoubtedly, feel myself called upon to call him to order, and to tell him that he was introducing considerations with which we had no concern whatever.

I have occupied your attention for a considerable time; yet there is still one other point respecting which I should like to say a few words, because some very striking reflections arose out of it. The British Association met in Dublin twenty-one years ago, and I have taken the pains to look up what was done in regard of our subject at that period. At that time there was no anthropological department. That study had not yet differentiated itself from zoology, or anatomy, or physiology, so as to claim for itself a distinct place. Moreover, without reverting needlessly to the remarks which I placed before you some time ago, it was a very volcanic subject, and people rather liked to leave it alone. It was not until a long time subsequently that the present organisations of this Section of the Association was brought about; but it is a curious fact, that although proper anthropological subjects were at the time brought before the Geographical Section—with the proper subject of which they had nothing whatever to do—I find that even then more than half of the papers that were brought before that section were, more or less distinctly, of an anthropological cast. It is very curious to observe what that cast was. We had systems of language—we had descriptions of savage races—we had the great question, as it then was thought, of the unity or multiplicity of the human species. These were just touched upon, but there was not an allusion in the whole of the proceedings of the Association at that time to those questions which are now to be regarded as the burning questions of anthropology. The whole tendency in the present direction was given by the publication of a single book, and that not a very large one—namely, "The Origin of Species." It was only subsequent to the publication of the ideas contained in that book that one of the most powerful instruments for the advance of anthropological knowledge—namely, the Anthropological Society of Paris—was founded. Afterwards the Anthropological Institute of this country, and the great Anthropological Society of Berlin came into existence, until it may be said that now there is not a branch of science which is represented by a larger or more active body of workers than the science of anthropology. But the whole of these workers are engaged, more or less intentionally, in providing the data for attacking the ultimate great problem, whether the ideas which Darwin has put forward in regard to the animal world are capable of being applied in the same sense and to the same extent to man. That question, I need not say, is not answered.

It is a vast and difficult question, and one for which a complete answer may possibly be looked for in the next century; but the method of inquiry is understood; and the mode in which the materials are now being accumulated bearing on that inquiry, the processes by which results are now obtained, and the observation of these phenomena leads to the belief that the problem also, some day or other, will be solved. In what sense I cannot tell you. I have my own notion about it, but the question for the future is the attainment, by scientific processes and methods, of the solution of that question. If you ask me what has been done within the last twenty-one years towards this object, or rather towards clearing the ground in the direction of obtaining a solution, I don't know that I could lay my hand upon much of a very definite character—except as to methods of investigation—save in regard to one point. I have some reason to know that about the year 1860, at any rate, there was nothing more volcanic, more shocking, more subversive of everything right and proper, than to put forward the proposition that as far as physical organisation is concerned there is less difference between man and the highest apes than there is between the highest apes and the lowest. Now my memory carries me back sufficiently to remind me that, in 1860, that question was not a pleasant one to touch on. The other day I was reading a recently-published valuable and interesting work, "L'Espèce Humaine," by a very eminent man, M. de Quatrefages. He is a gentleman who has made these questions his special study, and has written a great deal and very well about them. He has always maintained a temperate and fair position, and has been the opponent of evolutionary ideas, so that I turned with some interest to his work as giving me a record of what I could look on as the progress of opinion during the last twenty years. If he has any bias at all it is one in the opposite direction to which my own studies would lead me. I cannot quote his words, for I have not the book with me, but the substance of them is that the proposition which I have just put before you is one, the truth of which no rational person acquainted with the facts could dispute. Such is the difference which twenty years has made in that respect, and speaking in the presence of a great number of anatomists, who are quite able to decide a question of this kind, I believe that the opinion of M. de Quatrefages on the subject is one they will all be prepared to indorse. Well, it is a comfort to have got that much out of the way. The second direction in which I think great progress has been made is with respect to the processes of anthropometry, in other words, in the modes of obtaining those data which are necessary for anthropologists to reason upon. Like all other persons who have to deal with physical science, we confine ourselves to matters which can be ascertained with precision, and nothing is more remarkable than the exactness which has been introduced into the mode of ascertaining the physical qualities of man within the last twenty-five years. One cannot mention the name of Broca without the greatest gratitude; and I am quite sure that when Prof. Flower brings forward his paper on cranial measurements on Monday next you will be surprised to see what precision of method and what accuracy are now introduced, compared with what existed twenty-five years ago, into these methods of determining the physical data of man's structure. If, further, we turn to those physiological matters bearing on anthropology which have been the subject of inquiry within the last score of years, we find that there has been a vast amount of progress. I would refer you to the very remarkable collection of the data of sociology by Mr. Herbert Spencer, which contains a mass of information useful on one side or the other, in getting towards the truth. Then I would refer you to the highly interesting contributions which have been made by Prof. Max Müller and by Mr. Tylor to the natural history of religions, which is one of the most interesting chapters of anthropology. In regard to another very important topic, the development of art and the use of tools and weapons, most remarkable contributions have been made by General Lane Fox, whose museum at Bethnal Green is one of the most extraordinary exemplifications that I know of the ingenuity, and, at the same time, of the stupidity of the human race. Their ingenuity appears in their invention of a given pattern or form of weapon, and their profound stupidity in this, that having done so, they kept in the old grooves, and were thus prevented from getting beyond the primitive type of these objects and of their ornamentation. One of the most singular things in that museum is its exemplification—the wonderful tendency of the human mind when once it has got into a groove to stick there. The great object of scientific investigation is to run counter to that tendency.

Lastly, great progress has been made in the last twenty years in the direction of the discovery of the indications of man in a fossil state. My memory goes back to the time when anybody who broached the notion of the existence of fossil man would have been simply laughed at. It was held to be a canon of palæontology that man could not exist in a fossil state. I don't know why, but it was so; and that fixed idea acted so strongly on men's minds that they shut their eyes to the plainest possible evidence. Within the last twenty years we have an astonishing accumulation of evidence of the existence of man in ages antecedent to those of which we have any historical record. What the actual date of those times was, and what their relation is to our known historical epochs, I don't think anybody is in a position to say. But it is beyond all question that man, and not only man, but what is more to the purpose, intelligent man, existed at times when the whole physical conformation of the country was totally different from that which characterises it now. Whether the evidence we now possess justifies us in going back further, or not—that we can get back as far as the epoch of the drift is, I think, beyond any rational question or doubt; that may be regarded as something settled—but when it comes to a question as to the evidence of tracing back man further than that—and recollect drift is only the scum of the earth's surface—I must confess that to my mind the evidence is of a very dubious character.

Finally, we come to the very interesting question—as to whether, with such evidence of the existence of man in those times as we have before us, it is possible to trace in that brief history any evidence of the gradual modification from a human type somewhat different from that which now exists to that which is met with at present. I must confess that my opinion remains exactly what it was some eighteen years ago when I published a little book which I was very sorry to hear my friend, Prof. Flower, allude to yesterday, because I had hoped that it would have been forgotten amongst the greater scandals of subsequent times. I did there put forward the opinion that what is known as the Neanderthal skull is, of human remains, that which presents the most marked and definite characteristics of a lower type—using the language in the same sense as we would use it in other branches of zoology. I believe it to belong to the lowest form of human being of which we have any knowledge, and we know from the remains accompanying that human being, that as far as all fundamental points of structure were concerned, he was as much a man—could wear boots just as easily—as any of us, so that I think the question remains pretty much where it was. I don't know that there is any reason for doubting that the men who existed at that day were in all essential respects similar to the men who exist now. But I must point out to you that this conviction is by no means inconsistent with the doctrine of evolution. The horse, which existed at that time, was in all essential respects identical with the horse which exists now. But we happen to know that going back further in time the horse presents us with a series of modifications by which it can be traced back from an earlier type. Therefore it must be deemed possible that man is in the same position, although the facts we have before us with respect to him tell in neither one way nor the other. I have now nothing more to do than to thank you for the great kindness and attention with which you have listened to these informal remarks.

SECTION E.

GEOGRAPHY.

Opening Address by the President, Prof. Sir C. Wyville Thomson, F.R.S.

In doing me the honour to select me to preside over this section on the present occasion, the Council of the British Association have doubtless had in view the part which it has been my privilege to take in contributing to the physical description of the earth, as director of the civilian scientific staff on board H.M.S. *Challenger*. I will not, therefore, apologise for following the example of several of my more immediate predecessors in leaving to others the subject of topographical geography which I have never made a special study, and directing your attention for the short time at my disposal to advances which have been made of late years in certain directions in the application of the physical and natural sciences to the illustration of the general condition of the earth.

Before doing so, however, I must refer to the great geographical event of the year which has passed since the geographical section of the British Association last met—the return of the African explorer, Henry Moreland Stanley. As the graphic account which Mr. Stanley has given of his journey "through the dark continent" is in all our hands, and as we may hope to have an opportunity during this meeting of hearing something further of his adventures from the great traveller himself, it will not be necessary for me at present to enter into any details either with regard to the course taken by his expedition or to the brilliant results which it has achieved. It is, however, incumbent upon us in this place to acknowledge once more the flood of light which Stanley has thrown upon the geography of Central Africa, and to express our wondering admiration of the iron will and the daring intrepidity which carried him through these long years of labour and difficulty and danger. Although, in reading Stanley's narrative, we may be forced to regret some of the dark scenes by which his terrible march was chequered, still no one who has not himself had some dealings with savages can fully understand how entirely the action of a leader, solely responsible for the lives of his party, must be guided in every emergency by considerations which he alone is in a position to weigh.

During the last few years a factor, so altered in its proportions that it has appeared almost new, has entered into the calculations of the naval executive departments of all the maritime powers; and in harmony with the rapid advance of natural knowledge and the widening recognition of its practical value, many opportunities, hitherto too often lost, have been taken advantage of. Latterly almost all special expeditions, whether despatched avowedly with the object of extending the boundaries of science, or for hydrographic purposes, or for training naval cadets, or for drilling the inmates of a penitentiary, or pioneering commercial enterprise, as in the case of Capt. Wiggins' late excursions to the mouth of the Yenisei, have been supplied more or less fully with the means of scientific observation, and have been in many cases accompanied by observers trained in one department or another of physical research.

I will simply name among many such equipped and instructed expeditions of these later days, the splendid circumnavigating voyage of the Austrian frigate *Novara*, under the command of Admiral von Wüllerstorf-Urbair. The report of the scientific results of this expedition has been published by the Austrian Government in eighteen beautifully illustrated volumes, and the completion of this work, after seventeen years of heavy labour, was one of the scientific events of the year 1877. The voyage of the Italian corvette *Magenta* round the world, so well chronicled by Prof. Enrico Hillyer Giglioli; the very important sounding voyages of Capt. Belknap, in the American surveying ship *Tuscarora;* the *Hassler* expedition, the last crowning effort with which the elder Agassiz closed a long and brilliant career devoted to the study and illustration of nature, and the many scientific explorations undertaken from year to year by the officers of the American coast survey, with the co-operation of the younger Agassiz and Count Pourtales; the tentative cruises of the British gunboats *Lightning* and *Porcupine*, culminating in the *Challenger* expedition; the scientific voyage round the world of the German frigate *Gazelle;* the several expeditions sent out by different powers to observe the transit of Venus; the German Arctic expedition, under Capt. Koldewey; the several Swedish expeditions, so rich in zoological results, to the Spitzbergen Sea, under the guidance of Otto Torell, Nordenskjöld, and others; the exhaustive researches into the conditions physical and biological of the North Sea, by the North Sea Commission, under the direction of Dr. H. A. Meyer; the voyage of the *Tegethoff*, which Lieut. Payer has rendered ever memorable by his thrilling story of disaster, success, and heroism; the Arctic voyage of the *Alert* and the *Discovery*, of which Sir George Nares has just published the semi-official narrative, a simple and charming account of almost superhuman effort and insuperable obstruction, which it is impossible to read without a feeling of regret that the devoted little band had attempted what was so hopeless, and at the same time a conviction that if their task had been practicable by human skill and bravery, it must certainly have been accomplished. But although this expedition of necessity failed in its main object, that of reaching the Pole, the additional information which we gain from Capt. Nares' volume and from the more popular sketch of the voyage by Capt. Markham on the physical condition of the Arctic Sea is in the highest degree

valuable. I must also mention the very important cruises in connection with the Norwegian Department of Fisheries, which, through the skilled labours of Prof. Mohn and Prof. G. O. Sars, annually contribute largely to our knowledge of the distribution of temperature, of the course of the ocean currents, and of the range of animal life in the North Atlantic. I observe in a letter from Prof. Mohn, dated from Hammerfest on the 10th of last month, that the expedition of the past year has had a successful cruise to Bear Island, where she has left letters for the Dutch Arctic schooner the *Willem Barentz*, and has made many important temperature observations. Prof. Mohn speaks highly of the service rendered by Negretti and Zambra's new reversing thermometer. This is a most ingenious instrument, so constructed that by a simple mechanical arrangement the temperature may be registered at any given depth, irrespective of any number of zones of temperature, higher or lower, through which the instrument may have passed in descending. In the *Challenger* we felt greatly the want of such a thermometer, for although generally throughout the ocean the temperature of the water falls steadily from a surface maximum to a minimum at the bottom, in the Arctic and Antarctic Seas—where a special interest attaches to the vertical distribution of temperatures—the coldest layer is frequently, as in Prof. Mohn's observations, on the surface; and a warmer belt intervenes between it and a bottom-stratum, probably in many cases of intermediate temperature. With the ordinary deep-sea registering thermometer the temperature of the lowest layer cannot be ascertained with certainty. We had Negretti and Zambra's earlier instrument on the reversing principle on board during the latter part of our cruise, but through some defect in construction we did not find its indications trustworthy for great depths. I always believed the plan of construction of this instrument to be good, and I am very glad to find from Prof. Mohn's report that this defect has now been entirely overcome.

It follows from the nature of these many and varied enterprises that the department of geographical science to whose progress they have most specially contributed is the physical geography of the sea; and the special appliances with which they have been provided have been principally instruments for determining the temperature of the water at different depths, the depth of the sea and the nature of the sea-bottom, and, in some few cases, the distribution of the deep-sea fauna. It is of course impossible for me in so short a time even to sketch their several lines of investigation, or to attempt to assign to each its share in the general advance of knowledge; I think it may be better that I should give an outline of some of the conditions of the regions to which they refer by the light of their combined results. I am aware that in taking this course I shall be forced to face questions on which there has been some controversy; and I can only say that I will avoid the controversial aspects of such questions as far as possible, and merely describe as shortly as I can the condition of things as they appear to me.

The General Ocean Circulation.—It was pointed out long ago by Sir Charles Lyell that many of the most marked phenomena of the present physical condition of the globe depend upon the fact that the surface of the world is divided into two hemispheres, one of which contains nearly the whole of the dry land of this world, while the other is almost entirely covered by water. The centre of the land hemisphere is somewhere in Great Britain, and the centre of the water hemisphere, which includes the southern sea, the South Pacific, whatever antarctic land there may be, Australia, and the southern point of South America, is in this neighbourhood of New Zealand. With a full knowledge of the absolute continuity of the ocean we have hitherto been too much in the habit of regarding it as composed of several oceans, each possibly under special physical conditions. All recent observations have, however, shown us that the vast expanse of water which has its centre in the southern hemisphere is the one great ocean of the world, of which the Atlantic with the Arctic Sea and the North Pacific are merely northward extending gulfs; and that any physical phenomena affecting obviously one portion of its area must be regarded as one of an interdependent system of phenomena affecting the ocean as a whole.

Shallow as the stratum of water forming the ocean is—a mere film in proportion to the radius of the earth—it is very definitely split up into two layers, which, so far as all questions concerning ocean movements and the distribution of temperature is concerned, are under very different conditions. At a depth varying in different parts of the world, but averaging perhaps 500 fathoms, we arrive at a layer of water at a temperature of 40° F., and this may be regarded as a kind of neutral band separating the two layers. Above this band the temperature varies greatly over different areas, the isothermobathic lines sometimes tolerably equally distributed, and at other times crowding together towards the surface, while beneath it the temperature almost universally sinks very slowly and with increasing slowness to a minimum at the bottom.

The causes of natural phenomena, such as the movements of great masses of water, or the existence over large areas of abnormal temperature conditions, are always more or less complex, but in almost all cases one cause appears to be so very much the most efficient that in taking a general view all others may be practically disregarded; and speaking in this sense it may be said that the trade-winds and their modifications and counter-currents are the cause of all movements in the stratum of the ocean above the neutral layer. This system of horizontal circulation, although so enormously important in its influences upon the distribution of climate is sufficiently simple. Disregarding minor details, the great equatorial current driven from east to west across the northerly extensions of the ocean by the trade-winds, impinges upon the eastern coasts of the continents. A branch turns northwards and circles round the closed end of the Pacific, tending to curl back to the North American coast from its excess of initial velocity; and in the Atlantic, following a corresponding course, the Gulf Stream bathes the shores of Northern Europe, and a branch of it forces its way into the Arctic basin, and battling against the palæocrystic ice, keeps imperfectly open the water-way by which Nordenskjöld hopes to work his course to Behring's Strait. The southern deflections are practically lost, being to a great extent, though not entirely dissipated in the great westerly current of the southern anti-trades.

One of the most singular results of these later investigations is the establishment of the fact that all the vast mass of water, often upwards of 2,000 fathoms in thickness, below the neutral band, is moving slowly to the northward; that in fact the depths of the Atlantic, the Pacific, and the Indian Oceans are occupied by tongues of the Antarctic Sea, preserving in the main its characteristic temperatures. The maintenance of a low temperature while the temperature of the floor of the ocean must be higher, and that of the upper layers of the sea greatly higher, is in itself a conclusive proof of steady movement of the water from a cold source; and the fact that the temperature of the lower layers of water, both in the Atlantic and the Pacific, is slightly but perceptibly raised to the northward, while the continuity of every layer with a corresponding layer in the southern sea can be clearly traced, indicates the southern position of that source.

The immediate explanation of this very unexpected phenomenon seems simple. For some cause or other, as yet not fully understood, evaporation is greatly in excess of precipitation over the northern portion of the land-hemisphere, while over the water-hemisphere, and particularly over its southern portion, the reverse is the case; thus one part of the general circulation of the ocean is carried on through the atmosphere, the water being raised in vapour in the northern hemisphere, hurried by upper wind currents to the zone of low barometric pressure in the south, where it is precipitated in the form of snow or rain, and welling thence northwards in the deepest channels on account of the high specific gravity dependent on its low temperature, it supplies the place of the water which has been removed.

The cold water wells northwards, but it meets with some obstructions on its way, and these obstructions, while they prove the northward movement, if further proof was needed, bring out another law by which the distribution of ocean temperature is regulated. The deeper water sinks slowly to a minimum at the bottom, so that if we suppose the temperature at a depth of 2,000 fathoms to be 36° F., the temperature at a depth of 3,000 may be, say, 32°. Now, if in this case the slow current meet on its northward path a continuous barrier in the form of a submarine mountain ridge rising to within 2,000 fathoms of the sea-surface, it is clear that all the water below a temperature of 36° will be arrested, and, however deep the basin beyond the ridge may be, the water will maintain a minimum of 36° from a depth of 2,000 fathoms to the bottom. In many parts of the ocean we have most remarkable examples of the effect upon deep-sea temperature of such barriers intersecting cold in-draughts, the most marked instance, perhaps, a singular chain of

closed seas at different temperatures among the islands of the Malay Archipelago; but we have also a striking instance nearer home. Evaporation is greatly in excess of precipitation over the area of the Mediterranean, and consequently, in order to keep up the supply of water to the Mediterranean, there is a constant inward current through the Straits of Gibraltar from the Atlantic; I need not at present refer to an occasional tidal counter-current. The minimum temperature of the Mediterranean is about 54° F. from a depth of 100 fathoms to the bottom. The temperature of 54° is reached in the Atlantic at the mouth of the Straits of Gibraltar at a depth of about 100 fathoms, so that in all probability future soundings will show that the free water-way through the Straits does not greatly exceed 100 fathoms in depth.

The Depth of the Sea, and the Nature of Modern Deposits.—It seems now to be thoroughly established by lines of trustworthy soundings which have been run in all directions, that the average depth of the ocean is a little over 2,000 fathoms, and that in all probability it nowhere exceeds 5,000 fathoms. Depths beyond 4,000 fathoms are rare and very local, and seem to be usually pits in the neighbourhood of volcanic islands. In all the ocean basins there are depressions extending over considerable areas where the depth reaches 3,000 fathoms or a little more, and these depressions maintain a certain parallelism with the axes of the neighbouring continents.

Within 300 or 400 miles of the shore, whether in deep or in shallow water, formations are being laid down, whose materials are derived mainly from the disintegration of shore rocks, and which consequently depend for their structure and composition upon the nature and composition of the rocks which supply their materials. These deposits imbed the hard parts of the animals living on their area of deposition, and they correspond in every way with sedimentary formations with which we are familiar, of every age. In water of medium depths down to about 2,000 fathoms, we have in most seas a deposit of the now well-known globigerina-ooze, formed almost entirely of the shells of foraminifera living on the sea-surface, and which after death have sunk to the bottom. This formation, which occupies a large part of the bed of the Atlantic and a considerable part of that of the Pacific and Southern Seas, is very like chalk in most respects, although we are now satisfied that it is being laid down as a rule in deeper water than the chalk of the cretaceous period.

In depths beyond 2,500 or 3,000 fathoms no such accumulations are taking place. The shores of continents are usually too distant to supply land detritus, and although the chalk-building foraminifera are as abundant on the surface as they are elsewhere, not a shell reaches the bottom; the carbonate of lime is entirely dissolved by the carbonic acid contained in the water during the long descent of the shells from the surface. It therefore becomes a matter of very great interest to determine what processes are going on, and what kind of formations are being laid down in these abyssal regions, which must at present occupy an area of not less than ten millions of square miles.

The tube of the sounding instrument comes up from such abysses filled with an extremely fine reddish clay, in great part amorphous, but containing, when examined under the microscope, a quantity of distinctly recognisable particles, organic and inorganic. The organic particles are chiefly siliceous, and for the most part the shells or spines of radiolarians which are living abundantly on the surface of the sea, and apparently in more or less abundance at all depths. The inorganic particles are minute flakes of disintegrated pumice, and small crystalline fragments of volcanic minerals; the amorphous residue is probably principally due to the decomposition of volcanic products, and partly to the ultimate inorganic residue of decomposed organisms. There is ample evidence that this abyssal deposit is taking place with extreme slowness. Over its whole area, and more particularly in the deep water of the Pacific, the dredge or trawl brings up in large numbers nodules very irregular in shape, consisting chiefly of peroxide of iron and peroxide of manganese, deposited in concentric layers in a matrix of clay, round a nucleus formed of a shark's tooth, or a piece of bone, or an otolith, or a piece of siliceous sponge, or more frequently a fragment of pumice. These nodules are evidently formed in the clay, and the formation of the larger ones and the segregation of their material must have taken a very long time. Many of the sharks' teeth to which I have alluded as forming the nuclei of the nodules, and which are frequently brought up uncoated with foreign matter, belong to species which we have every reason to believe to be extinct. Some teeth of a species of *Carcharodon* are of enormous size, four inches across the base, and are scarcely distinguishable from the huge teeth from the tertiary beds of Malta. It is evident that these semi-fossil teeth, from their being caught up in numbers by the loaded line of the trawl, are covered by only a very thin layer of clay.

Another element in the red clay has caused great speculation and interest. If a magnet be drawn through a quantity of the fine clay well diffused in water, it will be found to have caught on its surface some very minute magnetic spherules, some apparently of metallic iron in a passive state, and some of metallic nickel. From the appearance of these particles, and from the circumstance that such magnetic dust has been already detected in the sediment of snow-water, my colleague Mr. Murray has a very strong opinion that they are of cosmic origin—excessively minute meteorites. They certainly resemble very closely the fine granules which frequently roughen the surface of the characteristic skin of meteorites, and from their composition and the circumstances under which they are found there is much to be said in favour of this view. I cannot, however, hold it entirely proved; there can be little doubt, from the universal presence of water-logged and partially decomposed pumice on the bottom, and from the constant occurrence of particles of volcanic minerals in the clay, that the red clay is formed in a great measure by the decomposition of the lighter products of submarine volcanoes drifted about by currents, and finally becoming saturated with water and sinking; and it is well known that both iron and nickel in a metallic state are frequently present in minute quantities in igneous rocks. I think it is conceivable that the metallic spherules may be derived from this source.

So far as we can judge, after a most careful comparative examination, the deposit which is at present being formed at extreme depths in the ocean does not correspond either in structure or in chemical composition with any known geological formation; and, moreover, we are inclined to believe, from a consideration of their structure and of their imbedded organic remains, that none of the older formations were laid down at nearly so great depths—that, in fact, none of these have anything of an abyssal character. These late researches tend to show that during past geological changes abyssal beds have never been exposed, and it seems highly probable that until comparatively recent geological periods such beds have not been formed.

It appears now to be a very generally received opinion among geologists—an opinion which was first brought into prominence by Prof. Dana—that the "massive" eruptions which originated the mountain chains which form the skeleton of our present continents, and the depressions occupied by our present seas date from the secular cooling and contraction of the crust of the earth from a period much more remote than the deposition of the earliest of the fossiliferous rocks, and that during the period chronicled by the successive sedimentary systems, with many minor oscillations by which limited areas have been alternately elevated and depressed, the broad result has been the growth by successive steps of the original mountain chains and the extension of the continents by their denudation, and the corresponding deepening of the original grooves. If this view be correct—and it certainly appears to me that the reasoning in its favour is very cogent—it is quite possible that until comparatively recent times no part of the ocean was sufficiently deep for the formation of a characteristic abyssal deposit.

Time will not allow me even to allude to the interesting results which have been obtained from the determination of the density of sea water from different localities and different depths, and from the analysis of sea water and its contained gases, and perhaps these results have been scarcely sufficiently worked out as yet to afford safe bases for generalisation. I must, however, say a few words as to certain additions which have been made to our knowledge of the two hitherto impregnable strongholds of the frost, the regions round the North and South Poles.

The Arctic Regions.—The question which has of late held the most prominent place in all discussions about the conditions of the Arctic Regions, particularly since the voyage of Dr. Hayes, is whether it is possible that there can be at all times or at any time anything in the form of an open Polar sea. This question seems now to be virtually settled, and in the most unsatisfactory manner imaginable. There can be no doubt that in the year 1871 Count Wilczek, in the schooner *Isbjörn*, found the sea between Novaya Zemlya and Spitzbergen nearly free from ice, and that the same sea presented to Weyprecht and Payer in the following year a dangerous stretch of moving and impenetrable

pack. There can be no doubt that in the year 1861 Dr. Hayes gazed over an expanse of open water where, in 1875-76, Capt. Nares studied the conditions of palæocrystic ice. It is evident, therefore, that the Polar basin, or at all events such portions of it as have been hitherto reached, is neither open sea nor continuous ice, but a fatal compromise between the two, an enormously heavy pack formed by the piling up and crushing together of the floe of successive years, in frequent movement, breaking up and shifting according to the prevailing direction of the wind, and leaving open, now here and now there, lanes and vistas of deceptive open water which may be at any moment closed and converted into a chaotic mass of hurtling floe-bergs by a hurricane from another direction. It seems, however, that in certain seasons there is more open water in the direction of Grinnell's Land and Smith's Sound than in others, and that there are also years comparatively favourable for the northward route following the lead of Franz-Josef Land; and there seem now to be only two plans, one nearly as hopeless as the other, to choose between in any future attempt—either to establish several permanent Polar stations, as proposed by Lieut. Weyprecht, and already initiated at one point, so far as preliminaries are concerned, by Capt. Tyson and Capt. Howgate, and to seize the opportunity of running north in early autumn from the station where the sea appears most open, or to run as far north as possible at enormous expense, with a great force of men and abundance of provisions and paraffin oil, and push northwards during the arctic winter by a chain of communicating stations with ice-built refuge huts. It seems possible that in a cold season, with the pack in the condition in which Markham found it in 1876, some progress might be made in this way if it were conceivable that the end to be gained was worth the expenditure of so much labour and treasure.

The Antarctic Regions.—But little progress has been made during the last quarter of a century in the actual investigation of the conditions of that vast region which lies within the parallel of 70° S. Some additional knowledge has been acquired, and the light which recent inquiries have thrown upon the general plan of ocean circulation and the physical properties of ice, have given a new direction to what must partake for some time to come of the nature of speculation.

From information derived from all sources up to the present time, it may be gathered that the unpenetrated area of about 4,700,000 square miles surrounding the South Pole is by no means certainly a continuous "Antarctic Continent," but that it consists much more probably partly of comparatively low continental land, and partly of a congeries of continental (not oceanic) islands, bridged between and combined, and covered to the depth of about 1,400 feet, by a continuous ice-cap; with here and there somewhat elevated continental chains, such as the groups of land between 55° and 95° W., including Peter the Great Island and Alexander Land, discovered by Billingshausen in 1821, Graham Land and Adelaide Island, discovered by Biscol in 1832, and Louis Philippe Land by D'Urville in 1838, and at least one majestic modern volcanic range discovered by Ross in 1841 and 1842, stretching from Balleny Island to a latitude of 78° S., and rising to a height of 15,000 feet. It seems, so far as is at present known, that the whole of the antarctic land, low and high, as well as the ice-cap of which a portion of the continuous continent may consist, is bordered to some distance by a fringe of ice, which is bounded to seaward by a perpendicular ice-cliff, averaging 230 feet in height above the sea-level. Outside the cliff a *floe*, which attains near the barrier a thickness of about 20 feet, and in some places by piling a considerably greater thickness, extends northwards in winter to a distance varying according to its position with reference to the southward trending branches of the equatorial current; and this floe is replaced in summer by a heavy drifting pack with scattered ice-bergs. Navigating the Antarctic Sea in the southern summer, the only season when such navigation is possible, it has been the opinion of almost all explorers, that after forcing a passage through an outer belt of heavy pack and ice-bergs, moving as a rule to the north-westward, and thus fanning out from the ice-cliff in obedience to the prevailing south-easterly winds, a band of comparatively clear water is to be found within.

Several considerations appear to me to be in favour of the view that the area round the South Pole is broken up and not continuous land. For example, if we look at a general ice-chart we find that the sea is comparatively free from icebergs, and that the deepest notches occur in the "Antarctic Continent" at three points, each a little to the eastward of south of one of the great land masses. Opposite each of these notches a branch of the equatorial current is deflected southwards by the land, and is almost merged in the great drift-current which sweeps round the world in the Southern Sea before the westerly anti-trades. But while the greater portion of the Brazilian current, the East Australian current, and the southern part of the Agulhas current are thus merged, they are not entirely lost; for at these points of junction with the drift-current of the westerlies, the isobathytherms are slightly deflected to the southwards, and it is opposite these points of junction that we have comparatively open sea and penetrable notches in the southern pack. But we have not only the presumed *effect* of this transfer of warmer water to the southwards; we were able to detect its presence in the *Challenger* by the thermometer. Referring to the result of a serial temperature sounding on February 14, 1874, with a surface temperature of 29° F. at a depth of from 300 to 400 fathoms, there is a band of water at a temperature of more than half a degree above the freezing-point. That this comparatively warm water is coming from the north there is ample proof. We traced its continuity with a band at the same depth gradually increasing in warmth to the northward, and it is evident that its heat can be derived from no other source, and that it must be continually receiving new supplies, for it is overlaid by a band of colder water, tending to mix with it by convection.

It is, of course, possible that these warm currents may by coincidence be directed towards those notches already existing in a continental mass of land; but such a coincidence would be remarkable, and there is certainly a suggestion of the alternative that the "continent" may consist to so great an extent of ice as to be liable to have its outline affected by warm currents.

In high southern latitudes it seems that all the icebergs are originally tabular, the surface perfectly level and parallel with the surface of the sea, a cliff about 230 feet high bounding the berg. The top is covered with a layer of the whitest snow; now and then a small flock of petrels take up their quarters upon it, and trample and soil some few square yards, but after their departure one of the frequent snow showers restores it in a few minutes to its virgin whiteness. The upper part of the cliff is pale blue, which gradually deepens towards the base. When looked at closely the face of the cliff is seen to be traversed by a delicate ruling of faint blue lines, the lines being more distant from one another above and becoming gradually closer. The distance between the well-marked lines near the top of a berg may be of a foot or even more, while near the surface of the water it is not more than two or three inches, and the space between the blue lines have lost their dead whiteness and have become hyaline or bluish. The blue lines are very unequal in their strength and in their depth of colouring; sometimes a group of very dark lines gives a marked character to a part of a berg. Between the stronger blue lines near the top of the cliff a system of closer lines may be observed, marking the division of the ice by still finer planes of lamination; but in the narrower spaces near the water-line they are blended and lost. The blue lines are the sections of sheets of clear ice; the white intervening bands are the sections of layers of ice where the particles are not in such close contact—ice probably containing some air.

The stratification in all these icebergs is, I believe, originally horizontal and conformable, or very nearly so. In many, while melting and beating about in the sea, the strata become inclined at various angles, or vertical or even reversed; in many they are traversed by faults, or twisted, or contorted, or displaced; but I believe that all deviations from a horizontal arrangement are due to changes taking place in the icebergs themselves.

I think there can be no doubt, from their shape and form, and their remarkable uniformity of character, that these great table-topped icebergs are prismatic blocks riven from the edge of the great antarctic ice-sheet. I conclude, therefore, that the upper part of the iceberg, including by far the greater part of its bulk, and culminating in the portion exposed above the surface of the sea, was formed by the piling up of successive layers of snow during the period, amounting perhaps to centuries, during which the ice-cap was slowly forcing its way over the low land, and out to sea over a long extent of gentle slope, until it reached a depth considerably beyond 200 fathoms, when the lower specific weight of the ice caused an upward strain which at length overcame the cohesion of the mass, and portions were rent off and floated away. The icebergs when they are first dispersed float in from 200 to 250 fathoms; when, therefore, they have been

drifted to latitudes of 65° or 64° south, the bottom of the berg, the surface which forced itself glacier-like over the land, just reaches the layer at which the temperature of the water distinctly rises; and is rapidly melted, and the pebbles and land *débris* with which it is more or less charged are precipitated. That this precipitation takes place all over the area where the icebergs are breaking up, constantly and to a considerable extent, is evident from the fact that the matter brought up by the sounding instrument and the dredge is almost entirely composed of such deposits from ice; for diatoms, foraminifera, and radiolarians are present on the surface in large numbers, and unless the deposit from the ice were abundant it would soon be covered and masked by the skeletons of surface organisms.

The curious question now arises, what is the cause of the uniform height of the southern icebergs—that is to say, what is the cause of the restriction of the thickness of the free edge of the ice-cap to 1,400 fathoms? I have mentioned the gradual diminution in thickness of the strata of ice in a berg from above downwards. The regularity of this diminution leaves it almost without a doubt that the layers observed are in the same category, and that therefore the diminution is due to subsequent pressure or other action upon a series of beds, which were at the time of their deposition nearly equally thick. About 60 or 80 feet from the top of an iceberg, the strata of ice a foot or so in thickness, although of a white colour and thus indicating that they contain a considerable quantity of air, are very hard, and the specific weight of the ice is not much lower than that of layers three inches thick nearer the water-line of the berg. The upper layers have been manifestly produced by falls of snow after the berg has been detached.

Now it seems to me that the reduction in thickness cannot be due to compression alone, but that a portion of the substance of the lower layers must have been removed. It is not easy to see why the temperature of the earth's crust, under a widely extended and practically permanent ice-sheet of great thickness, should ever fall below the freezing-point; and it is a matter of observation that at all seasons of the year vast rivers of muddy water flow into the frozen sea from beneath the great glaciers which are the issues of the ice-sheet of Greenland. Ice is a very bad conductor, so that the cold of winter cannot penetrate to any great depth into the mass. The normal temperature of the surface of the earth's crust, at any point where it is uninfluenced by cyclical changes, is at all events above the freezing-point, so that the temperature of the floor of the ice-sheet would certainly have no tendency and fall below that of the stream passing over it. The pressure upon the deeper beds of the ice must be enormous at the bottom of an ice-sheet 1,400 feet in thickness—not much less than a quarter of a ton on the square inch. It seems, therefore, probable that under the pressure to which the body of ice is subjected a constant system of melting and regelation is taking place, the water passing down by gravitation from layer to layer until it reaches the floor of the ice-sheet, and finally working out channels for itself between the ice and the land, whether the latter be subærial or submerged.

I should think it probable that this process, or some modification of it may be the provision by which the indefinite accumulation of ice over the antarctic continent is prevented and a certain uniformity in the thickness of the ice-sheet maintained—that in fact ice at the temperature at which it is in contact with the surface of the earth's crust within the antarctic regions cannot support a column of itself more than 1,400 feet high without melting. It is suggested to me by Prof. Tait that the thickness of the ice-sheet very probably depends upon its area, as the amount of melting through squeezing and the earth's internal heat, will depend upon the facility of the escape of the water. The problem is, however, an exceedingly complex one, and we have perhaps scarcely sufficient data for working it out.

The Fauna of the Deep Sea.—I can scarcely regret that it is utterly impossible for me on this occasion to enter into any details with regard to the relations of the abyssal fauna, the department of the subject which has naturally had for me the greatest interest. Recent investigations have shown that there is no depth limit to the distribution of any group of gill-bearing marine animals. Fishes, which, from their structure and from what we know of the habits of their congeners, must certainly live on the bottom, have come up from all depths, and at all depths the whole of the marine invertebrate classes are more or less fully represented. The abyssal fauna is of a somewhat special character, differing from the fauna of shallower water in the relative proportions in which the different invertebrate types are represented. It is very uniform over an enormously extended area, and in this respect it fully confirms the anticipations of the great Scandinavian naturalist Lovén, communicated to this Association in the year 1844. It is a rich fauna, including many special genera and an enormous number of special species, of which we, of course, know as yet only a fraction; but I do not think I am going too far in saying that from the results of the *Challenger* expedition alone the number of known species in certain classes will be doubled. The relations of the abyssal fauna to the faunæ of the older tertiary and the newer mesozoic periods are much closer than are those of the faunæ of shallow water; I must admit, however, that these relations are not so close as I expected them to be—that hitherto we have found living only a very few representatives of groups which had been supposed to be extinct. I feel, however, that until the zoological results of several of these later voyages, and especially those of the *Challenger*, shall have been fully worked out, it would be premature to commit myself to any generalisations.

I have thus attempted to give a brief outline of certain defensible general conclusions, based upon the results of recent research. Some years ago, certain commercial enterprises, involving the laying of telegraph cables over the bed of the sea, proved that the extreme depths of the ocean were not inaccessible. This somewhat unexpected experience soon resulted in many attempts, on the part of those interested in the extension of the boundaries of knowledge, to use what machinery they then possessed to determine the condition of the hitherto unknown region. This first step was naturally followed by a development of all appliances and methods bearing upon the special line of research; and within the last decade the advance of knowledge of all matters bearing upon the physical geography of the sea has been confusingly rapid—so much so, that at this moment the accumulation of new material has far outstripped the power of combining and digesting and methodising it. This difficulty is greatly increased by the extreme complexity of the questions, both physical and geological, which have arisen. Steady progress is, however, being made in both directions, and I trust that in a few years our ideas as to the condition of the depth of the sea may be as definite as they are with regard to regions to which we have long had ready access.

SECTION G.

MECHANICAL SCIENCE.

OPENING ADDRESS BY THE PRESIDENT, EDWARD EASTON, C.E.

On the Conservancy of Rivers and Streams.

By the conservancy of rivers and streams I mean the treatment and regulation of all the water that falls on these islands from its first arrival in the shape of rain and dew to its final disappearance in the ocean.

I had at first, in my ignorance, contemplated treating the subject in a still wider manner by referring to the rivers and streams of other countries; but I soon found that the vast extent of the field to be traversed would make it extremely unlikely that I could, with any satisfactory result, attempt the more restricted task which I have now before me.

The question of the conservancy of rivers and streams involves the consideration of their regulation for the following principal purposes:—

1st. For the supply of pure and wholesome water for the domestic and sanitary wants of the population.

2nd. For the supply of water of proper quality and sufficient quantity for industrial purposes.

3rd. For the proper development of water power.

4th. For the drainage and irrigation of land.

5th. For navigation and commerce.

6th. For the preservation of fish.

Until the appointment last year of the Select Committee presided over by the Duke of Richmond, no attempt, as far as I am aware, has been made to grapple with the question as a whole, and the Report made by them omitted to deal with, at least, two of the objects I have indicated as being necessary to the proper dealing with the subject.

The recommendations made in the Report of that Committee were most important, and will, if carried out, remove many of the difficulties which stand in the way of a complete system of conservancy of our rivers.

So much has been written on the engineering details of this subject, by men far better qualified than I am to deal with them, that I shall confine myself to the simple statement of the principles which have been recognised by the chief authorities as essential, and to a few suggestions, which my own experience leads me to think may be of some value. Almost all the great engineers of former generations, who have paid attention to this question, Smeaton, Telford, Rennie, Golborne, Mylne, Walker, Rendel, Stephenson, Jessop, Chapman, Beardmore, and without mentioning names, many of the most eminent now living, have agreed to the following general propositions:—

That the freer the admission of the tidal water, the better adapted is the river for all purposes, whether of navigation, drainage, or fisheries.

That its sectional area and inclination should be made to suit the required carrying power of the river through its entire length, both for the ordinary flow of the water, and for floods.

That the downward flow of the upland water should be equalised as much as possible throughout the entire year; and

That all abnormal contaminations should be removed from the streams.

In carrying out these principles, it is perhaps superfluous to say, that modifications must be introduced to suit the particular phenomena of each river. In some watershed areas it would be easy to construct reservoirs, which would to a great extent equalize the flow and reduce floods. In others, it might be better to control the floods by means of embankments. In others, to have weirs, and sluices, delivering into side channels, parallel to the main stream, with the same object. Sometimes reservoirs, or receptacles, must be made for catching the *débris* brought down by the streams. In fact, every river must be treated as a separate entity. It is therefore necessary that a systematic collection of data relating to rainfall, the geological character of the gathering ground, and the volume of each separate stream, should be made for each watershed area; and this should be carried on for a sufficient length of time to enable a fairly correct estimate to be formed of the behaviour of the river both in time of flood and in time of drought. The establishment of self-acting, tide-registering gauges at several points of every outfall should be insisted on. By these means the whole of the phenomena of a watershed area could be ascertained and recorded, and safe and trustworthy knowledge could be obtained, which would contribute towards the determination, not only of the works which ought to be executed, but of the incidence of the taxation by which the necessary funds should be raised. For instance, it is obvious that where the geological character of a watershed is variable, one portion of it consisting of a permeable stratum, such as chalk or red sandstone, and another portion of an impervious stratum, such as the tertiary clays or the shales of the millstone grit, the same works would not be adapted to each section of the river, nor would it be fair to charge all with the expense according to the same scale of contribution. The former, that is the permeable stratum, is not only, from its absorbent nature, not the cause of floods, but is, by reason of that characteristic, absolutely constituted by nature one of the very works which must be devised by art to mitigate the effects of rainfall on the latter, or impervious stratum.

Bearing this in mind, I have often thought that nature might be usefully imitated in this operation, by passing the surplus rainfall into the permeable strata of the earth by means of wells, or shafts, sunk through the impermeable strata overlying them. This has been done in isolated cases for the drainage of lands, but not for the deliberate purpose of preventing floods and equalizing the flow of rivers.

I also wish to remark that artificial compensating reservoirs may be much more frequently made use of than is generally supposed to be possible, when it is considered that, so long as the dams are constructed in situations where there is no danger of their giving way, it is by no means necessary that they should be watertight, and that, therefore, they can be constructed at a very much smaller outlay. In fact, the purpose would be answered by a series of open weirs, which would collect the water in times of flood and discharge it gradually down the stream.

The example of our French neighbours in the more general use they make of movable weirs—*barrages*—of various constructions could, I am satisfied, be followed by us with very great advantage in many cases.

The question of water power is one which I think deserves more consideration than it has lately received. It has been the fashion to consider that small watermills are of little or no value, and, in the present state of most rivers and streams, this is to a very great extent true, but only because the supply of water to work them is so variable and uncertain. Sufficient attention has never yet been given to the subject of the amount of compensation water which should be given for the use of riparian proprietors when the watershed areas are dealt with for purposes of water supply. There is a kind of empirical rule acknowledged by most of the eminent water engineers, that one-third of the average flow of three consecutive dry years is a fair equivalent for the abstraction of the water falling on a gathering ground. I am strongly of opinion that, looking to imperial interests, advantage should be taken of every opportunity of dealing with a gathering ground to provide for a much larger proportion of its available water being sent down the streams, so that the natural water power of the country may be properly developed. The extra cost of the necessary works must, as a matter of course, be borne rateably by the interests benefited. It is certain that with the progress of invention many more ways of utilising this power will be discovered. At present, through the medium of compressed air, of hydraulic pressure, and of electro-motors, the great disadvantage of its being only available at the spot where the water runs is overcome, and the power can be transmitted to any distance, and used wherever it may be most conveniently applied.

Sir Robert Kane, in his most valuable and exhaustive work on the "Industrial Resources of Ireland," has given an estimate of the value of the power allowed to escape every year in the shape of floods, and the same calculation might be applied to the sister kingdom. It is probably no exaggeration to say that where running streams exist the power required for estate purposes, on the majority of properties in the United Kingdom, might be obtained by a proper conservation of the natural water resources of those streams.

The consideration I have been able to give to this subject has helped to convince me that, although a vast amount of labour and research has been devoted to it, it is nevertheless one in which "a more systematic direction to scientific inquiry" is urgently needed.

A vast collection of scientific facts exists, but they require arrangement and collation, and future observations should be more strictly classified, so that the bearing of each one, both on the others and on the subject at large, may be properly appreciated with a view to a practical result.

In France this is being done to a very large extent, and an excellent map showing the phenomena of the rivers and streams of that country is now in course of preparation. For many years also very accurate observations of the phenomena of the whole of the basin of the Seine have been taken, and have been centralised (*centralisées*) by that eminent engineer, whose loss all who had the privilege of knowing him, either in his work or in private intercourse, are deploring, M. Belgrand, late Inspector-General of the Ponts et Chaussées, and by his able coadjutor, M. M. G. Lemoine. These observations have been published in the form of diagrams, admirable in their simplicity of design, which show at a glance the bearing of every one of those phenomena on the general character of that river.

In Italy also, where there exists a distinct department having control of the hydraulic works of that country, the same exhaustive system of collation and record has been followed, and the results have been published in a series of tables. In Germany, although the same complete system is not in vogue, its chief river has been the subject of most thorough investigation, the results of which have been published in a beautiful map of the Rhine and its regulating works.

In our own country, as might be expected from the number of engineering works which have been executed, there probably exists an amount of detailed information on special and often minute points which is unsurpassed, and probably unequalled in the world.

But, although as I have said before, a great number of eminent men have treated in an exhaustive manner the phenomena relating to many of the principal rivers of Great Britain and Ireland, yet, as far as I am aware, there has been no attempt to collect and combine these most valuable, though detached fragments of knowledge, so that their relation to one another might be seen, and a general conclusion arrived at. This can only be done by the establishment of a public department analogous to those described as already existing in France and Italy.

When it is considered that many lives are annually sacrificed,

either directly by the action of floods, or by the indirect but no less fatal influence of imperfect drainage—when it is remembered that a heavy flood, such as that of last year, or that of the summer of 1875, entailed a monetary loss of several millions sterling in the three kingdoms—that during every year a quantity of water flows to waste, representing an available motive power worth certainly not less than some hundreds of thousands of pounds,—that there is a constant annual expenditure of enormous amounts for removing *débris* from navigable channels, the accumulation of which could be mainly, if not entirely prevented,—that the supply of food to our rapidly growing population, dependent, as it is at present, upon sources outside the country, would be enormously increased by an adequate protection of the fisheries,—that the same supply would be further greatly increased by the extra production of the land when increased facilities for drainage are afforded,—that, above all, the problem of our national water supply, to which public attention has of late been drawn by H.R.H. the Prince of Wales, requires for its solution investigations of the widest possible nature, I believe it will be allowed that the question, as a whole, of the management of rivers is of sufficient importance to make it worthy of being dealt with by new laws to be framed in its exclusive behalf.

I do not wish it to be understood that in suggesting the collection of additional data relating to the phenomena of rivers, I am advocating delay in dealing with the existing state of things until the facts have all been ascertained. On the contrary, I believe that the first step ought to be the establishment of a distinct Water Department, which should at once address itself to the remedying of the evils which are found to be most pressing. The time has long since arrived when the present neglected state of many of our most important streams should be dealt with, and that this was also the conviction of Parliament and of the government is evident, from the appointment of such an influential Committee as that presided over by the Duke of Richmond last session.

A new department should be created—one not only endowed with powers analagous to those of the Local Government Board, but charged with the duty of collecting and digesting for use all the facts and knowledge necessary for a due comprehension and satisfactory dealing with every river, basin, or watershed area in the United Kingdom—a department which should be presided over, if not by a Cabinet Minister, at all events by a member of the government who can be appealed to in Parliament.

The department should have entire charge of, and control over, all estuaries and navigable channels, both because these are used by foreign vessels, and therefore the responsibilities attaching to their preservation are international, and because they must be protected from hostile attack, and on these accounts are essentially imperial property. For the same reason the cost of amending and maintaining them should be defrayed out of the imperial exchequer.

As regards the regulation of the remainder of the watershed area, the conclusions arrived at in the Report of the Duke of Richmond's Select Committee seem to me entirely satisfactory. I cannot do better than give a few extracts from that Report. The Committee say—"That in order to secure uniformity and completeness of action each catchment area should, as a general rule, be placed under a single body of conservators, who should be responsible for maintaining the river from its source to its outfall in an efficient state. With regard, however, to tributary streams, the care of these might be entrusted to district committees acting under the general direction of the conservators, but near the point of junction with the principal stream they should be under the direct management of the conservators of the main channel, who should be a representative body constituted of residents and owners of property within the whole area of the watershed. The Committee go on to say that "means should be taken to ensure the appointment of a conservancy board for each watershed area," but that application should first be made by persons interested in the district, and that then the departmental authorities should send inspectors to make local inquiries and to report upon the "necessities and capacities of the district, and suggest the area and proportions of taxation."

With regard to what is probably the most important point of all, the finding of the money necessary to carry out these recommendations, the Committee advocate the introduction of a new principle of taxation, the soundness of which cannot be questioned. Instead of the principle first introduced by the statute of Henry the Eighth, and observed ever since, of levying taxes in proportion to the direct benefit conferred, the Committee propose that the rates should be distributed over the whole area of a watershed, including not only the lands, but the towns and houses and all other property situate within that area. This is in fact no more than a general application of the law of highways, which in the time of the Romans, according to Justinian, applied equally to waterways. It is perfectly just that every acre, the drainage of which contributes to the flow of the streams and rivers of every watershed area, should, in some proportion or other, contribute also to the cost of maintaining the channels of those streams and rivers in an efficient state. The incidence of the taxation must of course, as has been pointed out, be determined by the circumstances of each particular case, but there is no doubt that the conclusion of the Duke of Richmond's Committee, that "the taxation should be levied on the basis of rateable value," is the only sound, and at the same time practical, way of dealing with this difficulty.

The word "taxation" is not, I fear, generally connected with any idea of profit to the individual taxpayer. But in this case, as I hope in the course of this address I have made clear, the prevention of large present losses, and the advantages gained by an improved system, will give not only a fair but an ample return on the capital expended.

It is my firm belief that an intelligent management of watershed areas would be compatible with an absolute profit to every interest affected; that we have here no question of give and take, but that in this, as in every other case, the laws of nature, under proper and scientific regulation, can be made subservient to the needs of the highest civilisation.

THE PHONOGRAPH AND VOWEL SOUNDS[1]

III.

WE now pass to the general conclusions which may be drawn from our experiments. In the first place it seems clear that vowels do not depend on pitch alone or on the simple grouping of partial tones independently of absolute pitch. Before the constituents of a vowel can be assigned, the pitch of the prime must be named. But on the other hand the pitch of the most prominent partial of the group is not alone sufficient to allow us to name the vowel in which it appears; to do this we also require to be told the relation of the constituent partials to one another.

The sound $\bar{u}$ consists mainly of one tone generally lying in the region above *a*.

The sound $\bar{o}$ requires at least two partials; when there are only two important ones these lie in the region between *g* and f'', a region covering nearly two octaves. Indeed the upper limit may extend above f'' with a tenor or woman's voice. Other partials than the prime are reinforced by the mouth-cavity over all this region. This great range is obviously a distinguishing mark of $\bar{o}$ as compared with $\bar{u}$, perhaps *the* distinguishing mark; for when $\bar{u}$ and $\bar{o}$ are sung at various pitches, the most prominent partial, first of one letter and then of the other, is highest, and the most prominent partial of both sounds may lie on $b'\flat$, the characteristic tone of $\bar{o}$. An $\bar{u}$ sung on $b\flat$ may even have the tone $b'\flat$ more strongly present in it than an $\bar{o}$ of the same pitch. When $\bar{u}$ was sung by voice 1 on $b\flat$ the prominent partial was the second; when $\bar{o}$ was sung at the same pitch by the same voice, the second partial was still the most prominent. Thus for voice 1 the chief distinction between $\bar{u}$ and $\bar{o}$ on this note lay not in the pitch of strongest reinforcement, but in the fact that the prime was larger for $\bar{o}$ than for $\bar{u}$. When voice 5 sang $\bar{u}$ on $b\flat$ the prominent partial was the prime; when it sang $\bar{o}$ the prominent partial was the second, the prime being also strong. Thus, for voice 1, the distinction lay in the fact that the prime was much smaller for $\bar{o}$ than for $\bar{u}$. It is obvious here that the ear cannot have been guided by the absolute pitch of the reinforcement to the distinction between $\bar{o}$ and $\bar{u}$. At this pitch the distinction lies in the fact that $\bar{o}$ contains two strong partials (the prime and second), whereas $\bar{u}$ contains only one (the prime *or* the second). The argument is not weakened by saying that the $\bar{u}$ of one voice was not the same as the $\bar{u}$ of the other. Identically the same it cannot have been; nevertheless, on higher or lower notes the two voices agreed as to the composition of $\bar{u}$, and generically the vowels were certainly the

[1] Continued from p. 397.

same. There remains the fact that speakers and hearers were unconscious of any generic change in the vowel ū when the pitch of the strongly-reinforced partial changed by a whole octave.

On the other hand, it is equally clear the voice, in singing a given vowel at various pitches, does not simply produce a certain constant group of relative partial tones. Possibly, indeed, the ear might recognise a single tone, especially if very feebly accompanied by higher harmonics, as a kind of ū outside the region within which the human voice forms ū in that way. Thus Helmholtz, in his *Tonempfindungen*, says that the single tone B♭, when sounded alone, gave a very dull ū, much duller than could be produced by the voice. This tone is an octave below the place where voice I ceased to make ū by reinforcing the prime. Quite similarly it is conceivable that the group consisting of a prime and its octave might be recognised as ō even when produced below the limits within which the human voice does produce this simple harmony in singing ō. Our own impression as to the result of running the phonograph slower when it is speaking than when it is spoken to, supports this view, but we do not desire to base any inference on that. Certainly the low ō produced in this manner is not the human ō.

Moreover, we find a very decided resemblance in the relative constituents of ō at a low pitch and a° or ā at higher pitches. ō in the neighbourhood of B♭, and ā in the neighbourhood of *f* and *g*, are pretty similarly constituted. Our experiments on a° and ā are not sufficiently extended to allow any very general conclusions to be drawn, but they are sufficient to show that between certain vowels the main distinction must lie in the absolute pitch of the reinforced group of partial tones.

We are thus brought back to our original statement that in distinguishing vowels the ear is aided by two factors, one depending on the harmony or group of partials, and the other on the absolute pitch of the constituents. It seems not a little singular that the ear should attribute a distinct unity to sounds so dissimilar in their relative and absolute composition as those represented by the curves of Fig. 1.

We are forced to the conclusion already adopted by Helmholtz and Donders that the ear recognises the kind of cavity by which the reinforcement is produced; that although the sounds which issue differ so much that we fail when they are graphically represented and mathematically analysed to grasp any one prominent common feature, nevertheless by long practice the ear is able to distinguish between the different sorts of cavities which are formed in pronouncing given vowels. Something of the same kind may be observed with other sources of sound than the human voice; the resonating cavities of various musical instruments aid greatly in allowing each particular species to be recognised at once, though their effect must be widely different at different parts of the scale. It is, moreover, no mere inference that we recognise the cavity. Prof. Crum Brown's gutta percha bottle, described before, proves that we do, and that it is a group of tones reinforced by a particular kind of cavity that we call a particular vowel. But we have to consider what light the experiments throw on the kinds of cavity that are required for certain vowels. The cavities are clearly distinguished in virtue of two distinct properties: first, the pitch of their maximum resonance or strongest proper tone, and second the range of reinforcement which they are capable of producing. This latter property has, we believe, been hitherto much neglected.

Prof. Crum Brown's bottle proves that a *constant* cavity is capable of producing the constant vowel ō over a large range of pitch. On the other hand our experiments with various human voices singing ō appear to exhibit a *tuning* of the cavity by which new partials are sometimes introduced somewhat abruptly; and for the sound ū it seems certain that the cavity is tuned, that is to say, that the pitch of its proper tone is not the same when the vowel is sung on different pitches. The appreciably strong *fourth* partial in all the duplex ū's of Table VI. may here be noticed as favouring the view that in each of these examples the oral cavity had been adjusted so as to be in unison with the second partial. We may describe the ū cavity as an adjustable cavity with a very limited range of resonance, whose effect is to reinforce strongly only one partial lying above *a*. It is possible that this cavity may keep itself constant throughout the very limited range of pitch employed in ordinary speech, but when the range is increased as in singing, a certain tuning seems indispensable.

If we assume that the ō cavity is absolutely constant, we must describe it as a cavity capable of reinforcing more or less strongly tones lying anywhere between *g* and f''. This cavity

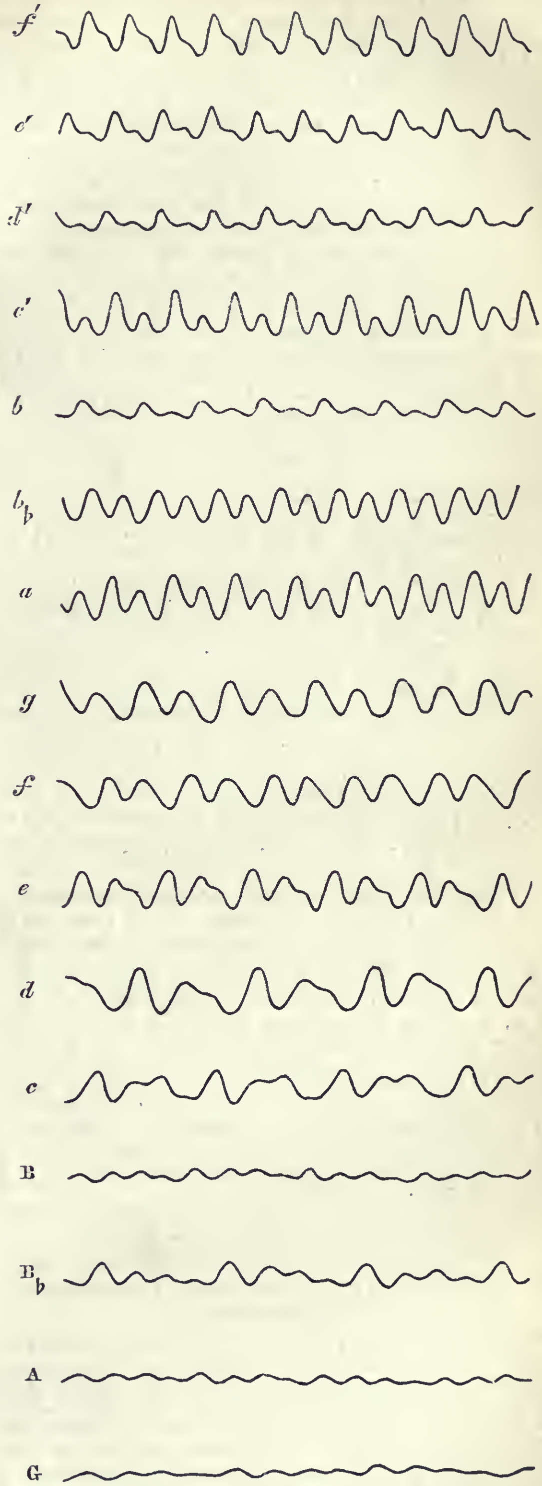


FIG. 1.[1]—*Wave-forms of ō Sung by the same Voice at Various Pitches.*

[1] We reproduce this week the figure which accompanied Profs. Jenkin and Ewing's first paper, on "The Wave-Form of Ō," p. 342. It shows the delicate forms of the curves with greater exactness, and will enable the reader to understand more clearly the value of the conclusions come to by the authors.—ED.

has, in the case of all the human voices investigated, one strong proper tone in $b'\flat$, but as the cavity which produced the artificial $\bar{o}$'s did not possess this characteristic, and it was possessed by different voices in very different degrees, it should perhaps be regarded as an accident of the human voice rather than as essential to the production of the vowel. It is difficult, however, on the constant cavity hypothesis, to see how the third partial of an $\bar{o}$ sung on or near a should not be stronger than it is. We are disposed to regard it as more probable that the $\bar{o}$ cavity also is tuned. We do not mean by this that it has a proper tone always in unison with one of the partials of the note sung, but only that there is a tendency to accommodate the pitch of the cavity so that it shall reinforce some partial more strongly than might have been the case without this tuning. On this view the range of reinforcement of each $\bar{o}$ cavity need not extend over much more than an octave, nor the range of adjustment over so much as six semitones. The upper reinforcement would lie round about $b'\flat$, on which note the greatest reinforcement can be given. Another way of putting the same conclusion would be to say that the generic character of the human $\bar{o}$ was given by the fact that the range of reinforcement of any $\bar{o}$ cavity extends over rather more than an octave, with an upper and strongest proper tone never far from $b'\flat$, but sometimes deviating slightly from that pitch on account of the voice choosing its cavity so as to bring a strong proper tone more closely into unison with one of the upper harmonics of the note sung.

We should describe the $\bar{a}$ cavity as differing from that for $\bar{o}$ chiefly in having a higher general pitch of resonance, and perhaps, also, a wider range.

It is evident that Willis and Wheatstone were right in considering that the vowel quality was given by a particular resonator, and that the pitch of maximum resonance of the resonator was an important element in determining the vowel character of the sounds produced. Willis's vowels were not thoroughly recognisable because the form and material of his resonator were not adapted to include the second element of range of reinforcement.

Further, our experiments agree with the observation of Donders that there is a pitch of maximum resonance in human mouth cavities for the vowel $\bar{o}$, although, as we have said above, we are disposed to consider the $\bar{o}$ cavity as not quite constant. We fail to distinguish any such characteristic tone in the case of $\bar{u}$, and we observe that it is fixed by Helmholtz only with considerable diffidence.

Our experiments entirely confirm Helmholtz's statement that vowel sounds are made up of harmonic partial tones, and the groups of partials, so far as he gives them, for the vowels we have investigated, agree fairly well with our results. Since these experiments were brought to a close our attention has been directed to a paper by Felix Auerbach (*Pogg. Ann. Ergänzung*, viii. 2), containing an account of experiments on vowel sounds made by him in Prof. Helmholtz's laboratory. By the aid of resonators applied to the ear he made numerical estimates of the strength of the several partial tones when vowels were sung on various notes. He was led, as we have been, to the conclusion that the relative partials were an important factor in the result as well as the absolute pitch, but we cannot say that our numbers agree with his estimates or support the deductions which he has drawn from them.

FLEEMING JENKIN
J. A. EWING

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

THE directors of the Polytechnic are about to make an important alteration in the science department of this institution. It is intended to separate the laboratory from the rest of the institution, and to establish a high-class school of practical science in all its branches. Dr. Edward B. Aveling has been appointed to the charge of this laboratory, and intends forthwith to establish classes for practical instruction in all the science subjects required for the university, government, and other examinations.

THE first series of 500 select public teachers in France arrived in Paris on August 15 to visit the Exhibition, at the expense of the government. They are accompanied by 1,000 teachers of the same districts travelling at their own expense, but conveyed at half price by the various railway companies, and boarded in several Paris colleges. They were received at the Sorbonne, in the large hall, by M. Casimir-Périer, the Sub-Minister of Public Instruction, and lectured by M. Levasseur on the teaching of geography. They will be lectured on the teaching of French, of history, and on the organisation of lectures and public libraries. They will leave on Friday, and be succeeded by another set of teachers.

M. BARDOUX has issued a circular intimating that a special financial department has been created for facilitating the building of school-houses in the several French communes. A credit of 60,000,000 francs has been voted by the Chambers, and will be divided amongst the several municipalities that desire to improve or rebuild their public schools, on the condition that each should expend a sum of at least double that taken from the public exchequer.

PROF. A. WOLTMANN, of Prague, has accepted a call to the directorship of the Archæological Institute at Strasburg.

THE first experiment of an educational turn for children, about which we spoke some months ago, has given such good results that a new society is in process of formation at St. Petersburg for a similar purpose. Several eminent teachers of the Russian capital have offered their services to the society, which will yearly send out companies of children on educational travel, as well as parties of young ladies and young men who have finished their studies in secondary schools, or are following the courses of high schools.

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, August 12.—M. Fizeau, president.—On the composition of the milk of the cow-tree (*Brosmium galactodendron*), by M. Boussingault. He finds that while the general constitution of the juice of the cow-tree approaches that of milk, the proportions of the various substances are very different.—Observation on the discovery announced by Mr. L. Smith of a new earth belonging to the cerium group, by M. C. Marignac. He does not see any reason for distinguishing the supposed new earth from terbene.—Studies on the placenta of the Aï (*Bradypus tridactylus*, Lin.); the place which that animal should occupy in the series of mammals, by M. N. Joly.—On the fundamental co-variants of a cubo-biquadratic binary system, by Prof. Sylvester.—New process for the analysis of milk, yielding rapidly butter, lactose, and caseine, in one and the same specimen, by M. A. Adam.—M. J. Vinot sent to the Academy a letter addressed to him by Leverrier in September, 1876, in which the late astronomer inferred, from various observations, that there are two intra-Mercurial planets.—On the functions of leaves; function of the stomata in the exhalation and inhalation of aqueous vapours by leaves, by M. Merget.—On the delay of the pulse in intra-thoracic aneurisms and in aortic insufficiency, by M. Fr. Franck.—Chemical researches on the division of cyclamine into glucose and mannite, by M. S. de Luca.—On parasitic isopods of the genus Entoniscus, by M. Alf. Giard.—On the changes of colour of *Nika edulis*, by M. S. Jourdain.—Importance of the partition of vegetable cells in the phenomena of nutrition, by M. Max. Cornu.—On the part of stipules in inflorescence and in the flower, by M. D. Clos.—On the fall of avalanches, by M. Ch. Dufour.

CONTENTS

THURSDAY, AUGUST 29, 1878

THE ECLIPSE

THE telegram sent from Denver, signed by Professors Young and Watson, Dr. Draper, and myself, will have given an idea of the results which we think have been secured on the eclipse of this year. Since the telegram was despatched I have been engaged in making as many notes on the various points of detail as incessant travel and a temperature of 91° in the shade would permit. A long time must elapse before anything like a general view of the total work done is possible, but I think that the readers of NATURE may rely upon the correctness of what I have collected, though it is quite possible, as I have not succeeded in finding all the observers, that some errors may have crept in. And again, it is quite possible that the many dry plates taken and even yet not developed, may contain information of which at present we have no idea. To take an instance: we do not yet know whether Prof. Harkness's attempt to photograph the polariscopic phenomena presented by the corona was successful or not, for I learn at the Naval Observatory here that he is still at Fort Steele, handing over his camp equipment to the military, who all along the line have been placed in the most unreserved way at the disposal of the different parties by the express orders of Gen. Sherman himself, who takes the greatest interest in such inquiries, and is anxious to foster scientific inquiries as far as in him lies. Strange as it may seem, this is the expressed feeling of all the authorities here, from the Chief of the State downwards. In interviews with which I have been honoured, the President of the United States himself, the Secretary for War, Gen. Sherman, and other members of the Cabinet, have one and all insisted upon the importance of securing records of all possible natural phenomena, and expressed their gratification that such records have been secured in the present instance by Government aid.

I will begin the extracts from my notes by referring to the appearance of the Corona itself.

I give a rough sketch of what *I* saw of the corona with the naked eye (Fig. 1), slightly exaggerating the dimensions of the streamers to show the wind-vane appearance, which, to me, was almost perfect, being pointed at one end and bounded by parallel edges at the other; others, I may say, however, saw a resemblance to a fish's tail. These streamers vanished absolutely in the telescope (Fig. 2), as did the radiating lines in 1871; not a shred of them was left. Prof. Cleveland Abbe (lying on his back at a height of eleven feet on Pike's Peak; he had been sent down the day before from the summit, as the rarefaction of the air was too much for him) saw them, with the naked eye, extending to a distance of six degrees on either side, and their appearance suggested to him meteoric streams for their origin. Prof. Newcomb also saw them with the naked eye when he had hidden the corona behind a screen. To him they suggested the zodiacal light, or rather its nucleus near the sun, even if it extend beyond the orbit of the earth.

These streamers seem to have been seen by everybody, and were doubtless cosmical; a system at right angles to them (they lay along the ecliptic), and quite as bright, was recorded by many, though many of the best observers saw not a trace of them.

Here is a lesson, and one which affords an explanation of a great deal of eclipse work, connected with these outliers of the corona.

I had a magnificent view of the corona with a power of 50 on my 3¾-inch Cooke, and saw exquisite structure at the north and south points. Curves of contrary flexure started thence, and turned over, and blended with the rest of the corona, which was entirely structureless and cloud-like; the filamentous tracery which in India I observed till three minutes after totality had ended had all gone. Prof. Bass, however, tells me that by confining his attention to the same point for nearly the whole of totality, the structure came out, and seemed to pulsate like an aurora.

Prof. Hall is almost the only one who is under the impression that the corona of 1869 was less brilliant than this.

Mr. Burnham, who is an observer of the highest order, thus gives his opinion, which agrees with that of Prof. Young, who remarked its unusual faintness and lack of polar extension, and all the other American astronomers:—

"The coronal display was far less than in the eclipse of 1869, as seen at Des Moines, Iowa, by members of the *Times* party, and it bore a more striking resemblance, probably, to the eclipse of 1870, as seen at Gibraltar and in the Island of Sicily. The corona was, in fact, a mild affair, according to the observations of this party, as compared with that seen in other eclipses. A few protuberances were seen, and several bright streamers. On one side there was observed a bright pink ribbon or crescent. The traditional bright lines (the rays) and dark patches (the rifts) were not nearly as conspicuous as usual."

Mr. Burnham made another interesting observation which may prove one of considerable value in determining the nature of the corona. Anticipating that the star ζ Cancri would be very near to the sun, he made special efforts to see it, and was altogether successful, for it was distinctly seen through the corona.

General Myer, the distinguished head of the Army Signal Service, who had given orders to utilise his station on Pike's Peak for eclipse observations, observed the corona himself from the summit, and therefore, under conditions which have never been utilised before. He describes the corona as built up of five radial lines of a golden colour; beyond this in the direction of the ecliptic were prolonged bright silver rays. This was seen with the naked eye. In the telescope the appearance was quite different; a layer close to the sun, only of a light pink colour, was seen, and the long bright silver rays had disappeared. The greater elevation, thus, was more suited to a study of the structure than the lower levels, and at the same time the colour observed seems to have been slightly changed. In the pure air of the Peak, also, he saw the corona steadily for about five minutes after totality, and watched the moon cover the outer striated edge of the corona, which appeared, then, to be more golden than ever.

We next come to the Photographs.

Photographs of the corona and of its spectrum were

obtained at nearly all the stations. In order to give an idea of the extensive preparations which have been made, I may state what were Dr. Draper's photographic appliances as an instance of the equipment at one station.

In the first place there was a telescope of 5 inches aperture, and 78 inches focal length, especially corrected for photography, to get as complete a photograph of the corona as could be obtained by an exposure lasting

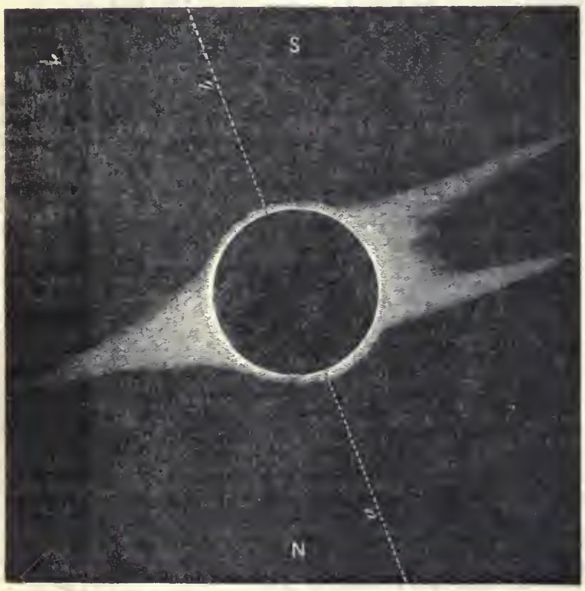


FIG. 1.—Naked eye.

during the whole of totality. This was during 165 seconds.

Next there was a large instrument which may be called a slitless "phototelespectroscope." This instrument consisted of an object-glass, composed of four lenses 6 inches in diameter and 21 inches focal length. The image of the sun at the focus was less than $\frac{1}{4}$ of an inch in diameter, and of extreme brilliancy. Before the rays of this lens reached the focus, however, they were intercepted by a Rutherfurd grating, about 2 inches square,

FIG. 2.—Telescope.

set at an angle of 60 degrees. This threw the rays to one side, and produced three images—a central one of the sun, and on either side of it a spectrum. One of these spectra was dispersed twice as much as the other, that is, would give a photograph of twice the length. This last photograph was actually about 2 inches long. With this instrument, mounted equatorially and driven by clockwork, if the light of the corona was due to gas, giving lines which lay in the actinic region of the spectrum, ring-formed images, one ring for each bright line, would be introduced. On the other hand, if the light of the corona arose from solid or liquid bodies, or was reflected light from the sun, a long band answering to the actinic region of the spectrum alone would be produced. If the light was partly from gas and partly reflected sunlight, a result partly of rings and partly a band would have been obtained.

As there was an impression that it was impossible to obtain a photograph of the spectrum of the corona, Dr. Draper, in order to give the best possible chance of

FIG. 3.—1, image of sun; 2, first order spectrum; 3, second order.

getting such a photograph, resorted to exceedingly sensitive materials, known as the lightning collodion process, furnished by the Messrs. Anthony, of New York. This involved a necessity of distilling a large part of the water used, because at Rawlins the water contains either alkali or sulphur, both of which are deleterious.

Since so many attempts were made, I had better confine my attention to the photographs. I have seen those of Prof. Hall and Dr. Draper. I will first deal with those of the corona itself. The former, taken by a dry-plate process, and, I believe, by means of a large apertured camera of short focus, are very admirable, and

FIG. 4.

show the structure at the north and south points most exquisitely.

One of Dr. Draper's was exposed during the whole of totality, and represents a corona somewhat like those seen in the photographs of the 1871 eclipse taken with a medium exposure. In another, by a fortunate shake of the instrument, a trace of the outline of the chromosphere was received.

Next I come to the Spectra; but first I may be permitted to refer to the only photographic contrivance which was possible for my light marching order. This was a small portrait camera with a lens of one inch aperture,

FIG. 5.

and a Rutherfurd grating of about 6,000 lines to the inch in front of it to act as a reflector and disperser.

I placed the camera and the attached grating on the ground in such a position that on the focusing screen I got the direct image of the sun at one edge of the plate, and running across the plate the spectra of the first and second orders on one side. A rough notion of what is seen on the plate when the sun is photographed thus will be gathered from Fig. 3.

Now if the corona had been built up of gases competent to give us lines with a spectroscope of ordinary construction, what I ought to get during an eclipse from

this instrument would have been something like Fig. 4, *provided I had a clockwork arrangement to drive it.* Without this contrivance the image and spectra will travel along the plate in the direction of the sun's motion—Let us suppose as here in Fig. 5.

It will be thus clear that, with this simple contrivance, the result obtained, if clock-work be employed, will be very similar in kind to that arrived at in Dr. Draper's more elaborate arrangement, to which attention has been drawn.

In the spectra, then, thus obtained, we had only one question to solve, as we all thought: we had to determine the position of the rings photographed in the first and second order spectra, or, at all events, in the first. Long before I developed my plate I knew what had happened. Dr. Draper, Prof. Thorpe, and myself, had photographed the spectrum—this I never doubted—but we had photographed as continuous a one as if a platinum wire, or ball, in a state of incandescence, had been in front of our instrument, instead of a shell of hydrogen gas 6′ high, as in 1869, and 10′ high, as in 1871.

Dr. Draper's plate was exposed during the whole of totality, to make assurance doubly sure. The continuous spectrum has no trace of a ring, though the plate was considerably over-exposed. In my own little photograph the continuous spectrum only is shown, and very clearly. While my plate was being exposed I held a duplicate grating close to the eye, expecting to see the rings as I saw them through five prisms in 1871. In first, second, and third order it was all the same—

Continuous spectrum,
Continuous spectrum,
Continuous spectrum ;

and it was this which told me of the gigantic change which had taken place in the atmosphere of the great body we were studying.

The remarkable phenomenon thus photographed was of course as remarked by the eye observers ; still bright lines were seen in the corona spectrum by one observer, using the term corona to represent everything outside the photosphere, and I make this remark because, whether the spectrum observed was that of the phenomenon which was visible or not I cannot say.

Prof. Young himself and many other observers saw the reversal of many lines at the moment of totality, thereby endorsing Pye and Maclear's observations of 1870 and 1871. After this, to all but Prof. Young the lines vanished utterly, and this was so absolute that in three parties there was consternation and fear, when nothing but a continuous spectra was seen, that something had gone wrong with the instruments.

Prof. Young saw 1474 very faint, *F without C*, and strangest of all, the H and K lines without the long calcium line. This is one of the most remarkable results of the eclipse, and must set students of solar physics to work on a new line. Young thinks the H and K lines were brilliant enough to suggest that the ring in this part of the spectrum photographed in the eclipse of 1875 was really due to them, and I am inclined to agree with him, though why the H and K lines should appear minus the longest line of calcium, passes comprehension, unless calcium is dissociated there from some cause of which at present we have no idea.

The discovery of a line in the ultra red by means of a thermopile, is another victory gained, and I sincerely hope that at the next eclipse Capt. Abney may use some "red molecules" to photograph it. Mr. Edison, with his wonderful tasimeter, also found indications of heat in the corona, but as he did not use a spectrum, he did not imitate Prof. Young in determining the exact position of the radiation.

We next come to the Dark Lines observed in the corona spectrum.

Janssen was the first to announce the fact that in the corona spectrum the chief Fraunhofer lines were to be seen. This observation was made on the Indian eclipse of 1871. The natural interpretation of this observation is that in the coronal atmosphere there are molecules sufficiently complex to reflect rays of every refrangibility, and that they do in reality reflect the solar light to us as it reaches them.

The observations on this point are a little doubtful, though it is held generally that the balance of evidence is in favour of their appearance. Prof. Barker, who observed with Dr. Draper at Rawlins, saw these lines at the moment that he was dumbfounded, as were most of the observers, at the absence of the bright ones. On the other hand at Prof. Newcomb's camp at Separation they were not traced, Commander Sampson seeing nothing but a perfectly continuous spectrum ; 1474, F, and even C being utterly invisible. If the corona gives us light containing the Fraunhofer lines, it can only do so because it reflects solar light. Let us next see, then, what the evidence as to Polarisation is.

When all the observations on this subject are collected we may hope for much new knowledge, but the matter was not finally settled this time, for here again was a surprise.

Professors Morton and Hastings had arranged to determine the quality of the polarisation observed, Prof. Harkness was to attempt to photograph the phenomena, while Prof. Wright set himself the difficult task of quantifying it. One observer was specially told off to settle the questions raised by Prof. Pickering's observations of 1870. I may commence by saying that these last observations suggest that in 1870 the instrument used by Prof. Pickering was out of order, so that now everybody agrees that there is polarisation.

I have not yet been able to learn anything of the results obtained by Professors Wright and Harkness as the latter used dry plates which were to be developed here, and the Las Animas party took a long mountain trip to shake off the bad effects of a long camping on an alcali plain, and so did not put in an appearance at Denver.

Prof. Morton got radial polarisation most distinctly as I did in India in 1871. The *amari aliquid* is to be found in Prof. Hastings' results, which will be best seen from the following account obligingly placed at my disposal by Prof. Bass, one of his party :—

"The most important and unexpected result of the expedition was reached by Dr. Hastings in his polariscopic observations. It will be remembered that in previous expeditions, and by many observers in the present eclipse, only a few seconds were devoted to the examina-

tion of the corona for polarisation, and this by a bi-quartz or a Savart polariscope, held in the hand so as to take in the whole corona in one view. In the organisation of the expedition, Dr. Hastings was requested to prepare a plan by which the question as to whether the polarisation of the corona was radial or tangential, or absent, could be definitively settled. This was accomplished by an arrangement of apparatus essentially novel.

"The four-inch telescope had a Savart polariscope at its eye end. A diaphragm perforated with holes, 3′, 4′, 5′, 8′, 10′, 16′ was interposed between the polariscope and the objective, and the polarisation phenomena of a definite and limited portion of the corona were thus seen. Diaphragms of 3′ and 5′ were alone used. The telescope was pointed by Lieut. Very to six points, the first one above the sun and 50′ from the circumference; the second one 25′ from the circumference and 45° from the vertical; the third $1\frac{1}{2}'$ from the sun and to the right of it; the fourth 16′ from the sun and to the right of it; the fifth 16′ from the sun and 135° from the vertical; the sixth 8′ from the sun and 135° from the vertical. These points were selected before the eclipse, but it was not known to Dr. Hastings in what order they would be viewed, so that the readings were absolutely free from bias. The position of the pointer of the polariscope was carefully verified before and after the totality. The readings were reduced immediately, and each reading (which was, as is evident, independent of every other) agreed in showing that the plane of polarisation was perpendicular to the sun's radius through the point examined—that is, *tangential*—a most unexpected result, about which, however, there is no doubt whatever. Four of the readings were made with a dark band in the centre, and two with a bright band. This result does not agree with others from the same eclipse, for the reason that in the method adopted by Prof. Morton, Mr. Lockyer, and others, there was, first, no means adopted for isolating a definite portion of the corona and determining its special polarisation. Second, that in the use of the Savart form, the phenomena of radial or tangential polarisation alike present bands radial to the sun. Third, that, with the bi-quartz, the field is so small that it becomes extremely difficult to interpret the tints of colour seen, if indeed any are seen. The novel fact was shown, by the use of this method, that the polarisation of the corona was exceedingly strong near to the sun's limb (one and a half minutes), and was relatively weak far from it."

I believe that no one is more astonished than Prof. Hastings himself at the result of his work, which, it has been laughingly said, goes to demonstrate the existence *ice crystals* in the corona. With reference to the remarks made on my 1871 work in the foregoing, I may add that I used a bi-quartz and a large field, so that the objections raised to the method do not apply.

This brings me to the end of my notes for the present on the photographic, spectroscopic, and polariscopic results obtained. Of course there was a whole world of wonders outside these fields of inquiry.

The view of the shadow of the moon from the summit of Pike's Peak cleaving its way along the lower air has been described to me as one of the most striking phenomena which it is possible to witness or imagine. The shadow, the boundary of which was seen curved, was preceded and followed by a spectrum. Where I was, nearly 8,000 feet high, and therefore a little over mid-height, there was no effect on the air, but after the close of totality the shadow was observed passing over clouds near the horizon. I may add, however, that the phenomena at the beginning and end of totality hardly existed for me, for I had to uncover and cover my photographic plates at those times.

It was not to be expected that in this country, where the anxiety for news and views seems always to be at fever heat, the astronomers would be allowed to quit their stations without giving an idea of the tendency of their work, and even its connection with the torrid temperature through which we have had to make our dusty way, or in which we have had to exist when locomotion had become impossible.

The utter disappearance of the large bright red corona of former years in favour of a smaller and white one in this year of minimum, struck everybody. Indeed it is a remarkable thing that after all our past study of eclipses this last one should have exhibited phenomena the least anticipated. It isolates the matter that gives us the continuous spectrum from the other known gaseous constituents. The present eclipse has accomplished, if nothing else, the excellent result of intensifying our knowledge concerning the running down of the solar energy. With the reduction of the number of spots or prominences for the last four years the terrestrial magnetism has been less energetic than it has been for the preceding forty years, while at both ends of this period we have had famines in India and China.

As the sun is the great prime mover of earth, and as every cloud, every air current depends upon it, its present quiet condition is worthy of the most minute study. The absence of lines from the corona spectrum shows a great reduction in the temperature of the sun, and such a marked change in the sun should produce a corresponding change on the earth, so that the associated terrestrial phenomena should be carefully observed. Hence I regard this eclipse as the most important that has been observed for many years as it throws much needed light on many points hitherto obscured in doubt.

Prof. Morton, of the Stevens Institute of Technology, remarked that the thing which impressed him most was the very curious character of the result; while on former occasions there has been projected into the space surrounding the sun a quantity of self-luminous gaseous matter which has no fixed place there, we now see that this was merely a temporary occupant which has, either by diffusion into space or absorption into the body of the sun, been removed. He also holds that the marked changes in the sun's condition would seem to call for corresponding marked changes in the condition of the earth. The results, in fact, recall to his mind one of the early theories of the maintenance of the sun's heat, which was that it was maintained by the impact of meteoric matter constantly falling in upon the sun from surrounding space. The quantity of heat produced by such an infall may be realised from the fact that a body so falling would develop by impact as much heat as 5,000 times its weight of carbon burnt in oxygen. Portions of the meteoric matter in the shape of meteorites fall upon the earth, and we thus know that most of them consist largely of iron, containing considerable quantities of absorbed gases; others are of a metallic character and devoid of such gases. If, now, we suppose that for a considerable period the sun's fires were fed with hydrogen-charged faggots, and then again that the main supply was of mineral matter, we might have at first a far-reaching atmosphere

of gas, such as has appeared hitherto, and at another time this might be absent, which seems to be the case at present.

He further adds that if such changes go on indefinitely it may not be irrational to inquire whether they may not in future produce such extraordinary climatic conditions in the earth as geology teaches us have existed in the ages of the past.

Prof. Young was careful not to commit himself to any decided connection between solar and terrestrial climatic changes; he, however, certainly concurs with me that the corona is fainter and the gaseous elements far less conspicuous than that observed at previous eclipses, and acknowledges that the different condition of the corona proves a change in the condition of the sun, as the corona acts with it in a sort of sympathy. Dr. Draper is resolute on the other side. He is reported to have said:—

"It is rather singular while the sun has been in such a quiescent condition for more than two years, that we have not seen more changes in the climate of the earth. This would seem to show that the abnormal condition of the sun at the maximum period of sun-spots, which occurs every eleven years, counts for but little against the total amount of heat that is sent out from the sun at all times. The present observations go to show that the activity or quiescence of the sun makes no perceptible difference in the earth's condition. I do not regard this most marked change in the corona as portending any change in the condition of either climate or crops."

Finally, on this whole question, I may remark that I have been not a little astonished to find how slowly European work percolates among the men of science here. I have met with few who are familiar with Meldrum's admirable work, and the discussion to which it has given rise. Still it is a great thing that at all events the cycle of solar changes has forced itself so markedly upon public attention.

For myself, as I have ever regarded sun-spots as

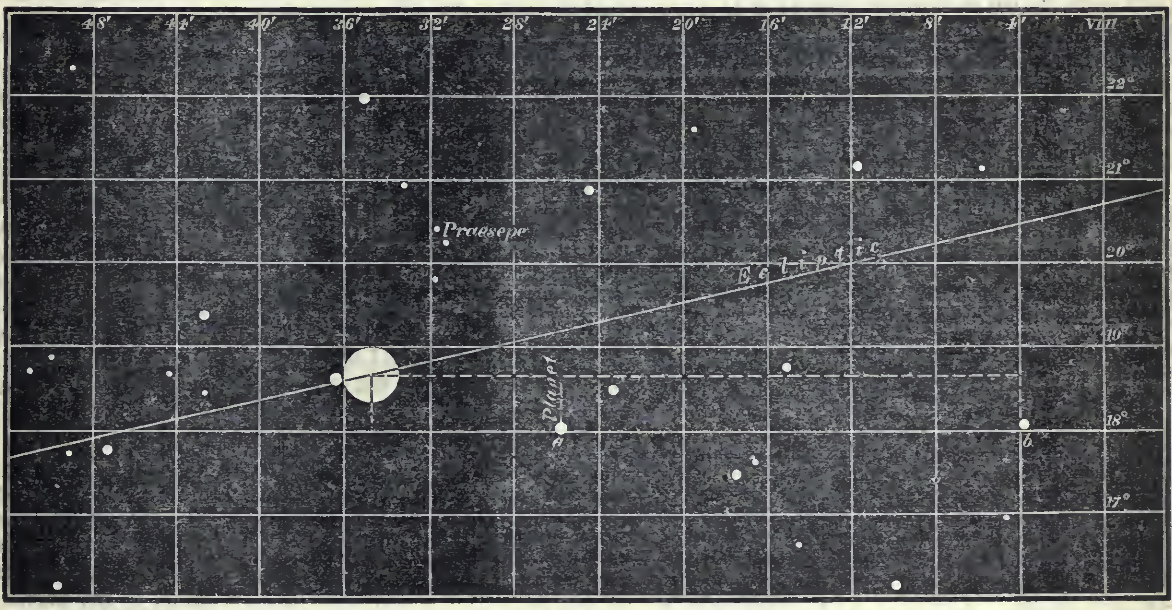


FIG. 9.

down-rushes—a term to which I still adhere—I am well content to see this view indorsed by such a chain of facts as the corona has now supplied. In spot-maximum years we have violent up-rushes of gas from the sun's interior, and the corona is mainly built up of such gas. Further, we have spots, and, if these are not evidences of the return convection currents, we have none other. In spot-minimum years, such as the present, we have no up-rushes, and the corona contains no gas, and there are no spots. Spots, then, are only observed when we have a right to look for the return of the upward current, about which there is no doubt, and the rate of which we have measured.

But if this puts beyond all question, as I hold it does, the nature of spots, on the other hand, the separation of the gaseous from the continuous spectrum of the corona indicates that we have yet much to learn of the temperature and nature of the corona when the spots are absent.

So much, then, touching the progress of solar matters during the eclipse of 1878. I have not yet, however, done with the observations.

There is little doubt, I think, that an intra-Mercuria planet has been found by Prof. Watson. If it will fit one of Leverrier's orbits, and should turn out to be Vulcan, no doubt astronomers will be able to keep a firm grasp upon it, and sooner or later its elements will be determined.

Prof. Watson, of Ann Arbor, whose belt, as the papers here put it, is graced with the scalps of I know not how many minor planets, broke off work on a planet beyond Neptune to come to discover one inside Mercury. He went with me in Mr. Silvis' railway photographic car from Rawlins to Separation on the morning of the eclipse, intending to observe with me at the station we were determined to occupy, with our light equipments, as the number of detached clouds visible at the time of totality on the pre-

vious days had strongly shown the advantage of separating the parties as much as possible. We chose a spot to leeward of one of the enormous water tanks of the Union Pacific Railway, which form the chief features in the interesting but desolate plains in that region, over which the wind sweeps at times with incredible violence.

On reaching our destination we found Prof. Newcomb, whose camp was about a mile away, and it was then agreed that as both he and Prof. Watson were to hunt for the planet they had better be together, so I lost his company during the eclipse.

Prof. Watson's plan of operation was to sweep south of the sun and observe all the stars in the map, a part of which is here reproduced (Fig. 6), and to refer the position of any new body to the stars, or, if possible, to the sun itself. For this purpose, with the assistance of the Rawlins carpenter, he armed his equatorial with paper circles and brass wire pointers. He commenced operations to the left of the sun and saw the stars marked, but none others. Then sweeping out to the star marked *b* he noticed on his return another not on the chart, marked *a*. He then made three marks on his right ascension paper circle, on the spots occupied by the pointer, when the sun, *a* and *b*, were successively brought into the centre of the field. He next determined the difference of declination in the same way between the sun and *a*, having the additional help that *a* was nearly in the same declination as *b*. He then repeated his R.A. measures, and called Prof. Newcomb, but the eclipse was over before anything more could be done. I give this statement from memory only (as I was too busy to make notes at the time), as I heard it soon after the eclipse at the camp, before the telescope was dismounted. It is probable that subsequent careful measures of the circles may alter the place—

R.A. 8h. 26m.
Dec. + 18° 00′

I telegraphed to you, somewhat, but the alteration will be small.[1]

Since arriving here I have learned that Mr. Swift, of Rochester, a well tried observer, also saw the planet. The first account I read of his work was as follows:—

"This gentleman made a very careful search for Vulcan, scanning the heavens very closely with his splendid comet eye-piece, made by the celebrated Gundlach, but he saw nothing of it. He did, however, see, about three degrees from the sun, two stars not down in the charts or star maps, and about as bright as the pole star—they were pointing directly towards the sun. On attempting to re-find them, he was prevented by a little cloud."

Since then, however, another fuller account of his work has appeared, from which I gather that about one minute after the commencement of totality two stars caught his (Mr. Swift's) eye about three degrees, by estimation, south-west of the sun. He saw them twice, and attempted a third observation, but a small cloud obscured the locality. The stars were both of the fifth magnitude, and but one is on the chart of the heavens. This star he recognised as Theta in Cancer. The two stars were about eight minutes apart. There is no such configuration of stars in the constellation of Cancer. In 1859 the French astronomer, Lescarbault, claimed that he had seen an intra-Mercurial planet crossing the sun's disc. He related his discovery to Leverrier, who became a firm believer in the existence of such a planet. The perturbations of Mercury's orbit demand such a planet as Leverrier named Vulcan. The star Mr. Swift saw may have been the same that was seen by Prof. Watson, who was located at Rawlins, Wy. T.

Mr. Swift possessed a comet eye-piece of very flat and large field, and distinct to the very edge. It was made in Rochester, and to it and his blunder in failing to untie his instrument, he believes he owes his success.

The instrument used in the search for Vulcan by Prof. Holden proved to be inadequate to show all the stars on the Washington star map, owing to the brightness of the corona. The space where the planet seen by Prof. Watson was, was four times swept over, but so near to the sun as this, a four and a-half magnitude star was not to be seen. A space of 10° in declination by 35° in right ascension was twice swept over.

Here, again, we get an idea of the thoroughness with which the work has been planned and executed.

It would be wrong to conclude these hurried notes without stating that, from the day in which I landed in New York to the present time, I have become everybody's debtor for acts of kindness, which have touched me greatly. This great country is a land of true courtesy, for which I here express my gratitude, not only to my scientific brethren, and chiefly to Dr. Draper, General Myer, and Prof. Newcomb, whose guest I have been, but to hundreds to whom I have been a stranger and unknown.

As significant of the keen interest taken in the eclipse by all classes here, I may mention, in conclusion, that on the Sunday before the event prayers for fine weather were offered in all the churches of Denver.

Washington, D.C., Aug. 8 J. NORMAN LOCKYER

[1] On going to press we receive a letter from Prof. Watson, dated Ann Arbor, August 14, stating that the result of more careful examination gives —Washington M.T., July 29, 5h. 16m., R.A, 8h. 26m. 54s., Dec. + 18° 16′.

AS I have been recently giving attention to the subject of solar spectroscopy in consequence of my discovery of oxygen in the sun, it seemed desirable to take advantage of the total eclipse of July 29, to gain as precise an idea as possible of the nature of the corona, because the study of that envelope has been regarded as impossible at other times. The main point to ascertain was whether the corona was an incandescent gas shining by its own light, or whether it shone by reflected sunlight.

For this purpose I organised an expedition, and was fortunate enough to secure the co-operation of my friends Professors Barker and Morton, and Mr. Edison. The scheme of operation was as follows: (1) the photographic and photo-spectroscopic work, as well as the eye slitless spectroscope were to be in charge of my wife and myself; (2) the analysing slit spectroscope was in charge of Prof. Barker, with the especial object of ascertaining the presence of bright lines or else of dark Frauenhofer lines in the corona; (3) the polariscopic examinations were confided to Prof. Morton, who was also to spend a few moments in looking for bright or dark lines with a hand spectroscope; (4) Mr. Edison carried with him one of his newly-invented tasimeters with the batteries, resistance-coils, Thomson's galvanometer, &c., required to determine whether the heat of the corona could be measured.

This entire programme was successfully carried out,

and good fortune attended us in every particular. The results obtained were: (1) the spectrum of the corona was photographed and shown to be of the same character as that of the sun, and not due to a special incandescent gas; (2) a fine photograph of the corona was obtained, extending in some parts to a height of more than twenty minutes of arc, that is, more than 500,000 miles; (3) the Frauenhofer dark lines were observed by both Professors Barker and Morton in the corona; (4) the polarisation was shown by Prof. Morton to be such as would answer to reflected solar light; (5) Mr. Edison found that the heat of the corona was sufficient to send the index beam of light entirely off the scale of the galvanometer. Some negative results were also reached, the principal one being that the 1474 K, or so-called corona, line was either very faint or else not present at all in the upper part of the corona, because it could not be observed with a slitless spectroscope, and the slit spectroscope only showed it close to the sun.

The general conclusion that follows from these results is that on this occasion we have ascertained the true nature of the corona, viz., it shines by light reflected from the sun by a cloud of meteors surrounding that luminary, and that on former occasions it has been infiltrated with materials thrown up from the chromosphere, notably with the 1474 matter and hydrogen. As the chromosphere is now quiescent this infiltration has taken place to a scarcely perceptible degree recently. This explanation of the nature of the corona reconciles itself so well with many facts that have been difficult to explain, such as the low pressure at the surface of the sun, that it gains thereby additional strength.

The station occupied by my temporary observatory was Rawlins (lat. 41° 48′ 50″, long. 2h. 0m. 44s. W. of Washington, height 6,732 feet above the sea), on the line of the Union Pacific Railroad; because, while it was near the central line of totality, it had also the advantages of being supplied with water from the granite of Cherokee mountain, and of having a repair shop, where mechanical work could be done. I knew by former experience that the air there was dry and apt to be cloudless; in this particular our anticipations were more than fulfilled by the event, for the day of totality was almost without a cloud and the dew-point was more than 34° F. below the temperature.

The instruments we took with us were as follows, and weighed altogether almost a ton:—1st. An equatorial mounting, with spring governor driving clock, lent by Prof. Pickering, Director of Harvard Observatory. 2nd. A telescope of 5¼ inches aperture and 78 inches focal length, furnished with a lens specially corrected for photography, by Alvan Clark and Sons. 3rd. A quadruple achromatic objective of 6 inches aperture and 21 inches focal length, lent by Messrs. E. and H. T. Anthony, of New York. To this lens was attached a Rutherfurd diffraction grating nearly 2 inches square, ruled on speculum metal. The arrangement with its plate-holders, &c., will be designated as a phototelespectroscope. 4th. A 4-inch achromatic telescope with Merz direct vision spectroscope, brought by Prof. Barker from the collection of the University of Pennsylvania. 5th. A 4-inch achromatic telescope, also brought by Prof. Barker; to it was attached Edison's tasimeter. Besides these there were polariscopes, a grating spectroscope, an eye slitless spectroscope with 2-inch telescope, and finally, a full set of chemicals for Anthony's lightning collodion process, which, in my experience, is fully three times quicker than any other process.

The arrangement of the phototelespectroscope requires further description, for success in the work it was intended to do, viz., photographing the diffraction spectrum of the corona, was difficult, and, in the opinion of many of my friends, impossible. In order to have every chance of success it is necessary to procure a lens of large aperture and the shortest attainable focal length, and to have a grating of the largest size adjusted in such a way as to utilise the beam of light to the best advantage. Moreover, the apparatus must be mounted equatorially and driven by clockwork, so that the exposure may last for the whole time of totality, and the photographic work must be done by the most sensitive wet process. After some experiments during the summer of 1877 and the spring of 1878 the following form was adopted:—

The lens being of 6 inches aperture and 21 inches focal length, gave an image of the sun less than ¼ of an inch in diameter and of extreme brilliancy. Before the beam of light from the lens reached a focus it was intercepted by the Rutherfurd grating set at an angle of 60 degrees. This threw the beam on one side and produced there three images—a central one of the sun, and on either side of it a spectrum; these were received on three separate sensitive plates. One of these spectra was dispersed twice as much as the other, that is, gave a photograph twice as long. This last photograph was actually about two inches long in the actinic region. If now the light of the corona was from incandescent gas giving bright lines which lay in the actinic region of the spectrum, I should have procured ring-formed images, one ring for each bright line. On the other hand if the light of the corona arose from incandescent solid or liquid bodies, or was reflected light from the sun, I was certain to obtain a long band in my photograph answering to the actinic region of the spectrum. If the light was partly from gas and partly from reflected sunlight, a result partly of rings and partly a band would have appeared.

Immediately after the totality was over and on developing the photographs, I found that the spectrum photographs were continuous bands without the least trace of a ring. I was not surprised at this result because during the totality I had the opportunity of studying the corona through a telescope arranged in substantially the same way as the phototelespectroscope, and saw no sign of a ring.

The plain photograph of the corona taken with my large equatorial (exposure 150 seconds) on this occasion shows that the corona is not arranged centrally with regard to the sun. The great mass of the matter lies in the plane of the ecliptic but not equally distributed. To the eye it extended about a degree and-a-half from the sun toward the west, while it was scarcely a degree in length toward the east. The mass of meteors, if such be the construction of the corona, is therefore probably arranged in an elliptical form round the sun.

For the fortunate results of this expedition we are not a little indebted to the railroad and express companies. The Pennsylvania, the Chicago and North-western, and the

Union Pacific railroads, the Pullman Palace-Car Company, and the American and Union Pacific Express companies made the most liberal arrangements, and Mr. Galbraith, the superintendent of the Repair Works at Rawlins, gave us the free use of his private house and grounds. Of the citizens of Rawlins it is only necessary to say that we never even put the lock on the door of the observatory, and not a thing was disturbed or misplaced during our ten days' residence, though we had many visitors. They sent us away with a serenade. HENRY DRAPER

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Floating of Solid on Molten Metal

I OBSERVE in NATURE (vol. xviii. p. 397) a note of some experiments on this subject. The results of these experiments (unless with lead) are, I think, very similar to some which I have made, and described in your pages (see NATURE, vol. xvi. p. 23), viz., that with *heavy pieces* the metal first *sank* and then *rose to surface*; with light pieces, the "skin" formed on the surface of the ladle was sufficient to keep them afloat. From these experiments I drew the conclusion that the cold solid metal was specifically *heavier* than the molten metal, but after a short immersion, depending on size of pieces, these pieces had *expanded* by the great heat around them so as to have *their bulk increased sufficiently* to enable them to float. My experiments with solid pieces of lead showed that they *sank* and did not come to the surface, and could be felt lying at bottom. Pieces of sheet lead rolled up floated.

In some recent experiments which I made, I found that *cold* pieces of steel rails placed in a furnace of molten steel sunk at first and floated afterwards, but that *hot pieces floated*, and did not sink. W. J. MILLAR

100, Wellington Street, Glasgow, August 10

A Meteorite ?

THIS day, at 12.15 P.M., I was considerably startled by what was to me a remarkable phenomenon. The weather had been very "thundery" all the morning, and heavy rain was falling in torrents. I sat at my desk by a window looking out upon a court inclosed by high walls. Chancing to look out of the window I heard a sharp report, just like the crack of a Snider rifle sounding immediately outside, followed instantaneously by the descent of a ball of fire about the size of an ordinary gas-lamp globe. This fell vertically and with lightning speed, but when just on a line with the centre of my window burst into a splendid mass of rays, whitish-blue in colour, and of dazzling brilliancy. That is all I can tell you about it. Every one in the house heard the report, and quite a temporary panic ensued. No material effect of the meteorite's presence can be found.

Perhaps some of your readers may be able to explain the phenomenon. J. HARRIS STONE

67, Chandos Street, Strand, August 23

The Australian Monotremes

THE *Tachyglossus* was shot by me near Georgetown, in lat. 18° S. I have found it inhabiting the porphyritic ranges (Newcastle and Mount Turner) in this locality, where they are rather numerous. In my letter (NATURE, vol. xvi. p. 420) I should have written "integumentary" pouch. The *Orinthorhynchus paradoxus* I saw floating with its bill above water in a lagoon between Georgetown and Normanton, 150 miles west of the former town.

Owing to the noise made by my detachment in riding up, I was unable to capture this specimen, but I do not despair of securing one on my next trip westward. I certainly believe the *Tachyglossus* extends throughout the length of the Cape York peninsula on the east, and through the Gregory, Leichhardt, and Cloncurry ranges to the southward and south-westward of Georgetown. Its habit of burrowing beneath rocks precludes the possibility of its occurrence on the Lower Gilbert and Flinders River plains.

"P. L. S." will find my "notes" on this subject in the Linnæan Society's *Journal*, as I sent them, accompanied by the skull of an adult female, to the Society in March last.

Georgetown, June 1 W. E. A.

Microphone in Indirect Circuit

IT is not absolutely necessary that the microphone should form part of the direct circuit. It works just as well if connected so that, when the carbons are not touching, the whole of the current goes through the telephone. When the carbons are together a small portion will of course leak through them; upon this leakage depends the rise and fall of tension in the receiver. For some experiments it is even better to work the microphone in this indirect manner, as the circuit always remains closed, and prevents, in a great measure, the *jarring* noise resulting from a break. ALFRED CHIDDEY

Bristol Mining School, August 19

OUR ASTRONOMICAL COLUMN

THE SATELLITE OF NEPTUNE.—We here present in a tabular form the means of determining the approximate position and distance of the satellite of Neptune, with respect to the primary for any time during the months of September and October, or indeed by extending the epochs subjoined, for any time during the present opposition. The argument u is the distance of the satellite from the ascending node of the orbit upon the earth's equator, and $u = 0°$, at these Greenwich mean times :—

		h. m.			h. m.
Sept. 4	...	18 38·3	Oct. 4	...	3 52·0
,, 10	...	15 41·0	,, 10	...	0 54·7
,, 16	...	12 43·8	,, 15	...	21 57·5
,, 22	...	9 46·5	,, 21	...	19 0·2
,, 28	...	6 49·3	,, 27	...	16 3·0

The motion of u in one day is 61°·257, in one hour 2°·552, and in one minute 0°·0425. Having determined the value of u from these epochs and motions for the proposed time of observation, the angle of position and distance of the satellite from the centre of the planet may be taken from the following table, in which the first and second columns of angles apply to the respective columns of the argument u :—

Arg. u.		Angle of position.		Distance.	Arg. u.		Angle of position.		Distance.
°	°	°	°	"	°	°	°	°	"
0	180	71·6	251·6	9·8	90	270	29·8	209·8	15·0
10	190	63·1	243·1	11·7	100	280	25·0	205·0	13·6
20	200	56·9	236·9	13·5	110	290	18·9	198·9	11·8
30	210	52·0	232·0	14·9	120	300	10·6	190·6	9·9
40	220	48·0	228·0	16·0	130	310	358·2	178·2	8·0
50	230	44·3	224·3	16·7	140	320	338·9	158·9	6·4
60	240	40·9	220·9	17·0	150	330	311·7	131·7	5·8
70	250	37·5	217·5	16·8	160	340	284·2	104·2	6·4
80	260	33·8	213·8	16·1	170	350	264·4	84·4	7·9
90	270	29·8	209·8	15·0	180	360	251·6	71·6	9·8

The period of revolution of the satellite is 5d. 21h. 2·74m., and by successive additions of this period the epochs may be continued for November or later.

As an example of the application of the table, suppose it is desired to know the approximate position of the satellite on September 14 at Greenwich midnight. Strictly the time for aberration should be deducted, which, in minutes, is given by [0·9189] × log. distance of Neptune from the earth—this log. distance being taken from p. 269 of the *Nautical Almanac*. In the present case we find 4h. 1·9m. to be deducted from 12h., so that

the time to be used in the calculation is September 14, 7h. 58·1m. This time follows the epoch in the above table by 2d. 13h. 36·4m., and with the motions already given, the value of u corresponding to this interval is found to be 225°·34, with which we enter the table and find the angle of position 226°, and the distance 16″·4. A direct calculation from Prof. Newcomb's tables of the satellite for the reduced Greenwich time gives 226°·4 and 16″·4.

NOTES

THE Chemical Society have lately made the following grants from their Research Fund:—50*l.* to Dr. Tilden, for an investigation into the chemical nature of the terpenes; 50*l.* to Mr. W. N. Hartley, for apparatus and materials required in carrying on a research on the action of organic substances on the ultraviolet rays; 30*l.* to Dr. W. Ramsay, for determining the electric conductivity and resistance of solutions of salts at different temperatures; 20*l.* to Mr. W. Jago, for the purchase of apparatus required for carrying on a research on the organic matter in sea-water; 10*l.* to Mr. W. A. Shenstone, for an examination of certain reactions of brucine and strychnine.

IT is understood that Signor d'Albertis has parted with the whole of the extensive zoological collections made during his two last expeditions up the Fly River, New Guinea, to the Marquis G. Doria, of Genoa, who will, no doubt, deposit them in the Museo Civico of that city, of which he is the founder and principal benefactor. These collections were first offered to the British Museum. Signor d'Albertis is now making arrangements for the publication in London of a narrative of his adventures in New Guinea.

WE regret to hear that the valuable collection of Chinese birds made by the late Mr. Robert Swinhoe, F.R.S., is still undisposed of. It was offered, we are told, to the British Museum, but, as too often happens in such cases, declined. It would be greatly to be lamented if this collection, which contains many types of species first described by Mr. Swinhoe, and the originals of his numerous papers on Chinese ornithology, should pass out of the country. It certainly ought to have been acquired for the national collection, even if a little more than what was considered its full value had had to be paid for it.

WE understand that the authors of the "Unseen Universe" are at present engaged on a work intended to serve as a sequel to that well-known volume.

WE are requested to state that Sir Joseph Whitworth having expressed a desire that some important alterations should be made in the conditions of his scholarships, the detailed rules for carrying out his wishes are now under consideration. They will be published as soon as possible; but in order to prevent disappointment this notice is given. No important changes will be made in the conditions of the competitive examination in May, 1879. But the conditions of the tenure and of the amount of the scholarships may be somewhat modified.

THE meeting of the French Association for the Advancement of Science was inaugurated in the large hall of the Sorbonne by an address by M. Fremy on Soda and Steel in 1878. The address was well received by a large audience, but it is generally regretted that the president did not take a subject of wider bearing for his address. MM. Virchow and Haeckel were present during the address. It is the first time that German men of science have taken part in French public assemblies in their private capacity. M. Gambetta was also present. Commandant Perrier gave a sketch of the work done at the last year's meeting of the Association. The number of members is now 2,384, the income for 1877 was 58,000 francs, and its capital 224,897 francs. The grants for 1877 amounted to 13,850 francs. A part of the effect of this scientific assembly is lost this year on account of the number at the Trocadéro, and the multitude of objects demanding the attention of Paris visitors. The morning sittings of the Meteorological Section are held at the Lycée St. Louis, and the evening meeting at the Trocadéro as a Congress of the Exhibition. One of the most interesting addresses will no doubt be that delivered by Dr. Janssen on celestial physics; and he will also give the substance of the papers read by him before the British Association. There are two candidates for the presidentship of the meeting of 1880, M. Krantz, Director of the Exhibition, and Col. Laussedat, of the Engineer service. Very probably the former will be successsful. Several cities are competing for the honour of receiving the Association in 1880. It seems probable that it will cross the Mediterranean to Algiers; M. Krantz is spoken of as the intended Governor-general of Algiers after the close of the Exhibition. Next year's meeting will take place at Montpellier under the presidency of M. Bardoux, the Minister of Public Instruction. On Sunday the Congress visited the Paris Observatory and the Meteorological Observatory at Montsouris.

AT the general meeting of the International Botanical and Horticultural Congress held at the Trocadéro, Paris, on the 16th inst., M. A. de Candolle was elected president. A *soirée* or reception was held on the evening of the 16th, and the members of the Congress went on the 18th to Sagrez, the residence of M. A. de Lavallée, president of the organising committee, by whom they were most hospitably entertained. The collection of hardy woody plants at Sagrez is unique in completeness in every sense.

ON Thursday last Mr. Cunliffe Owen was entertained at luncheon by some of the leading U.S. exhibitors at the Paris Exhibition, on board Capt. Boyton's yacht. Governor M'Cormack, in proposing Mr. Owen's health, spoke very highly of his executive abilities, and his courtesy towards the Americans, and attributes much of the success of the exhibits of English-speaking people to his energy, skill, tact, and good management.

IN the *Times* of August 13 an account is given of the working of the Mallory propeller, a very ingenious mechanical device for propelling and steering a ship in any direction by means of one and the same apparatus, recently brought over to England from America by its inventor, Col. William H. Mallory, of the United States army. It consists of a vertically swivelled screw propeller of peculiar construction, by the aid of which a vessel can be moved sideways, turned rapidly on a circle whose diameter is the ship's length, and can be made to advance or retire with equal rapidity. We cannot see any reason why this propeller may not be very useful for small vessels and others whose turning is more important than speed and sea-going qualities. That it will ever replace the direct propeller seems improbable, as there are three distinctly bad points about it:—1, the gear, which entails a loss of at least ten per cent. of the power; 2, the engines working on to a vertical shaft; 3, the engines being over the counters, which would render the ship very uneasy in a rough sea. Besides this, no vessel would go to sea without a rudder to use in case her engines broke down. On the other hand, the propeller presents many advantages for harbour work, and for such things as rams and torpedo boats.

WE learn from *Harper's Weekly* that, for the purpose of prosecuting biological researches, Prof. A. Agassiz has lately completed a superb establishment near his residence at Newport, wherein every device that experience could suggest has been brought to bear for the convenience of investigators. A building 45 by 25 feet has been erected on the side of a bay making up from the entrance to Newport Harbour, and provided with the purest of sea-water by means of a steam-pump, which keeps a tank constantly filled. The tables are covered with a series of

tiles of different colours, so that the minute animals of different shades can be the more readily overhauled when emptied upon them. The shelves in the laboratory are all of glass, the tanks are of slate, the conducting pipes are of iron, lined with a composition of rubber, which it is believed will protect them against corrosion. These tables are all well lighted, and are available for students, whom Mr. Agassiz invites to share his facilities. Four persons, in addition to himself, at present occupy the laboratory in prosecuting their special researches. There is, probably, no one of the many buildings erected here and in Europe for the prosecution of biological research so elegantly and thoroughly equipped as that to which we refer.

PROF. F. E. NIPHER, of Washington (U.S.) University, has undertaken a magnetic survey of the state in connection with the weather service. The work will require about three years to complete, and he contemplates getting over one-third of the state this summer. He will establish about twenty or twenty-five magnetic stations, to determine the dip of the magnetic needle and the deflection from the magnetic meridian. The professor started out on the survey early in July, accompanied by five or six students from Washington University, who will act as the assistants. He has received a complete set of instruments from the United States Coast Survey, consisting of a dip-needle and declination needle, with theodolite. The party will first go to St. Charles, and up the North Missouri railroad, taking in a territory as far west as Chillicothe, and embracing among the points of observation Hannibal, Macon City, Mexico, Columbia, Fulton, Kirksville, Moberley, and other eligible points.

MR. AUGUSTUS FENDLER, whose collections of plants, made thirty years ago in the vicinity of Santa Fé, were made known by Prof. Gray, and who for many years resided in Venezuela as a botanical collector, is now prosecuting similar work in the Island of Trinidad, devoting himself to the gathering of ferns and fern-like plants. Sets of the first series of these, embracing the first thirty-eight species, are procurable at a moderate price from the curator of Harvard University Herbarium, Cambridge, Massachusetts. They have all been named by Prof. Eaton, of Yale College.

SOME months ago we gave an account of the explorations of a young American, Mr. Herbert H. Smith, on the Amazon, referring to some geographical discoveries made by him, as also to his success in securing a large series of insects. Mr. Smith left New York on July 6 for Brazil, first to complete his work on the Amazon, and then to proceed to the southern provinces of Brazil.

A VIOLENT earthquake is reported from Innspruck. It occurred on the 9th inst. at 12.40 P.M., and was accompanied by loud subterranean noise. At 9 A.M. on the 26th, earthquake shocks were felt over a great part of Belgium and Holland, and in Rhenish Prussia at about 11 A.M. A shock of earthquake was felt at nine on the morning of the 26th inst. at Liége, doors and windows being much shaken and chairs disturbed. It was also felt at Elberfeld, Cologne, Osnabrück, and Barmen. At Barmen houses were upreared, roofs displaced, furniture shaken, and goods in the shop windows overturned. The shocks were repeated about eleven at Elberfeld, Düsseldorf, Cologne, and Bonn.

AT Halberstadt a remarkable Fata Morgana was observed on the 5th inst. at 7.10 P.M. The phenomenon is described by an eye-witness, who states that in a stratum of cloud in the direction of the Brocken both house and tower standing on the summit of that mountain were reproduced in distinct outlines and on a gigantic scale; even the windows could be recognised. The duration of the phenomenon was about one minute.

THE meeting of German anthropologists took place at Kiel on the 12th, 13th, and 14th inst., and was well attended. Strassburg was chosen as the meeting-place for next year. Prof. Schaaffhausen of Bonn presided, and addresses were delivered by Professors Handelmann, Mehlis, Fraas, Virchow, Ranke, Stieda, and others. On August 14 an excursion was made to Lübeck. The Schleswig-Holstein Anthropological Society had arranged an interesting exhibition for the occasion.

THE British Archæological Association concluded a very successful meeting at Wisbeach on Saturday. Under the presidency of the Bishop of St. David's, the Cambrian Archæological Association held its annual gathering at Lampeter last week.

A CANTON correspondent, under date April 12, sends us the following account of the tornado of April 11 :—Yesterday afternoon, April 11, Canton was visited by a whirlwind of terrific force and unparalleled destructiveness. There was a thunderstorm from 2 P.M. to 3.30; when this was over, lumps of ice, about the size of pigeons' eggs, but shaped like the star-fish, fell in great quantities. Our surprise was hardly over at this strange phenomenon (thermometer 80° F. in the shade) when a noise was heard, like that made by the screw of a ship as a person on board sleeping close to it would hear it, rising and falling in regular rhythm. This sound was occasioned by the travelling of a wind of an intensity that baffles description, which burst upon the city and settlement, scathing and blasting everything which came within its fell grasp. For a space of time, variously estimated at from three to five minutes, it performed its work of destruction, uprooting trees, unroofing houses, overturning walls, engulfing boats, and leaving behind a scene of desolation such as only an eye-witness would believe in. The path along which the tornado passed was converted into shapeless ruins, but fortunately its width was not more than 200 yards. In Shamien (the foreign settlement) 134 large banyans, some of eighteen years' growth, have been blown down, most of them torn up by the roots, and in rare cases the trunks have been rudely snapped in two. A large banyan, distant twenty yards from our house, was torn up and hurled roots foremost, right into the verandah, smashing to chips the solid stone balustrade. One single instance will give you a fair idea of the force of this hurricane. A tile was found sticking in the side of a tree (in the Consulate grounds), into which it had penetrated two inches. A coolie was killed on the road fifty yards from our house by a brick blown away. Men were blown yards into the air and killed, and in one instance, a cow was blown up, but was not killed. The stone seats in the Bund, and stones over a ton weight have been driven yards away. The immense granite blocks forming the parapets of the bridge were hurled bodily into the canal. The track followed by the tornado through the native city is marked by a confused mass of bricks and mortar, and in some places there is literally "not left one stone standing on another." According to a native estimate the number of houses entirely destroyed is about 5,000, the total damage to *foreign* houses about 12,000*l.* sterling, and the loss of life 6,000. Already 4,000 corpses have been interred, and more are being dug up every minute.

WE have received the first part (*Polypetalæ*) of a "Diagnoses Plantarum Novarum vel minus Cognitarum Mexicanarum et Centrali-Americanarum," by Mr. W. B. Hemsley. This, we believe, is merely the forerunner of a work of great extent and importance which Mr. Hemsley has undertaken, of the scope of which the following statement may give some idea. It may be known to some of our readers that Messrs. Godman and Salvin, aided by various specialists, have for many years been engaged in working up the zoology of Central America and Mexico. About three years ago it occurred to them that it would add greatly to the interest of their work if the botany could be so far worked

up as to determine the general laws of distribution for comparison with those obtaining in the animal kingdom. After some consultation with Sir J. D. Hooker, and other botanists, it was decided to make the Kew herbarium the basis of an enumeration of all the plants hitherto found in the countries lying between Panama and the territories of the United States. The principal reason that led to this decision was the fact that the vast collections at Kew have recently been carefully studied for Messrs. Bentham and Hooker's "Genera Plantarum," and for various other publications, so that as far as generic types are concerned, Kew herbarium is perfectly available and reliable for the object in view. To this will be added all the species published, but not represented at Kew by named specimens, and the novelties contained in the collections made by the French Scientific Commission. The enumeration will be supplemented by references, localities, altitudes, and everything that will be useful in drawing up an essay on the distribution, &c., of the plants. Although it was impossible to undertake a critical examination of all the species, a considerable number of interesting and apparently new species will be described, and many of them figured, from drawings by Fitch, and coloured drawings done on the spot by Mrs. Salvin.

THE valuable collection made by the Brothers von Schlagintweit during their extensive journeys through India and Thibet is now arranged for exhibition in the royal castle at Nuremberg. It forms one of the most extensive collections ever brought from the East, and possesses objects of rare value, especially for anthropology. Among these might be mentioned the plastic representations of Indian races, numbering 275 specimens, prepared from casts taken from living individuals, and carefully arranged according to castes and races. A large variety of skulls from different regions in India will also prove of no small use to the growing number of students of cranial development.

WE notice the death at Paris of M. J. Fordos, a well-known French chemist, and vice-president of the Paris Chemical Society. His name is chiefly known in connection with researches on thio-acids, and numerous derivatives of sulphur, ligneous colouring matters, estimation of morphium, and several technical subjects, especially the manufacture of cyanides and ferro-cyanides.

PROF. SUESS, the well-known Vienna geologist, is at present traversing Italy with a number of his students, engaged in a geological study of the peninsula. The journey includes Vesuvius and Etna.

THE additions to the Zoological Society's Gardens during the past week include a Prairie Wolf (*Canis latrans*) from the Rocky Mountains, North America, presented by Prof. M. C. Vincent, F.G.S., F.R.G.S.; a Common Jackal (*Canis aureus*) from India, presented by Mr. J. Smith; a Spotted Ichneumon (*Herpestes auropunctatus*) from Nepal, presented by Mr. W. Pyne; a Bronze Fruit Pigeon (*Carpophaga ænea*) from India, presented by Mr. A. H. Jamrach; a White-eared Bulbul (*Pycnonotus leucotis*) from North-West India, presented by Mr. W. Woolner; a Black Saki (*Pithecia satanas*) from the Lower Amazons, deposited.

THE BRITISH ASSOCIATION

REPORTS.

Report of the Committee for Commencing Secular Experiments on the Elasticity of Wires, by J. T. Bottomley.—The Committee have to report that the arrangements for suspending the wires for secular experiments on elasticity are now complete, and that within the last few days two wires, one of palladium and the other of platinum, have been suspended in their places.

An iron tube has been erected in one of the rooms in the tower of the University Buildings in Glasgow. It is 60 feet long, 9 inches wide, and 4½ inches deep from face to back. It is of rectangular section, in lengths of 6 feet, and it is supported by being firmly attached to the heavy outer stone wall of the tower.

At the top of the tube there is a heavy gun-metal plate which is supported independently of the iron tube; and from this plate the wires under examination are to be suspended, as well as additional wires to be used for carrying additional comparison marks. With this arrangement no yielding of the supporting plate that may take place will introduce errors into the results of measurement of the lengths of the wires; for the point of support of the wire carrying comparison marks will experience the same amount of lowering, due to the yielding, as is experienced by the wire to be measured against these marks. The gun-metal plate has been pierced with three rows of holes, through which the wires are to pass. The holes are trumpeted at each end, so as to avoid sharp contact with the wires; and the rows are arranged so that the wires shall hang down in their planes parallel to the face of the tube. It has not yet been decided what is the best way of fixing the upper ends of the wires above the gun-metal plate, or of attaching the weights to their lower ends. No thoroughly satisfactory mode of attachment has yet been found. In the course of experiments to be referred to immediately, which have been carried on at Glasgow on the breaking weight, and the Young's Modulus of Elasticity of the gold, platinum, and palladium wires, which, it is intended, shall be first suspended for examination, several modes of suspension have been tried; but it has not been found possible to make sure of avoiding very considerable weakening of the wire at the points of attachment at the ends.

At the bottom of the iron tube there is a window of plate glass through which the lower parts of the wires can be viewed, and the window can be drawn up so as to allow of the lower parts of the wires being reached.

In front of the window a strong gun-metal table is set up. It is supported independently of the iron tube and of the floor of the room, on iron brackets fixed to the stone wall of the chamber, and is very carefully levelled. On this table a cathetometer is carried, by means of which marks on the wires are to be observed. The cathetometer moves on the table parallel to the planes of the rows of wires. It has the two back feet of the triangular sole plate on which it is supported movable in a V-groove cut in the table, the third foot resting on the plane upper surface. There is also a slot cut in the table through which a screw passes up from below to the sole plate of the cathetometer, and by means of this screw the cathetometer can be clamped in any required place.

The cathetometer is a small instrument which has been constructed by Mr. James White of Glasgow for the purpose of these experiments. The main pillar is one foot high. It is supported on a sole plate having three levelling screws. The telescope or microscope, having cross-fibres, is raised or lowered on this pillar on a proper geometrical slide, and has also a lifting screw in connection with a vernier for giving fine adjustment. The vertical pillar is carefully graduated; and by means of this scale the differences of levels of proper marks put upon the wires are to be determined.

The arrangements have only been completed within the last few days. They require to be carefully tested in several points, and particularly the cathetometer requires careful examination. There is every reason, however, to expect that the work will turn out quite satisfactory. As soon as possible the work of testing will be completed and wires suspended, measured and marked.

During the past year experiments in connection with this investigation have been carried on in the laboratory of the University of Glasgow on the breaking weights and elastic properties of various wires. In the first place the breaking weights and the Young's modulus, or modulus of elasticity for longitudinal pull, have been determined for the gold, platinum, and palladium wire with which it is proposed that the secular experiments on elasticity shall commence. A large number of experiments on the effect of stress, maintained for a considerable time, in altering the breaking weight and the extension under increased stress of various wires, have been carried on. Soft iron wire, steel wire, and tin wire in particular, have been experimented upon, and already some interesting results have been obtained, showing that prolonged application of stress certainly produces a noticeable effect.

Numbers showing the nature of the results already arrived at are appended; but the whole subject still requires much careful examination.

Report of the Committee consisting of James R. Napier, F.R.S., Sir W. Thompson, F.R.S., W. Froude, F.R.S., J. T. Bottomley, and Osborne Reynolds, F.R.S., Sec., appointed to investigate the effect of Propellers on the Steering of Vessels.—The Committee are now of opinion that the work for which they were originally brought together has been fully accomplished. The importance of the effect of the reversed screw on the action of the rudder has been fully established, as well as the nature of its effect completely ascertained. Also, for two years, the Committee have urged the results of their work upon the attention of the Admiralty and the various marine boards, and although they regret that, as yet, they have failed to obtain that general recognition of the facts brought to light which their vital importance demands, they consider that this will surely follow, and that as a Committee they can do no more than publish the reports of the trials and the conclusions to which they have been led.

The following is a summary of the conclusions which have been established, and it is interesting to notice that the conclusions drawn by Prof. Reynolds from experiments on models, have been fully confirmed by the experiments on full-sized ships.

Summary of the Results of the Trials of the Effect of the Reversed Screw on the Steering during the Time a Vessel is stopping Herself.

It appears, both from the experiments made by the Committee and from other evidence, that the distance required by a screw steamer to bring herself to rest from full speed by the reversal of her screw, is independent or nearly so of the power of the engines, but depends on the size and build of the ship, and generally lies between four and six times the ship's length. It is to be borne in mind that it is to the behaviour of the ship during this interval that the following remarks apply:—

The main point the Committee have had in view has been to ascertain how far the reversing of the screw, in order to stop a ship, did or did not interfere with the action of the rudder during the interval of stopping, and it is as regards this point that the most important light has been thrown on the question of handling ships. It is found an invariable rule that, during the interval in which a ship is stopping herself by the reversal of her screw, the rudder produces none of its usual effects to turn the ship, but that, under these circumstances, the effect of the rudder, such as it is, is to turn the ship in the opposite direction from that in which she would turn if the screw were going ahead. The magnitude of this reverse effect of the rudder is always feeble, and is different for different ships, and even for the same ship under different conditions of loading.

It also appears from the trials, that, owing to the feeble influence of the rudder over the ship during the interval in which she is stopping, she is then at the mercy of any other influences that may act upon her. Thus the wind, which always exerts an influence to turn the stem (or forward end) of the ship into the wind, but which influence is usually well under control of the rudder, may, when the screw is reversed, become paramount, and cause the ship to turn in a direction the very opposite of that which is desired. Also, the reversed screw will exercise an influence, which increases as the ship's way is diminished, to turn the ship to starboard or port, according as it is right or left-handed; this being particularly the case when the ships are in light draught.

These several influences, the reversed effect of the rudder, the effect of the wind, and the action of the screw, will determine the course the ship takes during the interval of stopping. They may balance, in which case the ship will go straight on: or any one of the three may predominate, and determine the course of the ship.

The utmost effect of these influences when they all act in conjunction, as when the screw is right-handed, the helm starboarded, and the wind on the starboard side is small as compared with the influence of the rudder as it acts when the ship is steaming ahead. In no instance has a ship tried by the Committee been able to turn with the screw reversed on a circle of less than double the radius of that on which she would turn when steaming ahead. So that even if those in charge could govern the direction in which the ship will turn while stopping she turns but slowly, whereas, in point of fact, those in charge have little or no control over this direction, and unless they are exceptionally well acquainted with their ship they will be unable even to predict the direction.

It is easy to see, therefore, that if on approaching danger the screw be reversed, all idea of turning the ship out of the way of the danger must be abandoned. She may turn a little, and those in charge may know in which direction she will turn, or may even, by using the rudder in an inverse manner, be able to influence this direction, but the amount of turning must be small, and the direction very uncertain. The question, therefore, as to the advisability of reversing the screw is simply a question as to whether the danger may be better avoided by stopping or by turning. A ship cannot do both with any certainty.

Which of these two courses it is better to follow must depend on the particular circumstances of each particular case, but the following considerations would appear to show that when the helm is under sufficient command there can seldom be any doubt.

A screw steamship when at full speed requires five lengths, more or less, in which to stop herself; whereas, by using her rudder and steaming on at full speed ahead, she should be able to turn herself through a quadrant without having advanced five lengths in her original direction. That is to say, a ship can turn a circle of not greater radius than four lengths, more or less (see Hankow, Valetta, Barge),[1] so that, even if running at full speed directly on to a straight coast, she should be able to save herself by steaming on ahead and using her rudder after she is too near to save herself by stopping; and any obliquity in the direction of approach or any limit to the breadth of the object ahead is all to the advantage of turning, but not at all to the advantage of stopping.

There is one consideration, however, with regard to the question of stopping or turning which must, according to the present custom, often have weight, although there can be but one opinion as to the viciousness of this custom. This consideration is the utter inability of the officers in charge to make any rapid use of their rudder so long as their engines are kept on ahead. It is no uncommon thing for the largest ships to be steered by as few as two men; and the mere fact of the wheel being so arranged that two men have command of the rudder renders so many turns of the wheel necessary to bring the rudder over that, even where ready help is at hand, it takes a long time to turn the wheel round and round so as to put a large angle on the rudder.

The result is, that it is often one or two minutes after the order is heard before there is any large angle on the rudder, and of course, under these circumstances, it is absurd to talk of making use of the turning qualities of a ship in case of emergency. The power available to turn the rudder should be proportional to the tonnage of the vessel, and there is no mechanical reason why the rudder of the largest vessel should not be brought hard over in less than fifteen seconds from the time the order is given. Had those in charge of steamships efficient control over their rudders, it is probable that much less would be heard of the reversing of the engines in cases of imminent danger.

Report of the Committee on the best Means of Developing Light from Coal Gas. Read by Mr. T. Wills.—This first part of the report was by Dr. Wallace, and referred to cannel gas, the standard quality of which was assumed to be twenty-six candles. That was the quality delivered by most of the Scottish gas companies, while in London the illuminating power was sixteen candles; in Liverpool, Manchester, and Carlisle, twenty candles; and in most of the other cities in England and Ireland about fourteen candles. The report was largely occupied by a tabulated series of results obtained with different kinds of burners, such as the rat-tail, onion or fish-tail, batwing, and argand, and also the influence of globes of different sizes, shapes, and materials. The report was strongly in favour of cannel rather than common gas, on account of its comparatively small influence on the atmosphere of apartments and the smaller proportion of sulphur it contained. The report also advocated the burning of gas at a comparatively low pressure, and the use of district governors to equalise the pressure in different levels of towns, and of regulators in houses and street lamps, to give the exact pressure calculated to give the best photometric results.

Fourteenth Report of the Committee for Exploring Kent's Cavern, Devonshire. (Abstract.)

The Tortuous Gallery.—When their thirteenth report was drawn, at the end of July, 1877, the Committee had completed the exploration of the "Tortuous Gallery," with the exception of the portions of it termed the "Terminal Chamber" and the "Recess." On entering the Chamber its floor was found to be a complete pavement of blocks of limestone, the removal of which disclosed an almost horizontal bed of the typical breccia —the most ancient deposit yet found in the cavern. It was excavated to the customary depth of four feet, but without reach-

[1] B.A. Reports, 1876-77.

ing its base anywhere. The Chamber measured thirty feet from north to south, from seven to thirteen feet from east to west, and about the same in height. The only objects met with were four pieces of bone and a large lump of oxide of manganese. The Recess yielded twelve teeth of bear and several pieces of bone.

The Undervault.—On the completion of the "Tortuous Gallery" the exploration of an adjacent branch of the cavern, called the "Undervault," was begun. The deposit found in it must be regarded as an uncertain admixture of breccia and the less ancient cave-earth, washed confusedly together. In it were found forty-seven teeth of bear, thirty-three of hyæna, two of fox, numerous bones and fragments of bone, one chert flake, and the greater portion of a large quartzite pebble. Many of the teeth of both bear and hyæna were in jaws and portions of jaws, and some of them were remarkably fine specimens.

The Great Oven.—That branch of the cavern termed the "Great Oven" was partly explored in 1875, but its "Eastern Reach" was then left intact. This reach occupied the Committee from December 18, 1877, to February 15, 1878. It was thirty-four feet long, and varied in width from ten to three feet. The deposits were, in descending order, (1) granular stalagmite, a few inches thick only; (2) cave-earth, also but a few inches thick; (3) crystalline stalagmite, from two to three feet thick; and (4) breccia, the base of which was nowhere reached. The following specimens were exhumed:—Twenty teeth of bear, eight of hyæna, and three of fox, in the cave-earth; one tooth of bear, in the crystalline stalagmite; and fifteen teeth of bear in the breccia. The relics of hyæna were accompanied by a few coprolites.

The High Chamber.—A branch of the cavern, the most remote from the external entrances, has been called the "High Chamber" on account of its suddenly rising several feet above the level of the "Cave of Inscriptions," out of which it opens. Its exploration was commenced in February, 1878, and is still in progress. Up to the end of July it had yielded fifty-three "finds," including eighty-nine teeth of bear, numerous bones and pieces of bone, a chert implement, two chert flakes, and a quartzite pebble, all found in the breccia. There are reasons for believing that this chamber will lead to an entrance of the cavern hitherto unknown.

The president proposed a vote of thanks to Mr. Pengelly. With regard to the quartzite pebble found in the high chamber it appeared to be a block from which a fragment had been separated for some useful purposes. The flint implements are altered, and coated with a white soft substance, some of its original substance having been removed by water.

Prof. Boyd Dawkins said the exploration was a great feat of engineering. He objected to the term quaternary as some times applied to these deposits. The tertiary, quaternary, and pleistocene periods are so closely twined that separation is impossible. He accounted for the arrangement of the animal remains by alternations of climate. In some caves they found generally Arctic animals like the reindeer and glutton associated with animals that were now only found in warm climates, and the probable cause was that there was a great stretch of land over the greater part of the Mediterranean area, from the north of Africa over the region of Europe, until it finally reached Great Britain, and even Ireland. This would afford a means of migration to animals northward and southward, according to the season. This was also shown by the Pyrenees, and so on through Germany, until they arrived at this country. In the Derbyshire caves, Cresswell Cave, for instance, there was a sequence of events very much the same as in this cave. He was now of opinion that the *Machairodus* was a late cave animal, being found in upper cave earth of Cresswell Cave, as well as in Kent's Hole.

Mr. Pengelly was glad to hear Prof. Boyd Dawkins' recantation as to *Machairodus*.

Mr. Plunket read a *Report of the Committee* (secretary, Prof. A. Macalister, M.D.) *on the Fermanagh Caves*, and exhibited a number of animal remains, &c., collected in them.

Prof. Leith Adams said they could not come to any conclusion yet as to the animal remains, for they had not been examined. From what they knew of the extinct animals of these islands, they must come to the conclusion that there was a land connection between England and Ireland.

Prof. Boyd Dawkins said he had looked at the bones, and he found the remains of a domestic hog, shorthorn ox, goat, and red deer—animals which had been used for food—and the bone of a large dog, probably the large deerhound of the sixteenth and seventeenth centuries.

The fourth *Report of the Underground Water Committee* was read by Mr. C. E. De Rance, F.G.S., Assoc. Inst. C.E.—The value of the Government geological maps as a basis for investigation in questions of water-supply was acknowledged; and the areas occupied by permeable formations capable of yielding water in wells sunk in suitable situations was stated to be no less than 26,687 square miles. This, receiving a rainfall averaging thirty inches a year, would yield up to wells not less than six to fifteen inches per annum, or a daily quantity of not less than 240,000 gallons for each square mile of surface, a total quantity far in excess of that required by the population of England or Wales. The great value of the supplies for the towns and districts of the Midland Counties was insisted on because of their purity, and from the unlikelihood of such prospects of water-supply from such sources being stopped by the strong parliamentary opposition which is brought to bear against all large gravitation schemes, whether the water be proposed to be taken from natural lakes, as the Thirlmere for Manchester, or from natural reservoirs as was proposed in the case of Liverpool.

The well borings at Bootle, near Liverpool, of 26 inches diameter just completed for the Liverpool Corporation by Messrs. Mather and Platt, were described as of great interest, the boring having reached a depth of 1,000 feet without reaching the base of the new red sandstone.

The Committee expresses a hope that the boring will be continued, as it may settle several questions not merely of local interest but of national importance: the water-bearing capabilities of the lower beds of the new red sandstone at great depths, the character of the coal-measures which undoubtedly underlie Liverpool, and the depth to them.

Amongst a large number of wells and borings alluded to in the Report was one at Burford, near Witney, in Oxfordshire, where the coal-measures with a coal-seam were found under the ooliths and triassic strata. Then water was also described as being present in the new boring of the Metropolitan Board of Works at Crossness.

Report on the Proposed Kentish Exploration, by R. A. C. Godwin-Austen, F.R.S.—During the past year nothing has been done to warrant an application for any portion of the grant at the disposal of the Kentish boring exploration, but results have been arrived at by private enterprises which indicate the information sought for—whether the palæozoic rocks underlie the chalk formation of the south-east of England as in Belgium and the south of France.

The finding of characteristic upper Devonian fossils in the boring at the corner of Tottenham Court Road and Oxford Street, seems to prove the supposition of the Coal Commission of 1871, that the line of the Thames Valley is that of the course of the palæozoic rocks.

One point remains. In what direction from the end of Tottenham Court Road may the mountain limestone and the coal measures be looked for? That their existence may be certainly inferred is shown by our acquaintance with the physical and geological history of the European area at those early times.

The so-called "Devonian" is but an early stage of the "mountain limestone series," consisting of (1) lower carboniferous or Devonian; (2) carboniferous limestone proper; (3) coal measures.

The Devonian beds, dipping at a high angle, is important; had they been lying flat, it might be inferred that the upper series were denuded, but there is every prospect that these beds may be found underlying the Devonian at some distance from the present bore-hole.

The Tottenham Court Road boring suggests that the Franco-Belgian palæozoic band with which their coal is associated, is continued under London, and within the very narrow limits here assigned (between Oxford Street and the Thames). Considering the vast importance of the discovery of productive coal-measures from the south-east of England westwards, the time has come when the results so nearly arrived at at Tottenham Court Road should be completed. Half the money spent on the Wealden boring at Netherfield would have settled the theoretical question, and even if they were of the same quality as the *Boulonnais* coal, the objections against the quality of that coal have been fully answered.

Major-Gen. Lane Fox, F.R.S., read the *Report of the Earth-*

works Committee: being an Account of Excavations in Cæsar's Camp, Folkestone, and also a paper on *Excavations at Mount Caburn, Lewes, Sussex.*—On Mount Caburn a large number of pits were dug. They were found to contain iron implements and arms entirely of a late Celtic type, and pottery of three descriptions. The fact of these being of a late Celtic type was proved by the getting of the specimens after a considerable amount of digging, and the finding of British tin coins, which were extremely rare, and which were known to be of a late Celtic type. Then excavations were made through the rampart in order to ascertain by means of relics found on the old surface-land what was the date of the first Roman camp. In the upper rampart a quantity of British pottery was found, showing the probability that it was erected at an early time. In the outer rampart pottery of a superior quality was found, by which means it appeared likely that the camp was first erected during the early British period, and was occupied up to the late Celtic period. Subsequently the adjoining camp of Ranscombe was cut through, and Samian pottery was found in the interior, showing that it had been occupied by the Romans; but in the body of the work nothing but British pottery was found, which proves that, though occupied by the Romans, it was not constructed by them. The Ranscombe Camp may, therefore, have been utilised by the Romans, as suggested by Horsfield a long time ago, during an attack upon Mount Caburn. As to Cæsar's Camp, it was ascertained that the whole of the relics found in the body of the several ramparts were of the Norman period, nothing Roman or earlier having been found anywhere.

Report of Committee, consisting of Prof. Harkness and Mr. William Jolly, on the Fossils of the North-West Highlands of Scotland, by Mr. W. Jolly, H.M.I.S., Secretary.—Since the last report one of the most active members of the Committee, the late Dr. James Bryce, has perished in the prosecution of his favourite science. For several years the Committee have explored the great limestone band running from Durness and Loch Eribol to Loch Kishorn, opposite Skye. The fossils discovered were obtained almost entirely from the Durness limestone, fourteen miles east of Cape Wrath, the only other place where they have been obtained in the limestone being at Inchnadamph, or Loch Assynt, in the west of Sutherland. The considerable collection of fossils made was placed in Dr. Bryce's care, but, on his unexpected decease, his collection was found to be more or less scattered and unmarked, and the Committee have as yet been unable to discover the Durness fossils, although careful search and inquiries have been made; they hope to be able to report their discovery to the next meeting.

Through the good offices of several friends, and particularly the Rev. W. C. Grant, a considerable collection has been made from Garveilan, near Cape Wrath, while in the quartzites of Lough Eribol numerous worm or annelid borings have been obtained, and Mr. Mackay has this year discovered hitherto undescribed fossil remains in the same quartzite. It is proposed to have these carefully examined, and to endeavour to ascertain their bearing on the general problem of the place and succession of rocks of the North-West Highlands.

The Committee consider it desirable they should be reappointed, with a grant at their disposal to prosecute their search for fossils, and also to examine and report on those already obtained.

SECTION A.—MATHEMATICAL AND PHYSICAL.

A New Determination of the Number of Electrostatic Units in the Electromagnetic Unit. (Telegram and letter to Sir William Thomson from Prof. W. E. Ayrton.)

The Red Sea, August 3, 1878

From Singapore I sent you the following telegram on July 13. "Kindly inform British Association that air-condenser measured magnetically and statically gives mean value ratio of these units (29·80) twenty-nine point eight nought ohms.; Foucault's velocity light."

In the autumn of this year I propose communicating a full account of this investigation to the Physical Society or the Society of Telegraph Engineers, or otherwise as you may advise; but I thought that as the British Association Committee had for so long busied itself with the determination of electrical units you might deem the result of this investigation of Mr. Perry and myself worthy of a preliminary notice at the meeting of this Association to be held this year at Dublin. Not being sure that I should arrive in Europe in time to reach Dublin at the commencement of the meeting, although I hope to be present during the last three or four days, I took the liberty of sending you the telegram quoted above.

The result we have obtained for "v" is the more interesting inasmuch as without any bias being given to any one of our experiments, the mean value is identically the same as that obtained by M. Foucault for the velocity of light, whereas all previous determinations of the number of electrostatic units in an electromagnetic unit have led to results differing considerably from Foucault's velocity.

It appeared to Mr. Perry and myself that the method best suited for the accurate determination of v consisted in measuring the capacity of an air-condenser (1), electromagnetically by the swing of the needle of a ballistic galvanometer; and (2) electrostatically by a measurement of the linear dimensions of the condenser, since the equation connecting these capacities

$$s = v^2 S$$

s being the absolute electrostatic capacity,
S ,, electromagnetic ,,

leads to an equation involving only the *square root* of a resistance.

Two difficulties of course presented themselves in this investigation, difficulties that it took us many months to overcome, labouring as we were under the disadvantage of experimenting in a country like Japan. They were—

(1) To obtain a large air-condenser of which the plates had sufficiently true surfaces that the electrostatic capacity could be accurately measured; at any rate when the plates were not nearer than half a centimetre to one another.

(2) To obtain a galvanometric arrangement of sufficient sensibility to measure the small capacity of such an air condenser and sufficiently ballistic that the air damping should be almost inappreciable.

A full description of the condenser we employed (and which had a guard ring, and all the different arrangements we could think of for obtaining accurate results) will accompany the account of the investigation to which I have referred. It is sufficient here to mention that the errors arising from the surfaces of the condenser plates not being true planes were practically eliminated by capacity experiments being made with successive adjustments of the condenser plates, a different set of points in the upper plate being each time brought to the fixed distance from the lower plate.

The arrangement of a ballistic galvanometer to fulfil the two conditions mentioned in (2) was very troublesome. I made several astatic needles none of which satisfied us, and we were beginning to fear my departure from Japan would necessitate the abandoning of the investigation. At last, however, an astatic combination containing forty small magnets (and of which a description will accompany the paper), gave satisfactory results, and I obtained three excellent sets of observations on June 18, June 23, and June 25, when my departure put an end to further investigation. The *mean* values obtained for "v" on each of these three days were—

29·74 ohms,	June	18
29·95 ,,	,,	23
29·72 ,,	,,	25

Final mean 29·80

It will be observed that the greatest difference between any one of the three daily means and the final mean, is only about half per cent. The final mean 298·0 million metres per second (and which represents the mean of ninety-eight discharges of the air-condenser) may, I think, be regarded as correct to, at any rate one per cent, and is exactly equal to M. Foucault's velocity of light.

In the astatic combinations I employed prior to June 18 I used eight needles, and weighted the lower set of needles with pieces of brass, so as to give it a barrel shape, but the results were unsatisfactory, as there was either not sufficient delicacy or else too much damping. Consequently all the numbers obtained prior to June 18 have been abandoned. On June 18 were

made the first set of experiments with the forty-magnet astatic combination, the idea of this arrangement being to make an approximately spherical mass of little magnets all slightly separated from one another, and all previously magnetised to saturation. As it would have been too difficult to make this entire sphere all of magnets I finished it off with segments cut from a little wooden sphere. But the half Napierian logarithmic decrement was 0·12095, and we thought this too high. Consequently, in the interval from June 18 to June 23, I took this astatic combination down, and replaced the segments of the wooden sphere by segments of a small leaden hemispherical *shell*. This diminished the half Napierian logarithmic decrement to 0·07825, and with a periodic time of 39·5 seconds I obtained most consistent results. But on the other hand the close agreement of the results obtained on June 18 and on June 25 leads one to conclude that the wooden segments were quite satisfactory, and that replacing them with the leaden shell was unnecessary.

The table at the end of this letter gives the value of the most important constants employed. The battery consisted of 382 perfectly new Daniell's cells in series, and the galvanometer had a resistance of 20,000 ohms. All resistances were compared with a new German silver wire box recently received from Messrs. Elliott, London.

The values obtained for v are (as far as I am aware) up to the present time as follows:—

MM. Weber and Kohlrausch	31·074	ohms.
Sir W. Thomson	28·2	,,
Prof. Clerk Maxwell	28·8	,,
Professors Ayrton and Perry	29·80	,,
Velocity of light, M. Foucault	29·8	,,

During the last twelve months we have been hard at work with the determination of the electromotive force of contact of metals and liquids, using a new apparatus. Some of the results are, we venture to think, most interesting—for instance, the electromotive force of contact of hot and cold mercury, *no other conductors being in contact with either mercury*; the electromotive force of contact of a cold metal and hot mercury, no third conductor being in contact with either, &c. The determination of the electromotive force of contact of the pairs of constituents of Mr. Latimer Clark's constant mercurous sulphate cell was most laborious, and occupied me weeks, in consequence of the difference of potential that exists between the body of the mercurous sulphate paste, and the layer of water that floats on the surface. However, a forlorn hope kept me hard at it, and I am glad to say at last I was successful in getting good results. We have gone over all the old ground that furnished the basis of our former paper, as well as much new ground.

Determination of the Number of Electrostatic Units in an Electromagnetic one.

Date.	Area of condenser plate in square centimetres.	Distance between the plates in centimetres.	Weight of the astatic combination in grammes.	Periodic time in seconds.	Half the Naperian logarithmic decrement.	Mean value of v.	Remarks.
June 18	1324·96	1·024	2·15	25·3	0·12095	29·74	Ninety-eight discharges of the air-condenser.
,, 23	1323·14	0·7728	3·4	39·5	0·07825	29·95	
,, 25	1323·14	0·7728	3·4	42·2	0·081865	29·72	

The distance between the upper condenser plate and the guard ring was slightly increased by diminishing the size of the plate to avoid the possibility of loss of electricity.

June 18.—The lower set of needles was weighted with segments cut from a small wooden sphere.

June 23 and after.—The lower set of needles was weighted with segments cut from a small leaden spherical shell.

Number of magnets in astatic combination 40.

Number of new Daniell's cells in series 382.

General Results of some Recent Experiments upon the Coefficient of Friction between Surfaces moving at High Velocities, by Douglas Galton, C.B., D.C.L., F.R.S., &c.—The author of this paper has been recently engaged in making some experiments upon the coefficient of friction when the surfaces in contact move at high velocities, in connection with the action of brakes in use on railways; and the results which have been arrived at appear to present some interesting features in respect of the laws which govern the coefficient of friction.

The experiments were made to ascertain the friction between the brake blocks and the wheels of a railway carriage.

The levers which move the brake blocks were fitted with dynamometers to show first the pressure which was applied to force the blocks against the wheel, and secondly, the force or tangential strain exerted between the wheel and the block when the latter is pressed against the wheel. The dynamometers used were adaptations of Richards's indicators which act by water pressure which transfers the pressure to cylinders fitted with pistons to which a pencil is attached, so as to register the pressure over a travelling sheet of paper, as is used with steam indicator diagrams. A dynamometer on a similar principle was attached to the draw bar so as to register the force exerted during the experiment in drawing the carriage.

The speed was also recorded on diagrams by means of the Westinghouse speed indicator, which also acts by water pressure and depends for its action on the speed of revolution of the axles.

The carriage or van fitted with the apparatus had two pairs of wheels; one pair of wheels was fitted with brakes whilst the other pair was free. A speed indicator was attached to each pair of wheels, so that the speed of the carriage could be ascertained at any time independently of the speed of the braked wheels.

To check the Westinghouse speed indicator two of Stroudley's speed indicators were also attached to the van, but these do not register automatically. The distribution of the weight of the van between the two pairs of wheels was obtained, as well as the weight of the wheels and axles themselves; and in order to ascertain the weight thrown on the braked wheels during the progress of the experiment, a dynamometer fitted to the springs of the van showed the weight at every moment carried on the unbraked wheels, from which information it was easy to deduce the weight on the braked wheels.

The apparatus was designed by Mr. Westinghouse, and constructed under his supervision by the Brighton Railway Company, through whose assistance these experiments were carried into effect.

The effect of applying the brake to the wheels is twofold. So long as the wheels to which brakes are applied continue to revolve at the rate of rotation due to the forward movement of the train, the effect of the blocks is to create retardation by the friction between the block and the wheel; but when the pressure applied to the blocks causes the friction to exceed the adhesion between the wheels and rail, the rotation of the wheels is arrested, and the wheel becomes fixed and slides on the rail, being held in its fixed position by the brake blocks.

Therefore the experiments give the coefficient of friction—1, between the brake blocks and the wheel, which is equal to

$$\frac{\text{the tangential force}}{\text{the pressure applied}};$$

2, between the wheel and the rail, which is the

$$\frac{\text{friction of the brake blocks}}{\text{weight upon the wheels}}.$$

It has been generally stated that there is no difference in the coefficient of friction observed in the case of bodies at rest, *i.e.*, in a condition of static friction, and the coefficient of friction in the case of moving bodies, *i.e.*, in a condition of kinetic friction; but Mr. Fleeming Jenkin, in his paper read before the Royal Society in April, 1877, upon the friction between surfaces moving at very low speeds, alludes to the fact that in all cases where a difference in the coefficient of friction is observed between static and kinetic friction, the static friction exceeds the kinetic.

Coulomb also points out, in his experiments that in the case of static friction, the coefficient of friction increased with the time during which the bodies had been at rest.

The experiments of Coulomb, Rennie, Morin, and Jenkin, were made with bodies moving at comparatively low velocities.

The following table shows the mean results obtained from a large number of the experiments made with the apparatus above described, upon the action between the cast-iron brake blocks and the wheels fitted with steel tyres:—

Average.		Coefficient of friction between cast-iron brake blocks and steel tyres of wheels.			
Miles per hour.	Feet per second.	At commencement of experiment, *e.g.*, to 3 seconds.	At from 5 to 7 seconds.	At 12 to 16 seconds.	At 24 to 25 seconds.
60	88	·062	·054	·048	·043
50	73	·100	·070	·056	—
45	65	·125	—	—	—
40	58	·134	·100	·080	—
30	43	·184	·111	·098	—
20	29	·205	·175	·128	·070
10	14	·320	·209	—	—
Under 5	7	·360	—	—	—
Results by former observers. Fleeming Jenkin—					
Steel on steel dry	·0002 to ·0086	·351 mean ·365 max.	—	—	—
Morin—					
Iron on iron		·44	—	—	—
Rennie— At pressure of 1·6 cwt. per square inch.					
Wrought iron on cast iron		·275	—	—	—
Steel on cast iron		·300	—	—	—

A limited number of experiments were made with wrought iron blocks upon the steel tyres, a mean of which gave the following result :—

Average.		Coefficient of friction between wrought iron blocks on wheels.			
Miles per hour.	Feet per second.	At commencement of experiment, to 3 seconds.	At from 5 to 7 seconds.	At 12 to 16 seconds.	At 24 to 25 seconds.
48	—	·110	—	—	—
31	—	·129	·11	·099	—
18	—	·170	—	—	—

The following table shows the result obtained by the sliding of the wheel on the rail—that is, a steel tyre on steel rails :—

Average.		Coefficient of friction between wheel on rail—steel on steel.			
Miles per hour.	Feet per second.	At commencement of experiment, to 3 seconds.	At from 5 to 7 seconds.	At 12 to 16 seconds.	At 24 to 25 seconds.
50	—	·04	—	—	—
45	—	·051	—	—	—
38	—	·057	·044	·044	—
25	—	·080	·074	—	—
15	—	·087	—	—	—
10	—	·110	—	—	—

The general results of these tables show that the coefficient of friction between moving surfaces varies inversely in a ratio dependent upon the velocity at which the surfaces are moving past each other; probably the expression would be of the form of $\frac{a}{b+v}$.

The coefficient of friction, moreover, at these velocities becomes smaller also after the bodies have been in contact for a short time. That is to say, the longer the time the surfaces are in contact, the smaller apparently does the coefficient of friction become. This result appears more marked in the case of cast-iron blocks than of the wheel sliding on the rail. This effect, however, does not appear to be unnatural, as the friction developes heat, and the consequent expansion tends to close up the pores and to make the heated surface a more united surface than the colder surface; besides which it is probable that, in the act of rubbing, small particles may be detached which may act as rollers between the surfaces.

It will also be observed that the coefficient of friction between the cast-iron block and the steel tyre is much larger than that between the steel tyre of the wheel and the rails, which were also generally of steel. As has been above-mentioned, the sliding of the wheel on the rail takes place when the friction of the brake blocks is greater than the adhesion between the wheel and the rail, which is due to the weight upon the wheel. This was found to amount generally to about 24 to 28 per cent. of the weight. The influence which these results have upon brakes for railway trains may be briefly summed up as follows :—

In order to produce a given result at different velocities the pressure applied to the brake blocks must increase in the proportion shown by the coefficient of friction. Thus at fifty miles an hour the pressure required to make one pair of wheels slide on the rail was nearly 27,000 lbs., whilst at twenty miles an hour a pressure of about 10,300 lbs. was found sufficient to obtain the same result. The strain on the draw-bar showed that the retarding force or the tangential strain between the brake blocks and the wheels followed very nearly the same law of variation; that is to say, in order to produce a degree of friction on the wheel at fifty miles an hour which shall exert a retarding force on the train equal to that at twenty miles an hour the pressure applied to the brake blocks at fifty miles an hour must be nearly $2\frac{1}{2}$ times as great as that required at twenty miles an hour, and a still greater pressure is required for higher velocities. Therefore whilst a comparatively low pressure would make the wheel slide at low velocities, it was difficult to obtain any sufficient pressure to make the wheel slide at velocities over sixty miles an hour.

A satisfactory brake, therefore, should be capable of bringing on a very high pressure almost instantaneously, and then the pressure should be gradually reduced as the train comes to rest.

The figures given in the above tables must at present be accepted as only provisional until an accurate mean has been obtained from the diagrams, which are not yet all worked out. But it may be assumed as an axiom that for high velocities a brake is of comparatively small value unless it can bring to bear a high pressure upon the surface of the tyre almost instantaneously, and it should be so constructed that the pressure can be rednced in proportion as the speed of the train is reduced so as to avoid the sliding of the wheels on the rails.

I must add that these experiments were made upon the London, Brighton, and South Coast Railway, who, through their able general manager, Mr. Knight, and locomotive engineer, Mr. Stroudley, gave every assistance in the construction of the van and the running of the train. The apparatus was mainly devised by Mr. Westinghouse and the experiments were carried on under his immediate supervision. The earlier experiments were also made with the assistance of Mr. Horace Darwin.

SECTION B.—CHEMICAL SCIENCE.

On a Simplification of Graphic Formulæ, by Oliver J. Lodge, D.Sc.—In the graphic formulæ of a compound the elements were ordinarily represented by their chemical symbols (capital letters), and the connection between the atoms was represented by straight lines joining the letters. Graphic formulæ were of most use in organic chemistry, where the principal compounds consisted only of the elements C, H, O, N, whose atomicities were 4, 1, 2, and 3 or 5 respectively. In any formulæ, therefore, four bonds always radiated from the letter C : N was the meeting place of three or five bonds, according to circumstances; two bonds met at each O, and a single bond terminated at every H. Supposing that the letters were omitted and the bonds joined together, the position of the atoms would still be apparent as the meeting-place of a definite number of bonds, and therefore the letters were unnecessary. The simplification he proposed, then, was the omission of the usual symbols used to denote the atoms, and the joining of the bonds in such a way as clearly to define the atomicities, and therefore the natures of the several atoms. Formulæ so drawn became reduced to a sort of geometrical diagram, and conversely any geometrical curve represented some real or imaginary chemical compound.

Abstract of a Paper on the Action of Heat upon the Selenate of

Ammonium.—Dr. Edward W. Davy, Professor of Forensic Medicine, Royal College of Surgeons, Ireland, read, on behalf of his colleague, Dr. Charles A. Cameron and himself, a paper containing the results of some observations which they had conjointly made on the action of heat upon the selenate of ammonium, from which they have shown that, when that salt is exposed to heat, it first resolves itself into ammonia and an acid selenate of ammonium (a hitherto undescribed salt), and that this latter, on the application of a higher temperature, breaks itself up into water, selenium, selenious anhydride, and nitrogen, thus showing that in the first stage of the decomposition of this selenate by heat it resembles the sulphate of ammonium in furnishing an acid salt under the same circumstances, but that in the separation of selenium in the second stage of the process there is no analogy between the sulphate and selenate of ammonium.

Abstract of a paper *On the Action of Chlorine upon the Nitroprussides.*—Dr. Edmund W. Davy read a paper *On the Action of Chlorine upon the Nitroprussides*, an interesting class of compounds obtained by the action of nitric acid on the soluble ferro- or ferri-cyanides, which were first investigated by Dr. Lyon Playfair, several years ago. Dr. Davy has ascertained that the statements which exist in the different standard works on chemistry as to chlorine having no action on those salts, is incorrect, at least as regards several of the nitroprussides which he has made the subject of investigation, for he has found that some of them are immediately, and others, after long exposure, more or less, acted on by that substance, even when they are excluded from the light. When, however, they are subjected to the combined action of chlorine and the sun's rays, they are soon completely decomposed, the principal products being an oil-like matter, which agrees in its properties with the substance known under the name of chlorocyanic oil, ferric chloride, hydrochloric acid, and a chloride of the metallic base of the salt employed.

The following nitroprussides, viz., those of potassium, sodium, barium, calcium, zinc, iron, and silver, were found to be those decomposed when exposed to the action of chlorine and sunlight; and it is probable that other nitroprussides would be similarly affected. The only one of those salts, however, which the author has observed resisting this action is that of copper, which has remained apparently unaffected after some weeks' exposure to its influence.

On the Spectrum of Chlorochromic Anhydride, by Dr. Johnstone Stoney and Prof. Reynolds.—The authors exhibited and described the spectrum produced by the absorption of the vapour of chlorochromic anhydride. This spectrum is of peculiar interest from its having supplied information as to the duration and character of the motion of the molecules of the gas which produce it. The spectrum consists of lines nearly equally spread, but of various intensities. From the position of the lines, of which 105 have been examined, it has been ascertained that they are all to be referred to one motion in the molecules of the gas of which they are all harmonics or quasi-harmonics, and which, on the supposition that they are harmonics, is repeated 810,000,000,000 every second in each molecule, and from the succession of intensities it may be surmised that this motion is in some way related to that of a particular point in a violin string vibrating under the influence of the bow, viz., a point nearly, but not quite, two-fifths of the string from the one end.

On a New Method of Alkalimetry, by Dr. Louis Siebold, F.C.S.—The plan recommended consisted in the reverse application of Liebig's process for estimating hydrocyanic acid, and was based on the fact that the volumetric determination of an alkaline cyanide by means of silver nitrate was in no wise affected by the presence of free hydrocyanic acid. From the volume of silver solution used the quantity of alkali might be as readily calculated as that of the cyanogen. If the applicability of this process for alkalimetric purposes were confined to the estimation of caustic alkalies nobody would, in Dr. Siebold's opinion, think of using it in preference to the process commonly adopted; but he wished to show that it might with great advantage be applied to the determination of alkaline carbonates. From ·5 to one grain of the potassium or sodium carbonate should be dissolved in about 100 c.c. of distilled water, the solution mixed with an excess of hydrocyanic acid (10 to 20 c.c. of acid of Scheele's strength), and then decinormal solution of silver nitrate added from a burette until a permanent opalescence is produced, the reaction occurring according to the following equation:—

$$K_2CO_3 + 2HCy + AgNO_3 = KAgCy_2 + KNO_3 + CO_2.$$

Whereas under ordinary circumstances hydrocyanic acid was incapable of decomposing alkaline carbonates it effected a complete decomposition in the presence of silver nitrate. The mixture did not require boiling, and the whole operation might be performed within a few minutes. If, after the end of the titration, the mixture were boiled, and the addition of decinormal solution of silver nitrate proceeded with, this time using potassium chromate as an indicator, the volume of silver solution required to insure complete precipitation of the silver cyanide would be exactly equal to that used in the first titration. This second reaction might then, if desired, be used as a check on the determination. In the presence of chloride the volume of silver solution used in the second experiment would be greater than that used in the first, the difference between the two being exactly that required to precipitate the chloride. In this manner a determination of the chloride might be readily combined with that of the alkaline carbonate. The following results were quoted to show the accuracy of the process:—

Pure Potassium Carbonate.

Amount taken.	Amount found.
0·5850	0·5851
0·1670	0·1672
0·8775	0·8779

In mixtures of pure potassium carbonate and sodium chloride—

Amount taken.	Amount found.
K_2CO_3, 0·2000	0·2005
NaCl, 0·0680	0·0683
K_2CO_3, 0·9750	0·9750
NaCl, 0·1825	0·1830

On the Estimation of Mineral Oil or Paraffin Wax when mixed with other Oils or Fat.—William Thomson, F.R.S.E., read a paper on this subject. He said that mixed oils were now often used for lubricating purposes, and a common mixture, composed of mineral oil with some animal, vegetable, or fish oils, was now extensively used, and it was an important point to be able by analysis to determine the amount of mineral oil which such mixtures contained, and as he could find no published process to effect this, he devised, after much work, the following, which he found by repeated tests to give very accurate results:—He boiled some of the sample with an alcoholic solution of caustic soda, which converted all the animal, vegetable, or fish oils into soap. This was then mixed with sand, evaporated to dryness on the steam bath, the residue placed in a bottle washed with petroleum spirit, and distilled at a temperature under 190° F. This dissolves out the mineral oil, leaving the soap insoluble. The spirit is now distilled off from the spirit solution of mineral oil in a large flask, and after thus evaporating off the bulk of the spirit, the concentrated solution is transferred to a smaller flask with a hole blown in its side, into which is fitted a cork carrying a thermometer and glass tube; the thermometer should touch the liquid, going nearly to the bottom of the flask. It is placed on a sand bath and heated at a temperature not exceeding 220° F., and dry air blown into the flask through the tube in the cork, to remove the last trace of spirit, and the residue of mineral oil weighed and calculated on the weight of the original mixed oil taken.

On some Double Salts of Glucinum, by Prof. Emerson Reynolds.—This communication contained an account of some investigations upon some compounds of the rare metal glucinum, and demonstrated by several interesting experiments how the author had succeeded in obtaining the metal in a state of purity by the use of the double fluoride of glucinum and potassium. A second salt, double chloride of platinum, and glucinum, had also been prepared in an exceedingly pure crystalline form. Dr. Reynolds stated that he had also made experiments with a view to determine the specific heat of glucinum, which he fixed at ·642, and this multiplied by the probable atomic weight of the metal—namely, 9·2—gave 5·90, a result fairly in accordance with the law of Dulong and Pettit.

Summary of Investigations in the Pyridine Series, by Dr. W. Ramsay.—These bases, which possess the general formula, $C_nH_{2n-5}N$, are tertiary bases. They form an additive product with iodides of alcohol radicles, of which a good example is C_5H_5N, CH_3I, best named pyridine methyl-iodide, as it resembles a salt in its constitution. They are not attacked by nitrous acid, and the cyanate, when heated, undergoes no molecular change, but merely splits up into the base, and the usual polymer of cyanic acid, cyanuric acid.

Picoline, C_6H_7N, on oxidation, yields a dicarbopyridenic acid, $C_7H_5NO_4$, which, on distillation with soda-lime, decomposes into pyridine, C_5H_5N, and carbonic anhydride, $2CO_2$. It has therefore the structural formula $C_5H_3N(2OOH)_2$. Attempts to prepare lutidine, C_7H_9N, from the aldehyde of that acid, as well as by the reaction

$$C_5H_3N(COOCH_3)_2 = C_7H_9N + 2CO_2,$$

failed, owing, in the first instance, to the small yield of aldehydes, and, in the second, to the total decomposition of the product into pyridine, carbonic anhydride, and carbon.

In spite of the failure of these attempts the author regards it as probable that picoline is methyl pyridine; entidine, dimethyl pyridine, from the following consideration:—The amount of heat evolved in the formation of these bases is probably very high. That heat, added to the amount evolved by the combination of the base with an acid, is likely to be greater than the total number of heat-units evolved by the oxidation of the base; hence these bases are unoxidisable in acid solution. But when oxidised in alkaline solution, the amount of heat evolved by oxidation is supplemented by that arising from the combination of the resulting acid with the alkali, and then exceeds the heat evolved during the formation of the base. The presence of nitrogen therefore gives great stability to the molecule, and prevents the methyl groups from being oxidised to carboxyl-groups, as in the case of toluol, xylol, &c. At least three isomeric acids of the general formula $C_7H_5NO_4$ have been discovered, and it is probable that as many as six are capable of existence. These the author has named α, β, and γ, dicarbopyridenic acids. The α-acid is obtained by oxidising picoline or entidine, and the last two from entidine.

An attempt to pass from furfurol to pyridine by the series of reactions—

$C_5H_4O_2$, Furfurol, $C_5H_6O_2$, Furfuryl alcohol, C_5H_5OCl, Furfuryl chloride, $C_5H_5ONH_2$, Furfuryl amine, C_5H_5N, Pyridine,

was unsuccessful, owing to the instability of furfuryl chloride.

From the stability of the pyridine group, and the instability of the furfurol group, the author regards it as probable that the constitution of the former is best represented by a closed, and that of the latter by an open, chain.

On Some of the Derivatives of Furfurol, by Dr. W. Ramsay.—It was found impossible to prepare furfuryl chloride by the action of phosphoric chloride, or of hydrochloric acid gas on furfuryl alcohol, $C_5H_6O_2$, owing to a complete decomposition of the organic matter, with separation of carbon.

Furfurine, prepared by heating furfuramide and possessing the same formula, $C_{15}H_{12}N_2O_3$ unites with methyl iodide, forming the hydriodide of methyl-furfurine; this salt, on treatment with ammonia, deposits the base $C_{15}H_{11}(CH_3)N_2O_3$, as a viscous oil, insoluble in water, but soluble in alcohol. The base again unites with methyl iodide, giving the hydriode of dimethyl furfurine, $C_{15}H_{10}(CH_3)_2N_2O_3HI$, which is also decomposable by ammonia with liberation of the base, dimethyl furfurine $C_{15}H_{10}(CH_3)_2N_2O_3$. This base appears also to be capable of union with methyl iodide.

Furfurine, then, appears to be a secondary base containing two atoms of hydrogen replaceable by methyl. Whether more can be replaced the author was unable to decide, as the loss by repetition of the operation was very considerable.

On the Thetines, by E. A. Letts.—Prof. Letts' experiments were undertaken as a sequel to the research of Prof. Crum Brown and himself, on dimethyl thetine and its compounds, and with a view to the thorough investigation of the thetines as a group—the phenomena attending their formation—the action of heat and oxidising agents on them, and the difference in their properties as the series is ascended. Incidentally Prof. Letts has also studied the action of bromacetic acid on certain sulphides of hydrocarbon radicals and the action of bromacetic and iodacetic ethyl ether on sulphide of methyl.

Notes on Aluminium Alcohols, by Dr. Gladstone and Mr. Tribe.—In 1876 the authors described the joint action of aluminium and iodine on alcohol, and two aluminium ethylates resulting from it. They now showed that a similar reaction takes place with methylic alcohol, especially when the aluminium is rendered more powerful by conjunction with deposited platinum; and that an analogous body is still more readily formed from amylic alcohol. These two substitution-products had not yet been prepared in a pure condition, but the authors had succeeded in preparing the butylic compound in a satisfactory manner. This aluminic butylate is a solid body at the ordinary temperature, but melts when heated, and is capable of distillation. It is very soluble in anhydrous ether or benzole, from which it separates on evaporation without crystallising. It is decomposed by water, butylic alcohol and alumina being produced. Its composition was found to be $Al_2(C_4H_9O)_6$. There is also evidence of an intermediate compound, soluble in water, which is probably homologous with the aluminic iodo-ethylate $Al_2I_3(C_2H_5O)_3$.

On the Amounts of Sugar contained in the Nectar of Various Flowers, by Alex. S. Wilson, M.A., B.Sc.—Nectar, the sweet-tasting fluid found within the cups of insect-fertilised flowers is of use to the plant by affording an inducement whereby insects are attracted to visit the flowers. By this means cross-fertilisation is effected, as bees, butterflies, and other insects bring with them pollen from other flowers adhering to their bodies which they deposit on the stigmas. Mr. Darwin has shown experimentally what an additional amount of vigour is thus conferred on the resulting seeds in contrast with the degenerating effect of continuous inbreeding. Very often this sweet fluid is exuded from special glands, but in other cases from portions of the flower that do not seem to have been specially adapted for this purpose. Morphologically nectaries may represent very different structures, but not unfrequently they are of the nature of an aborted organ such as a petal or stamen. It is a disputed point among biologists whether this saccharine matter is a true secretion or simply an excretion of effete matter from vegetable cells—a bi-product of the chemical changes taking place within these cells. Nectar is of course the source whence the bee derives honey, but it also affords sustenance to many different kinds of insects as well as humming-birds. The bright colours of flowers as shown by Sir John Lubbock's experiments serve for the guidance of insects to them, and the odours which they emit fulfil the same end. The markings on a flower's petals, too, always converge towards the nectar. The importance of these guides to insects will be apparent from the following estimations which show how indispensable it is that as little time as possible should be lost by an insect while collecting honey. It must be remembered also that in order to protect the nectar from rain it is usually contained in the least accessible part of the flower. The formation of nectar is observed to take place most freely in hot weather, so great, however, is the economy of the plant that it is only formed at the time when insects' visits would be beneficial, *i.e.*, when the anthers are shedding their pollen or when the stigma is mature. Biologists believe that the visits of bees, butterflies, and other insects have in past time exercised an important influence in modifying the size, shape, colour, &c., of flowers. The following determinations are of interest as showing to what extent this action goes on and as a help towards ascertaining the value of this factor:—

Sugar in Flowers.

		Total.	Fruit.	Cane.
1. Fuchsia, per flower	mmg.	7·59	1·69	5·9
2. Everlasting Pea, per flower...	,,	9·93	8·33	1·60
3. Vetch, per raceme	,,	3·16	3·15	·01
4. ,, single flower ...	,,	·158	·158	—
5. Red Clover, per head... ...	,,	7·93	5·95	1·98
6. ,, ,, floret... ...	,,	·132	·099	·033
7. Monkshood, per flower ...	,,	6·41	4·63	1·78
8. *Claytonia almoides*, per flower	,,	·413	·175	·238

Approximately, then, 100 heads of clover yield ·8 grm. sugar or 125 give 1 grm. or 125,000 1 kilo sugar, and as each head contains about sixty florets it follows that 7,500,000 distinct flower tubes must be sucked in order to obtain 1 kilogrm. sugar. Now as honey roughly contains 75 per cent. of sugar, 1 kilo. is equivalent to 5,600,000 flowers in round numbers, or say two-and-a-half millions of visits for 1 lb. of honey. Another point worthy of note in these results is the occurrence of what appears to be cane-sugar, and that in the case of fuchsia in the proportion of three-fourths of the whole. This is remarkable, as honey is usually supposed to contain no cane-sugar, its presence being generally held as certain evidence of adulteration. The question therefore arises whether this change, which occurs while the sugar is in the bees' possession, be due to the action of juices with which it comes in contact while in the honey-bag, or whether on account of the acid reaction of nectar it may not take place spontaneously.

Notes on Waters from the Severn Tunnel Springs, by W. Lant Carpenter.—The plans for the construction of this tunnel had

been described by Mr. Charles Richardson, its engineer, to Section G, at the Bristol meeting in 1875. In June, 1878, the trial heading had been driven more than half-way under the Severn, and the most difficult part had been overcome. In the opinion of the engineer, the water in the tunnel springs—one of which had yielded 300 gallons per minute—was not Severn water, but derived from springs in the pennant and other strata, and the salt from the occasional beds of salt in the Bristol coal-field. The author had analysed water from four springs, and compared the results with those from Severn water at various times of the tide, and with water from wells on the English side sunk forty feet through the alluvium. The results were arranged under the heads of specific gravity, total solid residue, and total chlorine per cent. The author found it difficult to avoid the conclusion that the water in the springs was chiefly Severn water.

SECTION C.—Geology.

The Geological Relations of the Atmosphere, by T. Sterry Hunt, LL.D., F.R.S.—The author began by noticing the inquiries of Ebelmen into the decomposition of rocks through the influence of the atmosphere, resulting in the fixation of carbonic acid and oxygen, and discussed the question at length, with arithmetical data. He inquired farther into the fixing of carbon from the air by vegetation, with liberation at the same time of oxygen both from carbonic acid and from the decomposed water, the hydrogen of which, with carbon, forms the bituminous coals and petroleums. It was shown that the carbonic acid absorbed in the process of rock-decay during the long geologic ages, and now represented in the form of carbonates in the earth's crust, must have equalled, probably, two hundred times the entire volume of the present atmosphere of our earth. This amount could not of course exist at any one time in the air; it would, at ordinary temperatures, be liquefied at the earth's surface. Whence came this vast quantity of carbonic acid, which must have been supplied through the ages? The hypothesis of Elie de Beaumont, who supposed a reservoir of carbonic acid stored up in the liquid interior of the planet, was discussed and dismissed. The gas now evolved from the earth's crust from volcanic and other vents was probably of secondary origin, and due to carbonates previously formed at the surface.

The solution of the problem offered by the author is based upon the conception that our atmosphere is not terrestrial, but cosmical, being a universal medium diffused throughout all space, but condensed around the various centres of attraction in amounts proportioned to their mass and temperature, the waters of the ocean themselves belonging to this universal atmosphere. Such being the case, any change in the atmospheric envelope of any globe, whether by the absorption or the disengagement of any gas or vapour, would, by the laws of diffusion and static equilibrium, be felt everywhere throughout the universe, and the fixation of carbonic acid at the surface of our planet would not only bring in a supply of this gas from the worlds beyond, but by reducing the total amount of it in the universal atmosphere, diminish the barometric pressure at the surface of our own and of all other worlds.

This conception of a cosmical atmosphere, of which our own forms a part, is not new, but was put forth by Sir William R. Grove in 1843, and is developed in the very learned and ingenious work of Mr. Mattieu Williams, on "The Fuel of the Sun," and has lately been noticed by Dr. P. M. Duncan in its geological bearings. Ebelmen, in 1845, pointed out that the greater weight of an atmosphere charged with carbonic acid would increase the temperature due to solar radiation at the earth's surface, and greatly modify atmospheric phenomena.

Tyndall, by his subsequent researches on radiation, showed that certain gases, in amount too small to affect considerably the barometric pressure, might influence powerfully climatic conditions, and suggested that in the former presence in the atmosphere of moderate quantities of a gas like carbonic acid, might be found a solution of the problem of the climates of former geologic ages. According to the author, the amount of this gas, which, since the advent of life on our earth, has been subtracted from the universal atmosphere, although it may not have sufficed to diminish by more than a small fraction the pressure at the earth's surface, would account for all the conditions of geological history so far as temperature and climate are concerned.

He maintains that while we have evidence of a warm or subtropical climate prevailing over the Arctic regions from the carboniferous down to lower cretaceous times, and a gradual refrigeration up to the temperate climate of the miocene age, we had for the first time in the pliocene age the evidence of Arctic cold, which, with some variations, has continued until now. Since that date geographical variations have caused, and may again cause local climatic changes of considerable magnitude. But no such changes could permit the existence over continental areas within the Arctic circle, of such tropical vegetation as we know to have once flourished there. Geographical changes, as J. F. Campbell, Dawson and others have so well pointed out, might lift large areas into the region of perpetual frost, and thus give rise to local glacial phenomena, and may, moreover, account for considerable local climatic variations at the sea-level since the pliocene age. We cannot, however, account in this way for the warmer climates of previous ages, but must seek for their cause in the former constitution of the atmosphere.

Touching the suggestion that former climatic changes were due to a displacement of the earth's axis of rotation, the author expressed the opinion that it is irreconcilable with the fact long ago insisted upon by him that "the direction of the Arctic currents, which are guided by the earth's rotation, appears, from the distribution of marine sediments, to have been the same since very early periods." Dawson has reinforced this argument by recalling the fact that the southward migration of successive floras shows, in like manner, that from the Devonian age the general courses of oceanic currents, and consequently the position of the earth's axis, have not changed.

On the Filtration of Sea-water through Triassic Sandstone, by Isaac Roberts, F.G.S.—Mr. Roberts stated that he was led to investigate the effects produced on sea-water by filtration, in consequence of the constantly-increasing salinity of the water drawn from several wells in Liverpool which are sunk below the sea-level in the Bunter sandstones of that locality. He found that one of the wells, which he selected as the type of the rest, yielded water which increased in salinity at the rate of 4·91 to 5·81 per cent. annually, and inferred that the sandstone rock had the power of removing salts out of sea-water. To prove this he filtered sea-water through blocks of the sandstone, and found the inference to be greatly borne out by the results of his experiments. Two cubic feet of the stone removed from the first filtrate of 3½ fluid ounces of the water 80·8 per cent. of the salts held in solution, and each measured quantity of four ounces, which were afterwards filtered through, regularly showed an increase of the salts in solution, until 93½ fluid ounces had filtered through the stones. Then these ceased to be operative as filters, and the waters passed through unchanged. After allowing the stones to dry he passed spring water through them, and found that the salts which they had taken up were again removed and washed out, thereby showing the action to be mechanical.

New Geological Map of India.—Mr. W. Ball, M.A., presented and explained a new geological map of India which will shortly be published, with a manual. He mentioned that there were 8,000 square miles of coal measures in India. It was of an inferior character, and was unfortunately in the most inaccessible part of the country.

Mr. W. H. Baily, M.R.I.A., F.G.S., read a paper *On some Additional Labyrinthodont Amphibia and Fish from the Coals of Jarrow Colliery, near Castlecomer, County Kilkenny*.—These fossil remains, which were found below a bed of coal three feet in thickness, were all impressed on the true coal, and had, in fact, turned into carbon. One of them, which was almost perfect—*Megalichthys hibberti*—was three feet seven inches in length. This one locality had yielded a larger number of these fossils than all the other coal-fields in Europe.

On the Ancient Volcanic District of Slieve Gullion, by Joseph Nolan, M.R.I.A., Geological Survey of Ireland.—Slieve Gullion is a mountain situated some few miles north of Dundalk, west of the hilly country lying between the bays of Dundalk and Carlingford. The rocks which mainly compose it are of a plutonic character, consisting of dolerites and elvanites, and have been erupted through granite of lower Silurian age, probably about the close of the palæozoic epoch. On the west and south of the mountain the elvanite forms a dyke-like ridge when it changes from a granitoid rock to a felstone porphyry. Simultaneously with this change, suggesting conditions of less intense heat and pressure a remarkable fragmentary rock makes its appearance. It is here almost altogether composed

of granite fragments, farther south a mixture of granite and slate, and at the south-east almost exclusively of slate *débris*. At all these places it is so intimately associated with the porphyry that any demarcation between them is impossible, and there can be no doubt that it is due to the broken and disintegrated crust, shattered by the force of the volcanic explosions. That the eruption differed from volcanic explosions of the normal character is evident from the absence of fragments of lava or scoriæ. It was in fact entirely aëriform, the volcanic activity having evidently been subdued by the sudden dissipation of the elastic gases in that part of the dyke which alone reached the surface, aided by the immense weight of displaced materials falling back into the gulf.

The president remarked that the paper opened up several points of interest, especially in illustrating the passage of granitic into volcanic rocks; and in bearing out the views of Prof. Judd on these points, giving undoubted proof of the change from granitoid rock into one by which its protrusion has produced mechanical accompaniments.

On Some Fossils from the Northampton Sands, by John Evans, D.C.L., LL.D., F.R.S.—These fossils from the ironstone beds of Durton, near Northampton, are casts of lithodomous borings originally made in lumps of coral, impressions of which they still bear on their outer surface. In the interior the presence of the shells is still to be traced. Their history appears to be that the cavities were first filled with limonite, subsequently converted into carbonate of iron, and eventually hematite. Last of all the inclosing coral has been entirely removed by the infiltrations of water charged with carbonic acid. This last process has probably taken place since the emergence of the beds from below the sea-level. The Northampton Sands have been fully described by Mr. Samuel Sharp, F.G.S., in the *Quarterly Journal* of the Geological Society.

A paper was read by Mr. H. W. Baily, F.G.S., *on a New Star-fish from Lower Silurian Caradoc Strata, Co. Wexford, and some New Carboniferous Limestone Mollusca from the County of Limerick.*—The starfish, of which a number of very perfect specimens had been collected through the exertions of Mr. H. Kinahan, during the progress of the Geological Survey, was named by Mr. Baily *Palasterina kinahana*. The carboniferous limestone fossils were all molluscan shells, principally cephalopods, belonging to *Nautilus goniatites* and *Orthoceratites*, with a few bivalves and univalves belonging to *Cardiomorpha macrocheilus*, &c.

On the Saurians of the Dakota Cretaceous Rocks of Colorado, by Prof. E. D. Cope.—This paper was illustrated by crayon-drawings of the bones, of the natural size, which well illustrated the gigantic proportions of the several species. The first described was the *Camarasaurus supremus*, Cope, of one individual of which a large number of bones have been found. The vertebræ are opisthocœlous, and their centres are hollow, with the internal cavity divided into two chambers by a septum. The caudal vertebræ and limbs are solid, or nearly so, and the neck is long. The scapula is enormous, measuring 5½ feet in elevation, so that the fore limb was probably as long as or longer than the hind limb. There are four sacral vertebræ and the femur is six feet in length. The elevation to the top of the head was probably 26–28 feet. The description of three species of *Amphicœlias* followed. The type *A. altus*, Cope, presents the same kind of chambers of the vertebral cerebrum as *Camarasaurus*, but the articular extremities are both concave. Both genera possess a new articular element, called by Prof. Cope a hyposphen. This is the reverse of the zygosphen, standing below the postzygapophysis, and looking upwards and outwards. The neural spine of *Amphicœlias* is anteroposteriorly placed, that of *Camarasaurus* transversely, and in the latter, in the dorsal region, it is divided to the base, and the halves separated. *A. altus* was as large as *C. supremus*, the femur measuring six feet and the dorsal vertebra 3·5 feet elevation.

Prof. Cope then indulged in some reflections as to the habits of the genera so remarkable for their thin-walled dorsal and cervical vertebræ. He was of the opinion that the caverns were not filled with cartilage but with diverticula from the lungs or other air-cavities. Prof. Cope advanced the hypothesis that the species of *Camarasaurus* and *Amphicœlias* were dwellers in water of sometimes considerable depth, where they walked on the bottom, and browsed on the algæ and sometimes aërial vegetation growing on the coast. The long fore-limbs and long neck of *Camarasaurus* were further evidence that the animal reached upwards for food, as the giraffe, or for air when submerged.

Prof. Cope concluded by remarking that the Dakota epoch had been referred to the middle of the European cretaceous series on the evidence of the mollusca and plants, but now the vertebrata parallelise it with some portion of the Jurassic series in the wide sense.

Prof. Hull, F.R.S., gave a sketch of the geology of the environs of Dublin.

On the Progress of the Geological Survey of Ireland, by Prof. Hull, M.A., F.R.S., Director (communicated with the sanction of the Director-General).—The author gave a short account of the origin and progress of the Geological Survey from its commencement in 1832, under the late Gen. Portlock, R.E., down to the present day, stating that the whole country south of a line drawn roughly from Larne on the coast of Antrim to Sligo had been surveyed, while 160 sheets of the geological map, on a scale of one inch to the statute mile, had been published.

Along with the maps there had been issued seventy-eight separate explanatory memoirs, describing the structure and palæontology of 126 sheets. It had also been found necessary to revise the geology of the Leicester and Tipperary coal-fields, the carboniferous trap-rocks of county Limerick, and the south-east portion of the country, including parts of Wicklow and Wexford. The coal-fields of the north of Ireland had also been surveyed, and published in the maps both of the "6-inch" and "1-inch" scales; and it was also intended that the districts of county Antrim containing the pisolitic iron ores should be illustrated by maps on both these scales. The district still remaining to be examined includes the greater portion of Donegal, Tyrone, Sligo, Fermanagh, and Antrim.

On the conclusion of Prof. Hull's paper *On the Progress of the Geological Survey*, the President said they must all congratulate the chief of the Geological Survey in Ireland on the progress that had been made, but he hoped that however long he might live in the occupation of that high post, the survey would never be absolutely completed. It was quite right to re-examine surveys that had been made; for, from time to time discoveries were made which threw a further light on the geological views of the district, and which must be taken into account before an absolutely complete survey could exist. Therefore, though the survey had progressed so much, still he hoped it would be long before it was completed. He wished to say one word which appeared to him of importance, not only to the Geological Survey of Ireland, but of England also. The object of these surveys was to make the public at large thoroughly acquainted with the geology of the country in which they resided. Maps were carefully drawn and memoirs published from time to time in illustration of the maps; but, unfortunately, so far as the diffusion of knowledge was concerned at the present time, not owing to any prohibition of the Geological Survey, but owing to some mistaken view on the part of the Treasury, prohibitive prices were placed upon the geological memoirs. He had seen small pamphlets priced at 16*s*. or 17*s*., though these pamphlets were printed and published at the public expense for the benefit of the public. He held in his hand a very small pamphlet, which was published at 9*s*. He did not think a false economy of this kind ought to be suffered to go on without a protest on behalf of those who were interested in geological progress. He therefore felt it right to make these remarks, in the hope that possibly this Association or some other learned bodies might take steps to bring this matter under the consideration of the Treasury, and point out how with the one hand they were lavishly spending money for the advancement of geological knowledge, and with the other withholding it from the public.

Dr. Sterry Hunt said that in the State of Pennsylvania the reports and memoirs were printed and stereotyped, and then offered to the public for the mere cost of printing and paper. The memoir on the table priced at 9*s*. would be sold there for 2*s*.

Mr. Tiddeman said, with respect to the prices of the geological publications, he should be sorry any one should think that the Survey was responsible. It could not be more disagreeable to any than to the Survey themselves to find that their labour was almost entirely thrown away by reason of the high prices of the publications when they came out in the miserable manner in which they were produced, and which would almost disgrace a fifth-rate publishing firm.

On the Influence that Microscopic Vegetable Organisms had in the Production of some Hydrated Iron Ores, by M. Alphonse Gages, M.R.I.A.—In the tanks of the Royal College of Science, Dublin, there are considerable deposits of hydrated peroxides of

iron formed by the agency of Penecilliæ, which accumulate in large sponge-like masses so as to intercept the flow of the water. The author points out the connection between such deposits and iron-ore formations.

Notes on some New Fossils "Eribollia Mackayi," from the Quartzites of Loch Eribol and other Parts of the Western Highlands of Scotland, by James Nicol, F.R.S.E. (with photograph of the rock).—The fossils were discovered by Mr. Donald Mackay, early this year, in the Assynt quartzites of Lough Eribol, lying below the Durness limestones, in which fossils have been more abundantly found. The surface of the rock shows the ends of more than a dozen bodies, more or less rounded in form, running down into the interior of the stone, and tapering to a point. They show a central core with an outer wall around it. There is no doubt they are organic. The author rather inclines to regard them as corals approximating at least in form to *Cyathophyllidæ*. Thin slices might perhaps decide this question, but meanwhile the author has named them *Eribollia Mackayi*, indicating the locality and the finder. These and other fossils give the author no reason to reverse the relations of the strata in the north-west Highlands held by him, but much to confirm them.

Concerning the Extent of Geological Time, by Rev. M. A. Close, F.G.S.—Since geology has her own strong and unrefuted arguments for the great extent of geological time, it is not logically necessary for her to do more than show, if it can be shown, that the physical arguments for the very inconvenient restrictions thereof rest upon still unproved assumptions. The argument from the rate of cooling of the earth seems to have been satisfactorily shown by Mr. T. Mellard Reade to be quite inconclusive. The argument from the probable duration of the sun's radiation of heat assumes, *inter alia*, that the original nebula from which the solar system was formed was cold, and also that the unit of gravitation relatively to the mass of that system has been constant from the time when that mass began to fall together, and throughout the enormous interstellar distance which has doubtless been traversed by it since that time. Dr. Croll's suggestion in answer to the former of these assumptions is logically sufficient as a reply to the whole of this argument. Nevertheless it may be added, as to the latter assumption, that those physicists who have entered upon certain speculations as to the cause of gravitation, cannot deny that it is perfectly credible, and even probable, that gravitation is not an essential accompaniment of matter, and that the unit of gravitation may not be constant throughout all time and space. The arguments from the earth's figure in connection with the retardation of her rotation by the ocean tide depends on the doctrine of the steel-rigidity of the earth taken all together, as do also the calculations of various writers on subjects which bear in different ways on the present one. However, Sir W. Thomson himself has greatly weakened the support of this doctrine. But geology (as regards the matter in hand) is not concerned to question it, although it is, at first sight, a difficulty. The results obtained by the Tide Committee of the Association point to the conclusion that there is an 18·6 year-tide in the body of the earth depending on the revolution of the moon's nodes, and that the rigidity of the earth, even if it be in one sense as high as that of steel, is yet a *viscous* rigidity, by which she may yield almost indefinitely to sufficiently long-continued straining forces. Other considerations confirm this latter position. This 18·6 year-tide, whether resulting from such *viscosity* proper or from plasticity of a different kind, must cause a variation in the earth's rate of rotation during the same period. This variation would probably be desirable if looked for by the astronomers, who would confer a boon on the geologists by endeavouring to detect it. Dr. John Evans' suggestion of the possible considerable mobility of the axis of rotation relatively to the body of the earth, bears in certain ways on the present question; the mechanical objection to it, already greatly weakened by the Rev. O. Fisher, might be quite removed by the investigation suggested.

On some New Pre-Cambrian Areas in Wales, by Henry Hicks, M.D., F.G.S.—During some recent researches in Wales the author has been able to add many new areas to the pre-Cambrian rocks already described. In these examinations he has been assisted at different times by Prof. Torrel, of Stockholm, Prof. McKenny Hughes, Mr. Tawney, F.G.S., and Dr. Sterry Hunt, of Montreal. The additional areas to be now added to those previously known are:—

1. Some cupriferous schists with their associated greenstone bands (the so-called intrusive greenstone of the Geological Survey) to the north of Dolgelly, and including a great portion of Robel Tawr.

2. Masses of granitoid rocks, porphyries, and greenstone breccias, in the neighbourhood of Pwllheli.

3. The porphyries and granitoid rocks forming Myaydd Mynytho, and extending in a northerly direction towards Nevin, including also Nevin mountain and the porphyries and greenstone breccias to the north-east of Boducan.

4. The Bhos Hirwain syenite and the so-called altered Cambrian beds to the west of that mass in Caernarvonshire, and also Bardsey Island.

5. The granitoid rocks, felstones, and porphyries, forming the Rivals (yr Eifl) range of mountains.

6. The so-called altered Cambrian rocks to the west of the Penygroes porphyry.

7. The so-called intrusive granite in Anglesea, and the whole of the area marked as altered Cambrian in that island. In addition to these he has also extended some of the areas and defined more clearly the order of superposition of these rocks in Pembrokeshire. In North Wales, as in South Wales, he found that the pre-Cambrian rocks resolved themselves into three well-marked and very distinct types, and that these indicated separate formations, each of which, on careful examination, and when found in juxtaposition, proved to be unconformable to the other. At St. David's the granitoid rocks occur at the base, and, resting unconformably upon these, are found the quartz-felsites. These are again succeeded unconformably by the agglomerates, breccias, greenstone bands, and schists of the Pebidian group.

In North Wales this was also exactly the order in which the various rocks were found to succeed each other, but the middle or quartz-felsite group was found more largely developed in Caernarvonshire.

As this middle group had not previously been separated under a distinguishing name, the author now proposed to adopt for it the name *Arvonian*, from the Roman name *Arvonia*, and from which the present name of Carnarvon is derived. So many of the large ridges and lofty mountains of Carnarvonshire are composed of these felsitic rocks that it appeared to the author and his friends that this name would be very appropriate for the formation. The distinguishing characters most marked in these three pre-Cambrian formations may be briefly summed up as follows:—

1. *Dimetian:* Granitoid gneiss rocks.
2. *Arvonian:* Quartz, felsites, and porphyries (Halleflinta of Torrel; petro-silex rocks, Hunt).
3. *Pebidian:* Green and purple agglomerates and breccias, green chloritic schists, with massive greenstone bands, talcose schists, &c.

In these formations the bedding is usually easily recognised, but at present the actual stratigraphical thickness cannot be correctly estimated. It is perfectly clear, however, from the sections exposed, that each must have a vertical thickness of many thousand feet. That they have a very extended geological distribution over the British islands is also daily becoming more and more evident.

On the Metamorphic and Intrusive Rocks of Tyrone, by Joseph Nolan, M.R.I.A., &c., of the Geological Survey of Ireland.—The rocks described in this paper occupy the central parts of county Tyrone extending from Omagh eastwards and north-eastwards towards Slieve Gullion. They consist for the most part of an amorphous green hornblendic rock, in the midst of which is a wide lenticular tract of micaceous gneiss and schist. The author shows that these two classes pass gradually into each other, and that even among the amorphous hornblendic rocks traces of schistose structure can generally be observed, while local transitions into schists frequently occur. Gradations into more crystalline rocks were also noted and described, those of a hornblendic character passing into a felspathic variety in which little or no hornblende occurs, while quartz and orthoclase are developed, so that a coarse quartz porphyry is produced, passing ultimately into granite.

It was also shown that some of the granite was intrusive during the period of the old red sandstone, a new fact in Irish geology. Its association with metamorphic rocks, probably of lower Silurian age, was explained on the hypothesis that the intrusive granite was due to re-metamorphism of the later period, so that portions of the already crystalline rocks were completely fused and became irruptive. That metamorphic action in this district continued up to and even after the old red

sandstone age seems to have been the opinion of the late Gen. Portlock, who in his geological report on Londonderry, with parts of Tyrone, &c., has described these rocks and their relations to each other at considerable length. He does not seem to have considered the granite to be intrusive, but merely a metamorphosed condition of what we now call the lower carboniferous sandstone, which was then classed with those of the old red formation.

On the Cervus Megaceros, by W. Williams.

On the Occurrence of Certain Fish Remains in the Coal Measures, and the Evidence of the Fresh-water Origin of the Coal Measures, by James W. Davis, F.G.S.—These occur in a bed of cannel coal, and more particularly in the impure cannel above and below it, in the district of Morley and Adwalton, near Leeds. They consist of both Elasmobranchs and Ganoids, but by far the most common are *Cœlacanthus lepturus*. The fresh-water nature of the cannel coal and the internal anatomy of the Cœlacanthus, together with occurrence of Labyrinthodonts, lead to the conclusion that the strata were of subaqueous, and probably fresh-water origin.

On the Age of the Crystalline Rocks of Donegal, by Prof. W. King, D.Sc.—The author had succeeded in obtaining some true fossils in portions of the Innishowen limestone that have scarcely undergone any change. He had not had time to examine them as closely as he would have wished, but they appeared to be identical with Caradoc Bryozoon from the Desertcreat schists of Tyrone, which Portlock has called *Ptilodictya dicotoma*. This was the first example, as far as he could ascertain, of an undoubted fossil having been detected in these limestones. The fact may be taken as evidence that these deposits and their associated argillaceous and siliceous masses are of the lower Silurian age, and it seemed highly probable that the more intensely metamorphosed rocks in the north-west division of Donegal belonged to the same geological period.

On the Cervus Megaceros, by W. Williams.—The author considered that in some excavations he had made at Ballyhetagh Bog, near Dublin, he had met with evidence which led him to differ from former views as to the time of these animals' existence. He asserted that the clay on which the remains rested is the *lower boulder clay*. He considered the animal lived during the middle glacial period, and was killed off by the cold of the *uppper boulder clay* period.

Prof. Leith Adams, F.R.S., questioned the accuracy of Mr. Williams' views. There was no evidence for such a division of glacial beds in the locality in question, as Mr. Williams had mentioned, nor was there any proof that the animals had been destroyed by the intervention of a cold period. In England there is authentic evidence of the co-existence of this animal with man.

After the discussion on the *Cervus megaceros*, the president submitted the following lines:—

"Small comfort to the stag that's mired,
To think that in long distant ages
He'll be dug out to be admired,
And have his life discussed by sages."

which Mr. Pengelly capped as follows:—

"Yet had he known their fearful puzzle,
How far from truth each sage would be,
Methinks he'd rear his cervine muzzle,
And scent the future Section C."

SECTION D.—BIOLOGY.

Department of Anthropology.

Miss A. W. Buckland read a paper *On the Prehistoric Monuments of Cornwall compared with those in Ireland.*—In the course of her paper Miss Buckland said that the prehistoric monuments of Cornwall, believed by archæologists to be the work of the same race as those of Ireland, presented, in the midst of strong resemblance, certain points of difference which deserved the attention not only of archæologists but of ethnologists. In both countries they consisted of tumuli, including chambered barrows and giants' graves, monoliths or menhirs, circles, cromlechs or dolmens, and holed stones, all probably sepulchral and hut circles, cliff castles, curious caves and crosses, whilst in Ireland they found in addition earthworks called raths, and round towers. Long barrows, which were looked upon as the most ancient of burial places belonging to the stone age, were wanting in both countries, hence we may infer that the people who then existed in England and Scotland never inhabited Cornwall and Ireland, where the earliest barrows seem to belong to the bronze age, the mode of interment in Cornwall being chiefly by cremation; but these tumuli may not represent the earliest tombs in these countries.

Mr. W. J. Knowles read a paper *On Flint Factories at Portstewart and Elsewhere in the North of Ireland.*—The paper contained a further account of a find of flint implements found in sandhills at Portstewart, county Londonderry, near the mouth of the River Bann, which consisted of a large quantity of scrapers, some arrow-heads, bone implements, hammerstones, and flakes, with shells, broken bones, and pottery. These were found in hollows amongst the sand, but were supposed to have dropped from black layers on the sides of the pits as the sand was removed by the wind. The black layer represents the ancient surface, and similar objects have been dug out of it. The only new find since the subject was last brought before the British Association was some porous lava and flakes of obsidian. These substances are not found native, as far as the author could learn, and he believed they must have floated a distance, probably from the West Indies. He also described other places which he had explored in the neighbourhood of Castlerock, county Londonderry, and Ballintoy, county Antrim, where similar remains were found, and the same kind of black layers are to be seen. The black layers, when not destroyed or uncovered by denudation, are covered up with sand from ten to twenty feet in thickness, which is protected on the top, but is gradually wearing away where exposed at the sides. The animal remains were submitted to Prof. A. Leith Adams, of the Royal College of Science, and he decided that they contained man, horse, ox, hog, dog or wolf, fox, and deer. He also described objects from Larne, some of which were found nine feet from the surface, and he exhibited a photograph of a mammoth's tooth, found in a small delta-like field near Larne, which is now in the possession of the Rev. Dr. Grainger, M.R.I.A., of Broughshane, county Antrim, and flakes and rude implements from the same field. He also showed and described a series of rude implements thick at the one end, for holding in the hand, and pointed at the other, of the palæolithic form. Although there were no animal remains found with them they were taken from the diatomaceous deposit below the peat, where remains of the Irish elk are usually found, and the author drew attention to a statement of Dr. John Evans in his "Stone Implements of Great Britain," where, on comparing the implements found in the caves and those in the old river drifts, he says—"the large-pointed implements are mostly found in the latter;" and gives as his reason for their being found mainly there that they were probably used "for out-of-door purposes." The author states that it was strange that his large implements of similar form were found mainly in the bed of a river, and suggested that the rivers and that form of implement had probably some connection, and that they were not used for "out-of-door purposes" only. Reference was next made to the age of the implements found in Ireland, about which he said authors were not agreed, and the author again quoted from Evans' "Stone Implements," and showed that the descriptions of many of the palæolithic implements in that work would apply to the Irish ones, and concluded that we must either carry man farther back than the so-called neolithic age, or give up some of our theories regarding the distinguishing characteristics of the palæolithic and neolithic implements.

Mr. V. Ball, M.A., exhibited a number of objects of ethnological interest, collected in the districts of India, and also in the Nicobar and Andaman Islands. These included stone implements, battle-axes, an instrument like a boomerang used in killing small animals, but which was not capable of the return motion of the Australian boomerang, arrows, musical instruments, &c. There was a curious wooden figure, not an idol, but an effigy of some departed person, with a sort of girdle round the waist, which hung down like a tail behind; and the author thought it explained the ancient tradition of men with tails in those islands. Another singular item consisted of two ornamental skulls, in reference to which Mr. Ball mentioned that on the death of an Andamanese his body was placed on a tree, and as soon as his bones became bleached his skull was taken and ornamented, and first carried about by his widow, and afterwards by other members of his family. The collection included some photographs, one of which represented buildings on piles in water.

Mr. T. J. Hutchinson, lately her Majesty's Consul at Callao, read a paper entitled *Habits and Customs amongst some Tribes of*

Tropical Aborigines.—The tribes spoken of were some of those in West Africa and South America, with whom he had been acquainted during twenty-three years. The object of the paper was to show analogies in superstition and social barbarities, as well as in a sort of indigenous civilisation amongst people of different races dwelling on different sides of the globe, and with respect to whom there was no evidence of their having ever been in communication with each other.

M. Henri Martin, the French historian, read a paper written in French, entitled *Les Races anciennes de l'Irlande ; Utilité de l'Étude des Traditions qui les concernent pour l'Ethnographie de l'Europe primitive.*

The chairman, Lord Talbot de Malahide, said they must all be very much flattered at the interest which M. Henri Martin had taken in the ancient history and traditions of Ireland. He had shown in how many respects those traditions were connected with the traditions of the Continent, and how much light they threw on the migrations of the principal races which had colonised Europe.

Mr. H. H. Howorth made a communication on the *Spread of the Slavs.*—He said that if they excluded the Turks from Roumelia and Bulgaria, the Basques from Spain, and the various tribes of Finns from the north-east of Russia, they should have a tolerably homogeneous population left in Europe, which might be divided into the Celts, in the West; the Teuton in the centre, and the Slavs in the East. The ethnology of the Slavs had only been recently treated on a scientific basis. They owed this to the researches of Count Polocki and Shafarick, the results of which, together with some of his own, he would bring before them. Patriotic Russians derived the name *Slav* from *Slova*—glory—a derivation improbable, when it was remembered that the name was very recent, not occurring before the sixth century. Shafarick believed that *Serb* was the oldest name of the Slavs, but that also was a modern name, and appeared to be of foreign origin. The oldest name of the Slavs was that by which they were known to the Germans—namely, *Wends*, a name occurring in classical writers under the form of Veneti. It was curious that the name *Slav* (slave) and *Servus* (Servian) should both indicate a people of servile condition. The Emperor Constantine, who gave the first account of the Servians, said that the name Servian was derived from the fact that they served the Roman emperor. About the Christian era, there was a great revival amongst the races of Eastern Europe. The power of various nomades of South Russia, who are now known to be of Aryan origin, was broken by the Romans. That enabled the Turkish and other nomades east of the Volga to invade Europe; and they drove the Aryans to the centre of Europe, where they settled in the country about the Carpathians. This accounted for the fact that amongst the Poles and Bohemians the upper classes were entirely distinct from the lower, the Polish grandees being derived from the immigrants, while the peasants belonged to the old stock of Slavs. Afterwards the Turks and Huns pushed into Europe, driving the Slavs still further, and causing in the sixth century a great migration of them south of the Danube, where they overran Bulgaria, European Turkey, and the mainland of Greece. Others were driven to the Dalmatian coast and islands. The Emperor Heraclius, in order to protect himself, invited the Slavs of the Carpathians, called Croats, and the Serbs, to his assistance. The name Croat was derived from their living in the mountain districts. *Chrebet* and *Carparthian* being the same word. One section of the Serbs who lived on the river *Bosna* were called Bosnians; another, who inhabited the Black Mountains, were called Montenegrins. A third section inhabited a district which was afterwards created into a dukedom by the Emperor of Austria, on whom they were dependent, and were called Herzegovinians from the German, *Herzog*, a duke. The rest of the Serbs formed the Servian community. All these were of one race, and had the same traditions, the only difference being that the Croats were converted to Roman Catholicism, while the Serbs became the converts of Greek priests; and hence arose their existing religious differences. The Slavs of Eastern Thrace were conquered in the seventh century by a race of intruders closely allied to the Magyars of Hungary, and known as Bulgars. Their descendants were the dominant class in Bulgaria, and had the high cheek bones and oblique eyes of their race. The speaker concluded by describing the various migrations of the Russian Slavs.

Prof. Daniel Wilson, F.R.S.E., of Toronto University, read a paper *On some American Illustrations of the Evolution of New Varieties of Man.*—He said abundant traces were apparent of the intrusion into Europe in prehistoric times of one or more races superior alike in physical type and in the arts upon which progress depends to the primitive races. Furthermore it was assumed that an admixture took place between a fair, tall, intrusive race and the primitive Australioid savage of Western Europe, resulting in the so-called Melanochroi, still abundant in Britain, France, and elsewhere. On turning to the American Continent, they saw vast regions occupied exclusively, until a comparatively recent date, by tribes of savage hunters, upon whom the highly civilized races of Spain, France, and England have intruded with results in many respects so strikingly accordant with the supposed evolution of the Melanochroi of modern Europe, that they seemed to look upon an illustrative series of ethnological experiments carried on, on the amplest scale, and begetting synthesis altogether confirmatory of previous inductions. The intermingling of very diverse races at present taking place on the American Continent, was indeed complex and varied. Hybridity was the result on a great scale, and the process had already been perpetuated sufficiently long to beget important indications of the possible evolution of permanant hybrid varieties. A new race, as among the tribes of half-breeds of Manitoba, was seen as it were in the very process of evolution; while, sheltered within the remote Arctic North, man could be studied among the Esquimaux in conditions closely analogous to those which are ascribed to post-pliocene if not to pre-glacial man. In the abrupt collision of the civilized races of Europe with the American aborigines, it had been taken for granted that the latter were doomed to inevitable extinction, and were to be replaced by the purely intrusive race of the Old World. A growing feeling was manifested now, alike in the United States and in Canada, in favour of the idea that the Indian population was not wholly disappearing by extinction, that a much larger amount of healthful intermixture and consequent absorption had taken place than unobservant critics had any conception of, and that the native Indian element was a factor in the population of the New World, destined to exercise an enduring influence on the ethnical character of the Euromerican race. If so, and the result was to be the perpetuation of permanent traces of the native American man in the mixed race of settlers by whom the vast forests and prairies of the New World were being converted to the uses of civilized communities, rivalling Europe in all the highest elements of progress, it would be no more than had been already recognised in the mixed races of Europe.

Mr. A. L. Lewis read a paper *On the Evils arising from the Use of Historical National Names and Scientific Terms.*—The propositions endeavoured to be established by Mr. Lewis were: (1) That there were at the first population of Europe certain primitive races, of which three are particularly described; (2) that these races are so mixed at the present day that representatives of them appear not only in most European nations, but in the same families, and among children of the same parents; (3), that notwithstanding this mixture, and the effects which it must permanently have, racial character displays an astonishing permanence; (4) that this mixture, being so slow in its effects, and yet having become so general, has probably been at work, and for a very great length of time, so great that the peoples to whom the earliest history of Europe introduces us were probably nearly as much mixed as those of the present day; (5) that it is desirable to discontinue the use of the political names of those people as ethnic names, and to employ others based on the physical characteristics of the individuals; (6) that while physical characteristics are the only basis for a true division into races, yet in any practical application of this division, we must consider the influence upon individuals of different races of a community of language, whose history or tradition must not be lost sight of, although these things do not prove community of race, but only the contact at some time or other of the races to whom they are now common.

Prof. Huxley said the subject of the paper was one of importance, not merely on ethnological or scientific grounds, but because it was unfortunately the source of a great many practical fallacies which have had, and in fact still have, a very important political influence. He doubted very much whether there was any deliberate system of misnomer which was working more mischief in this world than the preposterous talk about the national qualities of the Celt and the Saxon. He had taken the liberty a number of years ago of getting himself into hot water by trying to awaken people's attention to what was the effect with regard to the use of these terms, and to the sort of mischief that was

being done by using them in the exceedingly inappropriate manner in which they were naturally used by political writers. His conclusions then were entirely in accordance with those which Mr. Lewis had just now brought before them. He (Prof. Huxley) believed that if there was a proposition in ethnology which was capable of historical proof it was that, so far as physical characteristics were concerned, the ancient Gauls—as was the opinion of the Roman and Greek historians—were persons of precisely the same physical peculiarities as the ancient Teutons known to the same historians. In fact, there was a most extraordinary correspondence to the phraseology in which the Teutons are described by a well-known writer, and those in which the earlier historians described the Gaulish invaders of the Roman Empire and the Greek Kingdom. That he believed to be beyond all question, and so far as physical characteristics went, he did not believe that there was a shred of evidence to show that the persons who spoke Celtic dialects at the time they made their appearance in Western Europe were in any physical respect different from those who spoke the older Teutonic dialect, and not only that there was no difference, but there was a most extraordinary resemblance, inasmuch as those stocks when they came into contact with the civilised world were described in the same terms—as sturdy, fair-haired people, with fair skins, and what he thought without any exaggeration may be described as a remarkable shortness of temper. He would not enter now into the interesting questions which Mr. Lewis had raised. The deliberate conclusions which were drawn from this subject with regard to the real distinction of race in our islands were, that the people of some particular race were marked by a tendency to certain social organizations and certain peculiar mental constitutions. Now he dared say that might be so. He could not—no person who was a professional zoologist could—fail to entertain the most exalted ideas of the influence of race, and he had no doubt there was great influence; but what he did very much doubt was whether they had the smallest means of knowing what at the same time was the amount of influence exerted on the people of this country by the different ethnological elements which compose it. Let any one who listened to the talk about national characteristics, and what was said about particular institutions being impossible for some of the people of these islands and possible for others—let him carry his mind back for the last twenty years and think what was at that time said about the German people. Great writers of public opinion at that time were never tired of enlarging on the saying of one great German, that while the Empire of France was on the land, and that of the British on the sea, the Germans had the empire of the air; but they proved themselves during the last fifteen years to be about as practical and hard-fisted a people as any that existed at the present time in the universe; and we did not hear anything of the Teutonic dreamers since the battle of Sedan. He believed that we knew so little about the races that it was impossible to disentangle what any particular nation was. We did on the other hand, know that there was a great deal of human nature in all kinds of men, and of social conditions which exercise an enormous influence. He thought he would endeavour to make out what in any given race at the present time was due to the pre-existing social and political relations—and when he had sifted that he would have some reason to talk of *residuum* as being the consequence of race influence. He himself did not believe, taking any one section of the British empire—whether Scotch or English or Irish—he did not believe that race has any appreciable influence upon their social and political condition of the present day. That was to say, his impression was that if the south-eastern parts of the British empire, the county of Kent for instance, had been subject to just the same sort of conditions for 400 or 500 years as, he would say, Connemara and Galway, he should expect the results to be as nearly as possible the same; and it was a curious fact of ethnological study that those parts of Ireland which are supposed to exhibit in the most marked manner these characteristics, sometimes complimentary and sometimes uncomplimentary, were those in which it could be proved to demonstration that the Norman and English elements were most predominant.

Captain R. F. Burton read a paper entitled *Notes on the Tribes of Midian.*—The country once belonged to the Moabites, Ammonites, and Amalekites of Scripture, but the tribes now inhabiting it were comparatively modern. They were a mixed race. The inhabitants of the uplands were fairer in complexion and more fleshy and muscular than the dwellers in the lowlands, who were more dark and slender. Some of the higher classes were decidedly handsome, having erect muscular figures, straight features, black hair, and olive-coloured skins, fine eyes, restless and piercing, though their beards were rather thin. Longevity was rare amongst them in consequence of incessant fatigue, indifferent nourishment, and want of cleanliness. The inner man was not so easily described. Their chief characteristics were strong social affections, eternal suspiciousness, extreme pugnacity, and proportionate revengefulness. Their sociability was extreme, and they made great sacrifices for one another.

Prof. W. H. Flower read a paper *On the Methods and Results of Measuring the Capacity of Crania.*—Of all the measurements by which they could determine the difference between the human skulls of people of one race and of a foreign race, perhaps the most important was that which gave the cubic capacity of the great cavity of the skull which contained the brain. Many ways of ascertaining it had been tried. Some persons laid great stress on the weight of the brain, but for his part he thought that on the whole if the capacity of the skull could be got it would be more valuable. The weight of the brain differed very much according to the age or physical conditions of the person when he died, and there were certain diseases which went to increase the specific gravity. But when the actual capacity of skull was found they had the actual capacity of the brain at the time of health. There was another very important reason why they laid stress on obtaining the capacity of the crania in preference to the other method. It was because all their museums now contained a number of skulls from different parts of the earth, some of which were very inaccessible to scientific observation, and it was, of course, impossible to ascertain the actual weight of the brains of these people after death. Then, again, how could they get the capacity of the skull by the weight of the brains in cases where the races had become extinct, such as the Tasmanians, many of the Polynesians, the ancient Britons, and the ancient Irish, and others, specimens of whose skulls they possessed, and by which they could ascertain the capacity of the brain? He supposed he would be expected to say at once whether he attributed any great and direct importance to the weight and age of the brain as an indication of intelligence. Well, he thought it was one of the very many points that had to be considered in this question; but he thought there were a great many other things to be remembered in this view of the question. For instance many people had large brains and did not know how to use them, and some who know how to use them did not try to do it. They would see that many of the races that were naturally considered the higher races, and had taken the lead in the civilisation of the world, had undoubtedly larger cranial capacities than the peoples who were at the bottom of the ladder of civilisation. He would never accept the mere fact of a man's head being large as an indication of superior intelligence, but it was one point to be considered. The measurement of the skull was not only an important but it was also a difficult work, more difficult in fact than a great many people supposed, and a great many of the uncertain results that had been obtained on this subject were owing to the persons who had taken the matter in hand not having yet discovered the best and most certain method of carrying out the investigation. A large number of measurements published were only of an approximate value, owing to the numerous fallacies and difficulties experienced in arriving at a satisfactory method of measurement. Nothing, apparently, could be easier than to take a skull and stop the cavities, and pour some fluid into it and then pour it out and measure it, but they could not do this with the skull, as the bone was very porous and full of minute invisible holes, through which the fluid soaked as it would through a sponge. It was only by making the skull waterproof that they could seek to measure its cavity by a fluid. He had a skull by him which had been so prepared. The large holes had been filled with wax and the skull soaked in melted paraffin, which filled up the minute cavities, and when it was cooled it was as impervious to any fluid as delf. But the materials that had to be used in testing the capacity of the skull must be something solid. Various things, such as shot, grain, &c., had been used. He would pass over the various methods that had been tried and failed, and which would be found recorded in the *Transactions* of the Anthropological Society of Paris, and speak of two methods which at the present time meet with the greatest amount of success. One was the method of Mons. Broca, and the other the method of Mr. Busk. The latter had shown such good reasons for his plan that he thought it particularly safe to try it, and after doing so he had adopted it with some modifications.

He filled the skull with mustard seed well shaken, and pressed into it with the thumb, and then poured into a long wooden box with glass sides in it, in which it was well shaken and pressed down. The figures on the glass indicated the spaces filled. This he thought was the most satisfactory way as yet invented, and they could hardly hope for better. He always kept his experimental skull by him when measuring other skulls, in order that he might occasionally go back on it to see if he had gone wrong. Now, as to the measurement of the skulls of the different races of the human family, a very important point to consider, and a very difficult one, was the sexes, because there was a great difference in the size of the skulls; a much greater difference than there was between men of different races. To get the average of any race they must get a large number of skulls, and he must say their collection was very insufficient at present. According to a comparison between the skulls of sixty-three men of various races, and skulls of twenty-four women, the size of the woman's skull to the man's was as 854 to 1,000. The largest normal skull he had ever measured was as much as 2,075. He knew nothing of its history. It might have been the head of a great philosopher, but unfortunately they were not in the habit of getting the heads of philosophers in their museum. Nearly all the English skulls were those of persons in the lowest ranks of life. It was these they had to compare with the specimens of other races. The smallest head he had measured was 960 centimetres, and that belonged to one of those peculiar people in the centre of Ceylon, who were now nearly extinct. The largest average capacity of any human head he had measured was that of a race of long, flat-headed people on the West Coast of Africa. The Laplanders and Esquimaux, who were a very small people, had very large skulls. The latter gave an average measurement of 1,546. He then came to the English skull, which was nearly the same size—1,542; but, as he had said, they belonged to the lower grades of English skulls. He could not tell them anything about Irish skulls, for there was not a single specimen of the Irish skull in any London museum. The inhabitants of the Canary Islands give a capacity of 1,498; the Japanese, 1,486; the Chinese, 1,424; the modern Italian, 1,475; the ancient Egyptian, 1,464; the true Polynesians, 1,454; negroes of various kinds, 1,377; the Kaffirs, 1,348; Hindoos, 1,306. They then came to the Australian aborigines, who were amongst the smallest, only giving an average of 1,283. There were two races still below the Australians, namely, the Andamanese, who were a very diminutive people, with a capacity of 1,220, and the Veddahs, of Ceylon, who had an average skull.

The President (Prof. Huxley) said he might, without hesitation, offer the best thanks of the Section to Prof. Flower for the important and interesting paper he had just read. Persons not ordinarily occupied with scientific pursuits might not be aware of the amount of care that had to be taken when it was desired to do any good in scientific matters in obtaining data, which data would, when obtained, pack into the very smallest possible results. It would be seen what care was required to obtain measurements of the cubical contents of the skulls, and yet the whole of the labour, if Mr. Flower published his paper, as he hoped he would, would go into the space occupied by the three or four rows of figures. There was one very interesting question he wished to put to Mr. Flower—whether it was possible to establish not only a series of absolute measurements of the capacities of the skull, but also some kind of index of capacity in which can be expressed the ratio of capacity of the skull to the stature of the person to whom it belonged, or if it was impossible to obtain that, yet even to obtain such data as would show the relation between the contents of the skull and the length of the part of the skull which was, as it were, the foundation of the skull.

Department of Anatomy and Physiology.

On a Direct Method for Determining the Calorific Power of Alimentary Substances, by J. A. Wanklyn and W. J. Cooper.—The amount of oxygen consumed by an organic substance being the measure of its heat-producing force, the importance to physiologists of a direct and rapid method for measuring the consumption of oxygen in organic fluids is obvious. It is well known that an elementary combustion will effect this object, but, as is likewise well known, the great difficulties which beset it render it unavailable for physiological researches. The process by which we obtain all the results of an elementary combustion can be completed in about an hour, and in the course of our experiments on various organic substances we have been enabled to get an amount of oxygen absorbed which is equal to the theoretical quantity required by the substance operated upon. In point of fact, we have so modified Forchhammer's process as to make it work in a satisfactory manner.

Forchhammer's process, as hitherto practised, does not effect anything approximating to complete oxidation down to carbonic acid and water, as was illustrated by some experiments published by Frankland and Armstrong in 1868 (*vide* Chem. Soc. *Journ.*, vol. vi. p. 82), which we quote:—

Name of substance (30 parts dissolved in 1,000,000 parts of water).	Oxygen absorbed during six hours.	Oxygen required for complete oxidation.
Gum arabic	0·35 ...	35·5
Cane-sugar	0·15 ...	33·7
Starch	0·30 ...	33·5

showing that, as usually carried out, the oxidising process does not avail to accomplish more than about one-hundredth part of the task set before it.

The modifications whereby we have completely altered the character of the Forchhammer process are as follows:—

Instead of simply mixing the standard solution of permanganate with the water to be examined, we *distil* a given volume of the water (say 1 litre) with a considerable excess of standard solution of permanganate, and thereby get more oxidising action than in the ordinary operation. We find advantage in having the liquid strongly alkaline during the distillation; but we render it acid before titrating the residue.

Mr. Lawson Tait, F.R.C.S., read a paper entitled *Note on the Occurrence of a Sacral Dimple and its Possible Significance*.—Some years ago he noticed casually the occurrence of a curious pit-like depression or dimple in the skin over the lower bones of the sacrum amongst the patients of the hospital for women to which he was attached. He paid no special attention to it until about two years ago, when an instance came under his notice of a woman in whom it was well marked, and in all of whose children it was to be observed. Three of them, all girls, had it strongly pronounced; and in one, the eldest, about eight years of age, he found the best marked instance he had yet seen. It was quite a centimetre in depth, and it expanded outward, so that its mouth had a diameter of about 13 millimetres. This circumstance induced him to make observations as to the frequency of its occurrence in some hundreds of women passing under his notice, and he found that in 55 per cent. no trace of it was visible, that in 22 per cent. it was faintly marked, and in 23 per cent. it was well marked. Occasionally two depressions were present instead of one, both being in the middle line always, and situated from half an inch to an inch and a half apart. The average age of the women, in whom it was well marked, was slightly over 32 years, whereas that of those in whom it could not be observed was nearly 45, from which he concluded that it had a tendency to disappear with advancing age. The same inference was to be drawn from observations made by Dr. A. H. Carter at the Children's Hospital. The prevalence of this curious mark was suggestive that it must be a remainder from some embryonic process connected with the neural canal and its closure. The explanation which he was about to offer of its significance must be viewed charitably, and as the result of an accidental observation. Mr. Tait then gave an account of the appearance presented by the body of a kitten remarkable as having no tail. The mother was a favourite cat in a district where there were no tailless tom cats. The lady who presented it to him told him that during the whole of the kitten's brief life water was seen to run from the spot where the tail ought to have been, which water was industriously removed by the mother. The kitten took nourishment, and moved about after the fashion of new-born kittens, but did not appear to use its hind legs. On examining the body he found a small aperture through which fluid could still be pressed, and on opening this carefully he found that it passed through a deficiency of the neural arches of the sacrum, directly into the cavity of the spinal cord. At this point the cord broke up into its ultimate branches, and the tail was represented just as it is in other tailless vertebrates, the neural arches being inclosed, and the caudal vertebra being represented by only three elongated centra, enveloped in a slight fold of skin, just as in the Manx cat and in the Guinea pig. The study of a number of human sacra revealed the

curious fact that they differed very materially. The sacrum is generally figured as being composed of five vertebral elements, but he had frequently found six, and in one case seven. A friend had told him that he had seen one with only four. Of the five usual sacral vertebræ the two lower are generally figured as having gaps in the neural arches, but it was his experience that it was more usual to find the gap also in the third, and he had seen sacra in which there was only one neural arch. He had not studied the sacra and coccyges of any of the tailless vertebrata but the guinea pig, but even in it he had found curious variations, chiefly in the length and number of the coccygeal centre, and he assumed that in such an early type such variations must of necessity be less marked than in the later ones. He thought it likely that spina bifida was not confined to the human animal. In the human animal its most common seat is the sacral and lower lumbar vertebræ. It was fair to assume, therefore, that in a tailed vertebrate it would occur more frequently in the tail than elsewhere. If this be the case, and if, likewise, it ever does occur amongst animals not domesticated, it will most certainly occur occasionally that a tailless vertebrate will, by reason of a spina bifida, be born of properly tailed parents. If the deformity was sufficiently high up to destroy the tail, yet not high enough to interfere with the nervous supply to the posterior limbs, then he might survive in the struggle for existence, provided the tail was not essential to life. They could easily imagine that under favourable circumstances a monkey born with this change in his economy might strike out a new line of life, breed tailless children, and by the change of other structure necessitated by the new method of life, introduce a new variety of monkey. Those who were still opposed to the evolutionary view of creation might sneer at this as a series of "ifs," but they must be reminded that the whole of life—individual and collective—is but a change of circumstances, precedent to any one of which an "if" can be introduced, and that had "if" really been effective the whole chain would have been different from that point. What, then, was the significance which might be attached to the sacral dimple which was the subject of the paper? Looking at it surgically there could be no question that it was a cicatrix. What he ventured to suggest was that it was the hereditary cicatrix of the *spina bifida* by which the human tail had been lost. Such a suggestion was of course open to ridicule, but only, he ventured to think, in the minds of the incautious or the ignorant. That man was once a tailed vertebrate was beyond doubt, and that he lost his tail was, of course, equally evident. He was not the only vertebrate who had lost a tail, and in whatever way those tails had been lost they had evidently all been lost in the same way. Now, curiously enough, they had an animal living in one of the sister isles of which not a genus, nor a species, nor a variety, but a mere family had lost its tail; he referred of course to the Manx cat. He had obtained some of these cats from the Isle of Man, and he was quite certain that if they knew the history of this curious family of cats they should know exactly how all the vertebrates which are now tailless became so, and he felt very confident that the Manx cats lost their tails through the occurrence, within very recent times, of a tom cat with a *spina bifida* placed exactly where it was in the case of the kitten he had referred to. A limited area like Man would present the most favourable circumstances for the protection and propagation of such a variation; for he (Mr. Tait) had already elsewhere pointed out that the bushy tail of such animals as the cat served chiefly for the purpose of maintaining temperature, and in the mild and equal climate of the charming island a tailless cat would have little hardship to endure. It could not have survived, however, in any place where snow lay long on the ground. In the few Manx cats he had examined he had seen no trace of dimple, nor had he seen any appearance of it in the guinea pig; but these negative results did not seem to him to be important. What would be important would be the examination of a number of tailless monkeys, especially young ones. If in these no dimple was to be found, then he feared they should have to look forward to some other history for this curious cicatrix.

The president said he was not inclined to take the same view as Mr. Lawson Tait as to the means by which the tail in the human species had been got rid of. It appeared to him that what Mr. Tait had described was more likely to be the remains of infantile *spina bifida*. It was all very well for human beings to object to their having tails, but, as a matter of fact, they had tails, though they had disappeared to very small dimensions, in which they now existed in the human family.

Department of Zoology and Botany.

On the Remains of a Permian Fauna in North America, by Prof. E. D. Cope, U.S.A.—Prof. Cope described the remains of a fauna, now extinct, which had inhabited North America during the period of geologic time next succeeding the coal measures, which is known as the Permian. He had first ascertained its existence through specimens sent from Illinois in 1875, but had discovered much larger deposits of similar animals in Texas. The characters of the latter showed that they were to be referred to the classes *Reptilia* and *Batrachia*. A number of generic types of reptiles were mentioned, most of which are characterised by the notochordal vertebræ. He particularised the characters of the dentition of *Diadectes* and *Bolosaurus*, where the teeth are transverse to the long axis of the jaws. He entered more fully into the structure of *Clepsydrops*, Cope, where almost the entire skeleton had been discovered. This includes clawed lizards, with large canine teeth and several incisors, humerus without condyles, but with supracondylar foramen; reptilian posterior limb; boat-shaped pelvis, without obturator foramen, and with the neural spines of the sacral vertebræ greatly elevated (in *C. natalis*). There are small intercentra between the inferior parts of the adjacent centra, which support the chevron bones in the tail.

The Batrachia display remarkable characters of the vertebral column. In *Cricotus* the intercentra are developed so as to resemble centra, so that the column appears to consist of two kinds of vertebræ alternating with each other. A neural arch of the caudal series stands equally on centrum and intercentrum, but the intercentrum only bears the chevron bone.

Sir J. Lubbock read a paper *On the Habits of Ants*.—The author observed that he had kept about thirty species of ants in confinement. They throve well, and he had some specimens which he had kept since 1874. They were probably bred in the previous year, and would now, therefore, be five years old. He also referred briefly to the other insects which were kept by ants in their nests, and especially to aphides, some species of which are kept and carefully tended by the ants throughout the winter, though at that season they are not of any use. He referred shortly to his experiments on the senses of ants. Their sense of smell is very delicate, though much more so in some species than in others. On the contrary, he had never observed any proof that they are capable of hearing. As regards sight, he had been able to satisfy himself that they were capable of distinguishing colours, and that they are, for instance, very sensitive to violet. The ants of a nest not only knew one another, but they remembered one another even after a year's separation; and he recorded some experiments by which he attempted to ascertain how the recognition is effected. He also referred briefly to the insects which are domesticated by ants, and gave a short account of the slave-making species, which are (at least in one case) entirely dependent on their slaves, and would perish even in the midst of plenty if left to themselves. He kept some of these ants, however, alive for months by giving them a slave for an hour a day to clean and feed them. The communities of ants, he said, offer numerous analogies to those of men, and the difference in the habits of the various species of ants are also in this respect not without interest. The slave-making ants, indeed, represent an abnormal and, perhaps, only a temporary state of things, for it is not impossible that the slave-making species will eventually find it impossible to compete with those which are more self-dependent and have reached a higher phase of civilisation. But putting these species on one side, we find in the different species of ants different conditions of life curiously answering to the earlier stages of human progress—namely, the hunting and pastoral, and even to the agricultural. For instance, some species, such as *Formica fusca*, live principally on the produce of the chase, for, though they feed partly on the honeydew of aphides, they have not domesticated these insects. These species probably retain the habits once common to all ants. They resemble moreover the lower races of men who subsist mainly by hunting. Like these, they live in comparatively small communities, and the instincts of collective action are little developed among them. They hunt singly, and their battles are single combats like those of early history. Such species as *Lasius flavus* represent a distinctly higher type of social life. They may literally be said to have domesticated certain species of aphides, and may be compared to the pastoral stage of human progress, to the races which live on the produce of their flocks and herds. Their communities are more numerous, they act more in concert, their battles are no mere single combats, but

they know how to act in combination. Sir John was disposed to hazard the conjecture that they will gradually exterminate the mere hunting species, just as savages disappear before more advanced races. Lastly, the agricultural nations may be compared with the harvesting ants, none of which, however, live in our country. When he first began keeping ants, Sir John surrounded the nests by moats of water. This acted well, but the water required continual renewing, especially of course in summer, just when the ants were most active. At length, however, in considering the habits of ants and their relation to flowers, another plan occurred to him. The hairs by which plants are clothed are of various forms, and fulfil various functions. One is to prevent ants and other creeping insects from climbing up the plants so as to obtain access to the flowers, and thus rob them of their honey. It occurred to him, therefore, that instead of water he might use fur, arranged so that the hairs pointed downwards. This he found to answer perfectly; and he mentioned it specially because the same arrangement may perhaps be found practically useful in hot climates. It is, of course, very possible, indeed, that the tropical species might be able to climb up the fur, or that for other reasons the plan might fail—the fur itself, for instance, might be devoured—but, at any rate, the experiment would be worth trying. It is generally stated that the queens alone lay eggs. This, however, appears not to be the case. The workers do sometimes, though only as exceptions, lay eggs; but it is curious that their eggs always appear to produce males.

Sir Walter Elliott made a few observations *On the Annual Increase of the Common Vole* (*Arvicola agrestis*) *of late Years.*—In the spring of 1876 they appeared in such numbers in the hill pasture farms of the border districts between England and Scotland, and parts of Yorkshire and Wensleydale, as to destroy the grazing ground on which the sheep depended in spring, causing serious loss to the farmers by impoverishment and death of stock.

Dr. R. H. Traquair read a paper *On the Genus Ctenodus* (Agassiz).—The object of the paper was to suggest the great probability of the identity of the genus Campylopleuron of Huxley with Ctenodus of Agassiz. Should that be the case, Ctenodus will differ more from the old red sandstone genus Dipterus than is usually supposed, as in Camplyopleuron there is, as in the recent Ceratodus, a continuous dorsocaudal fin.

Aberrant Sacrum connected with the Oblique Pelvis.—Dr. Allen Thomson, F.R.S., showed a number of sacra exemplifying the irregularity which he regarded as the cause of oblique pelvis. He referred to similar developmental irregularities occurring in the lower animals, and exhibited one specimen—namely, the skeleton of a wombat—where the irregularity was one-sided, as is the common cause of the oblique pelvis in the human subject.

Prof. Macalister, Dr. Lawson Tait, and Dr. Harvey took part in the discussion, and all the speakers agreed that the developmental cause must be considered the usual one, and in ignoring the occasional occurrence of this form as resulting from disease.

Mr. R. W. Sinclair read a paper *On Recent Additions to the Irish Lepidoptera.*—In his paper he mentioned fifty-four species new to the Irish list. Mr. Sinclair said that it was very remarkable that of the large number of fen and marsh insects that occur in England, hardly one-third occur in Ireland; for instance, in the genus Leucanidæ, out of the thirty-one English species only thirteen are Irish.

Mr. C. Spence-Bate, F.R.S., presented a report *On the Present State of our Knowledge of the Crustacea.*—This paper formed Part 4 of the series, and was on development. Mr. Spence-Bate also read a paper *On the Willemœsia Group of Crustacea in the "Challenger" Collection.* These had all been stated to be animals without even the rudiments of eyes, and appeared to correspond very closely with the genus described from the Mediterranean as Polycheles. The author pointed out that not only were the animals of the group not blind, but that they had eyes of varying proportions. The species of these genera were taken at thirteen different stations, at depths varying from 120 to 1,900 fathoms, and chiefly on a soft bottom of mud or globigerina ooze, and he thought they might safely infer that the entire group are dependent upon the nature of the bottom for their existence, and that their general form is in accord with the habits of an animal that burrows in the deep sea ooze, which has been selected as its best adapted food.

Mr. H. H. Howorth, F.S.A., read a paper *On the Extinction of the Mammoth in Siberia*, the principal object of which was to deal with the difficulties that surrounded the explanation as to the mode in which the animal became extinct. After looking at the problem from every side, he had come to the conclusion that there had been a sudden and violent change of climate in Siberia which had frozen the previously soft ground, and had also preserved the mammoths as in a huge meat safe. Although the mammoth had even originally lived in the place where he was now found, it was impossible that he could live there now, owing to the absence in that part of the food which would be necessary to sustain him. Such trees as he used to live on were only now to be found about 500 miles from the spot where his remains were discovered. The natural corollary that followed from this theory was that something similar must be postulated with regard to other regions. The conditions in which the elephant was found in Siberia were precisely similar to those in which it was found in the north-western part of Russian-America, and precisely the same as those in the Great Lakes, where the mammoth itself was found, and it could not, therefore, be doubted that the mammoth lived in Europe and America with the same food and surroundings as it did in Siberia.

SECTION E.—Geography.

Captain Burton read a paper *On the Land of Midian*, giving an account of his recent explorations in that region, which have already been referred to in these pages.

Dr. Phené read a paper *On the acquisition of Cyprus, and Observations on some Islands in the Levant with Reference to Recent Discoveries.* The author, who had recently made a prolonged and careful voyage of research in the Levant, described the physical features of the islands of Chios, Mitylene, Lemnos, Imbros, Thasos, and Samothrace. He selected Samothrace for ascent, and was, so far as he could learn on the island, the only European, not being a native, who had made the ascent, which was very difficult. The height was slightly over 5,000 feet. The climates, culture, and salubrity of the different islands were dwelt on. Cyprus had a variety of climate, so that the debility produced by the heats in the south could be relieved by a retreat to the northern coast, which was cooled by the breezes coming from the Karamanian Mountains, while in some inland parts were rich woods abounding with game and objects for the chase.

Major Wilson, Director of the Ordnance Survey of Ireland, also read a paper *On Cyprus.*

Lieut. Kitchener, R.E., in a paper *On a Survey of Galilee*, gave an account of the progress of the work of the Palestine Exploration Fund in that region. The great work on which the Society has been employed for the last six years, is a map of Palestine on the model of the Ordnance Survey of England and Ireland. The map of Palestine on the one-inch scale has now been completed. Lieut. Kitchener then detailed the progress of himself and his predecessors in the work of surveying and in exploring ancient remains and sites. At the end of this year, if funds are available, an expedition will start to explore the sites of the most sacred scenes of the New Testament history—the northern shores of the Sea of Galilee, where undoubtedly Capernaum, Chorazin, and Bethsaida still exist. In addition to this the expedition will make a thorough survey of the unknown country forming the eastern shores of that sea, on the same scale and with the same accuracy as the present survey. They hope also to rescue from the hands of that ruthless destroyer, the uneducated Arab, one of the most interesting ruins in Palestine, the Synagogue of Capernaum, which is rapidly disappearing, owing to the stones being burnt for lime.

Mr. W. H. Dall, of the U.S. Survey, gave an interesting account of its recent exploration in Alaska, some of the results of which we have referred to in Nature.

SECTION F.—Economical Science and Statistics.

A paper by Prof. Jevons was read *On the Periodicity of Crises and its Physical Explanation.*—Various reasons, such as wars, trades unions, luxurious living, &c., had been given as explanations of the now constantly recurring depression of trade. Such explanations he (Prof. Jevons) did not consider satisfactory. Depression of trade had occurred during the present century with remarkable regularity at intervals of ten years. Sir John Herschel had attempted to find a connection between certain meteorological phenomena and the price of corn in Europe. If we traced backward from 1866, when a very great depression of

trade took place, it was to be observed that in 1857 an equally severe commercial crisis took place both in England and the United States. The year 1847 was memorable for the excessive number of bankruptcies, and in 1839 and 1836 crises took place in England, while in 1837 the crisis took place in the United States. Some exceptional cause appeared to have broken up the crisis into minor crises. From 1837 they progressed 11 or 12 years to the great bubble year, 1825. The paper went on to show that during the last 165 years there had been 16 great commercial crises at intervals of about ten years and concluded by stating that the fact of periodicity of commercial crises was so strong that it could not be doubted, and the question of a physical cause was only a matter of speculation.

SECTION G.—MECHANICAL SCIENCE.

Mr. G. J. Symons, F.R.S., read a paper *On the Rainfall of Ireland*, in which he mentioned as a remarkable fact that the Irish hills do not exhaust rain clouds as the English hills do. With the exception of a dry central area around Dublin, the rainfall all over Ireland may be taken to be the same. At present, instead of the greatest rainfall being in the south-west, or in Galway, we have the wettest spot of all (with one exception) in the south of the county Down, the very place which, theoretically, might be expected to be almost the driest part of Ireland. That shows that it is really a question more of the elevation of hills than of geographical position. He exhibited a map showing the number of rain stations established for the observation of the rainfall. He had succeeded since the meeting of the Association in Belfast in obtaining the services of a large number of gentlemen volunteers throughout Ireland who had taken charge of the rain-gauges supplied to them, and had engaged to register their observations. There were still large districts, however, in which he had not been able to establish rain-gauges, and the observations were, therefore, necessarily defective as to the average rainfall. There was a large district in the neighbourhood of Longford without a single station. The same could be said of other stations, where it was essential that observations should be taken. If he could induce some gentlemen having property in these neighbourhoods to take charge of rain-gauges, Ireland, instead of having to depend upon ten stations, as it did not many years ago, would be fairly represented both geographically and physically.

Mr. W. H. Preece, C.E., read a paper *On Recent Advances in Telegraphy*, with the effect of showing that improvement in telegraphy was never more active in England than it has been since the Government managed the business. Having indicated the improvements effected, the paper concluded :—"The control of Parliament and of the press exercises a far more disciplinary and supervising power on the management of a Government department than any half-yearly meeting of shareholders, or occasional committee of investigation."

Mr. Wigham read a paper *On the Irish Siren Fog Signal.*—The Irish siren is adapted either for steam or compressed air, and differs from those made in America and in England in being driven by a species of small turbine actuated by the current of the steam or air by which the instrument is sounded, the rate of rotation being controlled and rendered uniform by a simple governor, a much less complex arrangement than the somewhat cumbrous mechanism which has heretofore been used. The Irish siren is applicable to steamships as well as to lighthouses.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

PROF. HELMHOLTZ is succeeded in the rectorship of the Berlin University by Prof. Zeller.

THE University of Halle is receiving valuable additions in the form of new edifices for the various departments. The necessity of new buildings with modern appliances has long been felt, and the want is being met at present by the erection of a library, two new clinics, and pathological, physiological, and anatomical institutes.

SWEDEN possesses at present one of the most thorough systems of education in Europe. It is difficult to find a district where one per cent. of the population are unable to read and write. The common schools of the country cost about 220,000*l.* yearly. There are numerous institutions for agricultural and technical education, and two universities at Lund and Upsala, which compare fairly with the German universities, in regard to the amount of valuable research carried out in various departments.

SPAIN has at present ten universities. Those of Madrid, Barcelona, and Granada possess each five faculties—law, medicine, pharmacy, the exact sciences, and philosophy and literature. In those of Salamanca, Seville, and Valençia pharmacy is absent. Saragossa has law, medicine, and philosophy; Santiago and Valladolid law and medicine; and Oviedo law only. The professors number 414, and the students 15,000; Madrid alone contains, however, 76 professors and 6,500 students. Technical education is provided for by schools of mines at Madrid and Almaden, the agricultural schools of Madrid and Cordova, the veterinary institutes of Madrid, Cordova, Leon, and Saragossa, the School of Architecture, and the school of Civil Engineers at Madrid, as well as polytechnics in various cities. Preparatory schools number 63, with 30,000 students, and elementary schools number 28,000, with an attendance of 1,400,000.

SCIENTIFIC SERIALS

American Journal of Mathematics, vol. i. No. 2.—On the application of the new atomic theory to the graphical representation of the invariants and covariants of binary quantics, by Prof. Sylvester.—Researches in the lunar theory, II., by G. W. Hill.—Bipunctual co-ordinates, by F. Franklin.—Desiderata and suggestions. No. 2. The theory of groups; graphical representation, by Prof. Cayley.—On the electric potential of crystals, by W. E. Story.—Théorie des fonctions numériques simplement périodiques, by Prof. Ed. Lucas.

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, August 19.—M. Daubrée, president.—Meridian observations of the minor planets made at the Paris Observatory during the second quarter of 1878; communicated by M. Mouchez.—Experimental researches on the nervous sudoral fibres of the cat, by M. A. Vulpian.—On a new species of mineral named thaumasite, by Prof. Nordenskjöld.—On the alterations caused by the phylloxera in the roots of the vine, by M. A. Millardet.—Elements of the planet 148, Gallia, by M. Bossert.—Second note on the employment of identities in the solution of numerical equations, by M. Desboves.—Spectrometric study of some sources of light, by M. Crova.—Ambulant electric sparks, by M. G. Planté.—On a telephone able to transmit sounds to a distance, by M. Righi.—On a new improvement in the peroxide of manganese and sal ammoniac pile, by M. Leclanché.—On the dissociation of metallic sulphurs, by MM. Ph. de Clermont and J. Frommel.—On the value of magnesia as an antidote to arsenious acid, by the same.—On two beds of phosphatic lime in the Vosges, by M. P. Guyot.—Researches on the nutrition of insects, by M. L. Joulin.—Researches on the relations of weight which exist between the bones of a skeleton of the goat, by M. S. de Luca.—The new meteoric mineral, Daubréelite; its constitution; its frequency in meteoric irons, by Mr. Lawrence Smith.—New molluscs of the Parisian tertiaries, by M. Stan Meunier.

CONTENTS

THURSDAY, SEPTEMBER 5, 1878

THE ZOOLOGICAL RECORD

The Zoological Record for 1876; *being Volume Thirteenth of the Record of Zoological Literature.* Edited by Edward Caldwell Rye, F.Z.S., &c. (London: 1878.)

ANOTHER volume of this useful annual is now before us. When, about a twelvemonth since, we noticed (NATURE, vol. xvi. p. 357) its predecessor, we took occasion to complain of the ingratitude of zoologists in not giving more efficient support to a work which, if not indispensable to the due prosecution of their studies, would at least lighten their labours in a remarkable degree, and we pointed out how it was a matter of obligation upon all literary and scientific institutions to include the *Zoological Record* among the books they regularly purchase. We fear that our words fell upon dull ears, and that there is as much need now as there was then to impress these considerations on the public. The Zoological Record Association, to judge from its recent balance-sheet, still lives upon charity, to the great discredit of the zoologists of our own tongue as a body, and if its existence be prematurely brought to a close, it will be to their everlasting disgrace. In some respects the energy of zoologists is boundless, in other respects their apathy is amazing. They will compass sea and land to gain one new specimen for their collections, or one apparently new species for their monographs. They think themselves happy in the short-lived glory of being able to inscribe "*Nobis*" or "*In Mus. nostr.*" after its name in some printed list; but they care very little to know what others are doing in the same line of research, and when a few years after, some industrious German or Scandinavian naturalist quietly relegates the name on which they had plumed themselves to the limbo of synonyms (perhaps with a mark of admiration which does not mean praise), they accept the rebuff and console themselves with the reflection that "a fellow can't be expected to know everything," or, if twitted by a friend, will ask in an aggrieved tone whether it is possible for anybody to be acquainted with the contents of two hundred foreign journals. Then, again, there is the obstacle caused by dislike to, or suspicion of, any new thing, from which even scientific men are not entirely exempt. The conchological investigations of Mr. A. or the entomological studies of Dr. B., some thirty or forty years ago, conferred imperishable renown upon the ancient borough of Little Pedlington, in which they were both resident. Their investigations and their studies were accomplished without the aid of the *Zoological Record.* Therefore the *Zoological Record* is of no use to persons engaged in such labours, and therefore the Little Pedlington Literary and Scientific Institute need not go to the expense of adding the yearly volumes of the *Zoological Record* to its bookshelves. Perhaps some of our readers may smile, but we believe this to be no uncommon case, and though the sight of a just man struggling with adversity is said to have been pleasant to the gods of old times, we cannot say that the condition of the Zoological Record Association awakens similar feelings in ourselves.

Now as to the present volume. With the same contributors as the last, it has naturally almost the same qualities; but the editor has introduced a new feature in the separate pagination of many of the records. This scheme has been tried, he tells us, in the hope of saving time, but the very insignificant amount of delay which it seems to have avoided, appears to us but a poor and utterly inadequate recompense for the additional trouble there always is in citing a book so paged, and the amount of confusion to which this manifold system invariably gives rise. Here we have page 1 fifteen times over, and yet one set of numbers runs to 240 pages! We certainly trust the editor will reconsider his determination of continuing this practice, though he holds out hope of its "possibly leading to other improvements." Otherwise we have not a word to say against Mr. Rye's management, which, as before, proclaims his great ability. We venture, however, to throw out a suggestion that perhaps may not be included among his possible improvements. This is that *general* works should receive a separate notice in addition to that which each individual recorder thinks fit to give them. The year 1876 will long be remarkable as that in which Mr. Wallace's great work on the "Geographical Distribution of Animals," appeared; but we must say that the attention it receives in this volume of the *Zoological Record* is utterly unworthy of the magnitude of the subject. Our readers shall judge for themselves. The recorder for *Mammalia*, Mr. Alston, after most properly naming it especially in his preliminary remarks, says of it:—

> "A considerable part of this most important work is devoted to the Mammalia. Besides the discussion of the genera characteristic of each of the zoological regions and sub-regions, the classification of Mammals is considered (i. pp. 85-90), the distribution of the extinct forms (i. pp. 107-160), and that of existing families and genera (ii. pp. 170-254). The author believes that the principal groups first appeared in the northern hemisphere, from which the southern continents were peopled by successive waves of migration."

The recorder for *Aves*, Mr. Salvin, writes:—

> "The class Aves supplies a large proportion of the material investigated in this work, and the distribution of birds entering into the subjects is examined under the the following heads:—(Part i.) The principles and general phenomena of animals. (Part ii.) On the distribution of extinct animals. (Part iii.) Zoological Geography: a review of the chief forms of life in the several regions and sub-regions, with the indications they afford of geographical mutations. (Part iv.) Geographical Zoology. a systematic sketch of the chief families of land animals in their geographical relations. The book itself is full of details most carefully elaborated, and is doubtless destined to be the standard work on the subject for some time to come."

The recorder for *Reptilia*, Mr. O'Shaughnessy, descants at greater length:—

> "The geographical distribution of the families and genera of Reptiles and Amphibians is treated collectively as a section, in chap. xix. of this work, vol. ii. pp. 392-423.
>
> "In discussing the means of Dispersal and Migration of the various classes of animals, Mr. Wallace remarks (vol. i. p. 29) that Reptiles, exclusive of serpents and sea-snakes, being scarcely more fitted than Mammals for traversing seas and oceans, are generally wanting in

oceanic islands which possess no indigenous Mammals; this rule is, however, subject to exceptions among the lizards, which apparently have some unknown way of passing over the ocean (probably in the egg state), as they are found to inhabit many islands where there are neither Mammals nor snakes. Snakes entirely cease at 60° N. lat., and at 6,000 feet elevation in the Alps. Lizards, though essentially tropical, go sometimes farther north than snakes, and ascend higher, reaching 10,000 feet in the Alps. Amphibians extend much farther north; Frogs to within the Arctic circle; their eggs are no doubt carried certain distances by aquatic birds, but salt water is fatal to them, and deserts and oceans constitute the most effectual barriers to their dispersal.

"Further remarks on the possible mode of transport of Reptiles to remote distances are made, vol. i. pp. 400-401, where the author treats of the points of similarity between the fauna of the Australian region and that of South America.

"Describing first in detail the faunæ of the six great geographical regions (Neotropical, Nearctic, Palæarctic, Ethiopian, Oriental, and Australian), the author refers successively to the Reptiles in their subordinate relation to each fauna, and afterwards, vol. ii. pp. 372-423, collects his results and tabulates them under the heads of the different families."

None of the other recorders seem to make mention of the work, except Dr. von Martens, who, under the head of *Mollusca*, contents himself with the following:—

"A. R. WALLACE gives an outline of the geographical distribution of the terrestrial (and freshwater) Mollusca in his 'Geographical Distribution of Animals,' vol. ii. pp. 512-529 and 534-535, and some instances of means for their passive dispersal, vol. i. p. 31."

Now we humbly submit that no adequate idea of Mr. Wallace's work is given by any one of these notices separately nor by all of them combined. As we said on the last occasion, we cannot find it in us to criticise the recorders, though they differ greatly (and this will be evident from the above extracts) in their mode of treatment. But in a case like this the editorial hand might surely be shown with advantage, and none can doubt that in a few sentences Mr. Rye, had his scheme allowed it, would have been able to put the reader in possession of Mr. Wallace's general principles and general results, while the different recorders would still be left to show how those principles and those results affect their respective branches.

It is, perhaps premature to say that the excellent plan of giving an index to the genera and sub-genera recorded as new, and of marking those names that had been bestowed before, has yet had the wholesome disciplinary effect that was expected of it, but there are indications that such is the case. The index to last year's volume showed that *fifty-nine* preoccupied names, implicating *thirty-seven* authors, had been reintroduced to zoological literature in the year 1875. The present volume shows the corresponding numbers to be *thirty-six* and *twenty-eight*—a manifest improvement, though not quite so great as at first sight appears, since in 1875 nearly a *thousand* new genera or sub-genera were instituted, while in 1876 the number is only about *eight hundred and seventy*. No one has again sinned as M. Mulsant did on the last occasion, but it seems strange that so learned an entomologist as Dr. Leconte should now head the list of offenders with *four* homonyms, and we suspect this must be due to a different reading of the laws of nomenclature which may obtain in America. Next to him come Messrs. Cope, Dybowski, Jacovleff,[1] Kirchenpauer, Linstow and Snellen with *two* each, and the rest with one. The selection of the same name, *Coptingis*, for two apparently distinct genera of *Erotylidæ* by M. Chapuis and the late Mr. G. R. Crotch, is curious, and the Arachnid *CorynEthrix* of Dr. Koch, and the Thysanurous *CorynOthrix* of Herr Tullberg seem to clash with one another. Collisions of this kind are, of course, unavoidable, but of the three dozen homonyms which come into the crop of 1876, a score-and-a-half might certainly have been avoided had their authors but followed the advice of Mr. Rye's motto:—

"Explorate solum: sic fit via certior ultra."

That is to say had they consulted their Agassiz's *Nomenclator* and used the *Zoological Record*.

It remains for us to say that the present volume contains an abstract of the zoological portion of more than two hundred-and-fifty distinct periodicals, besides separately published works, and that those journals hold a good deal hardly any one requires to be told. Nevertheless, it may be new to some of our readers to learn that papers which have appeared in the older volumes of many of these periodicals are in so much request that lists of them, with the proper pagination, are being reprinted. This has been done in the *Deutsche entomologische Zeitschrift*, by Herren von Heyden and F. Blücher, with regard to the entomological articles in the first sixteen volumes of *Der zoologische Garten*, the first thirteen of the *Verhandlungen des naturforschenden Vereines in Brünn*, four volumes of the *Bulletin de la Société des Naturalistes de Moscou*, and fourteen of the *Archiv für Naturgeschichte*. Trusting that next year we may be able to congratulate Mr. Rye and his fellow-labourers on having a more promising prospect before them, we bid them be of good cheer, for they have the sympathies of all who know how to appreciate hard and honest work.

OUR BOOK SHELF

Annual Report and Transactions of the Plymouth Institution and Devon and Cornwall Natural History Society. Vol. VI. Part II. 1877–78. (Plymouth: Bredon and Son.)

THIS Report seems to us to deserve more than the passing notice we gave it in a recent note on the Reports of provincial societies. The society embraces a wide range of work—science, history, archæology—and many of the papers which it publishes will compare favourably with papers of a similar class read in metropolitan societies. The society has a large membership, and valuable collections in various departments. In the Report before us the president, Prof. Anthony, discusses various interesting points in connection with the doctrine of evolution, and although he holds the theory to be "not proven," his discussion of the subject is fair. Mr. R. N. Worth, a great authority on most subjects connected with Plymouth, has papers on "The Palæontology of Plymouth," "The Early Commerce of Plymouth," a paper of much interest showing considerable research, and "The Ancient Heraldry of Plymouth." Mr. R. Briggs's paper on "The Hedgerows

[1] It is much to be wished that there were some recognised way of rendering Russian proper names into the languages of Western Europe. Germans, Frenchmen, and Italians, each render them phonetically, and of course the name is differently spelled according to the nation of the writer. Mr. Jacovleff's name thus appears also as Jakowleff, Yakovleff, and Jacovlev!

of Plymouth," is valuable and readable, containing the results of careful observation. "A Catalogue of the Geometrina of Plymouth and its Vicinity," by Mr. G. C. Bignell, will interest entomologists. "Our Obligation to Greek Thought," by Prof. Chapman, is a thoughtful paper, and there are several other excellent papers of historical and antiquarian interest.

Vulcanologische Studien. Von Dr. Eduard Reyer. (Wien: 1878.)

WE have already had occasion to direct attention to the valuable contributions to the theory of volcanoes which have recently been published by Dr. Reyer, of Vienna. The memoir before us fully maintains the reputation of its author as an able investigator and original thinker. In it he discusses the nature of the materials which remain in the throat and deeper portions of a volcanic vent, after the eruptive action has ceased, and the features presented by those volcanic cones which are formed not by violent explosive action but by the quiet outwelling of liquid lava. Dr. Reyer's remarks on both these questions will be found to be eminently valuable and suggestive. J. W. J.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Is the Sun One-sided?

WHEN Broun and Hornstein detected the existence of a terrestrial magnetic inequality, of which the period is nearly that of the sun's rotation, it was natural to regard this inequality as a direct result of the rotation of our luminary. Nevertheless, there are grave reasons against this hypothesis. In the first place, it is extremely difficult to imagine the sun to be one-sided in its magnetic influence. From what we know of our luminary, it must in a great measure be composed of gaseous matter, of which the outer layers are in violent motion, so that we can hardly imagine one meridian to be permanently different from another.

Another objection is derived from the fact that the period of this peculiar magnetic inequality (of whose existence there can be no doubt) is very nearly that of the sun's rotation in space, and decidedly less than that of its synodic rotation. Now, if we can imagine the sun to affect the earth in this peculiar manner, we should be inclined to suppose that the period of such influence would be that of its synodic rotation, that is to say, of its rotation with regard to the earth.

But if this inequality be not due to the sun's rotation, how (it may be asked) is it possible to account for it?

In the first place there is accumulating evidence of the existence of an intra-mercurial planet with a time of revolution, not differing greatly from that of the sun's rotation. Again, there can be little doubt that we have various magnetic inequalities, of which the periods are the same as those of the most prominent planetary configurations. May not, therefore, this magnetic inequality be connected with the intra-mercurial planet of the existence of which we are becoming sure, and is it not possible that a discussion of magnetic changes may aid us in settling this very interesting and important question?

St. Andrews, August 26 BALFOUR STEWART

Von Cotta's "Geologie der Gegenwart"

YOUR journal contains, in the number for August 8, a short notice of my newly-published "Geologie der Gegenwart," with a signal misrepresentation of the coloured frontispiece, beginning with the lines, "We can scarcely regard," to the end of the paragraph.

The illustration in question really is a representation of the fact that igneous rocks—both volcanic and plutonic—being originally products of the central parts of earth, form different species, according to their being rich in silica (acidic rocks) or poor in silica (basic rocks), and according to their consolidation at a considerable depth as *plutonic*, or nearer the surface as *volcanic* rocks. Becoming solid at a great depth, the *acidic* fluids have formed granites or porphyrites; nearer the surface, trachytes or trachytic lavas. The *basic* fluids, on the other hand, became syenites or greenstones at a great depth, and basalts or basaltic lavas nearer the surface.

All these various rocks by no means belong to particular geological periods, but *at all times* have either overflowed or penetrated other formations, and been accompanied by tuff formations.

The misapprehension contained in your notice is doubly painful to me, because I think I have been the first German geologist who decidedly adopted Lyell's view with regard to the utter independence of the nature of rocks from their geologic age: witness the first edition of my "Geologie der Gegenwart," published in 1866. BERNHARD V. COTTA

Freiberg

On the Wax of Pœciloptera

A SPECIES of Pœciloptera was this year rather common in the neighbourhood of this city, covering thickly the branches of *Cassia obtusifolia*, L., and more sparingly of *Cassia spectabilis*, D.C. I have not the means of identifying the species; I inclose, therefore, the wings of a specimen, so that some entomologist may give you the right name.[1] The females of Pœciloptera, as indeed of many other Fulgoridæ, are known to have the property of secreting a wax-like substance from between their abdominal rings, and especially from peculiar appendages of the last ring. This substance is, in the present species, of a beautiful white colour, glossy like silk, and formed of exceedingly thin threads, 1·500 to 1·700 of a millimetre thick, and generally less than a centimetre long. When taken off the living insect, the latter will be found to produce new threads in somewhat less than twenty-four hours. The threads are pure wax, lighter than water, insoluble in cold alcohol and ether, but dissolving a little in hot alcohol, and very easily in hot benzol. The fusing-point I found by repeated experiments to be *a little higher* than that of boiling water, though I could not determine it exactly, owing to the small quantity of wax I had collected (from 150 insects I obtained but six centigrammes of wax). In a heated silver spoon, or on platin-foil, the wax melted very easily, leaving no residue whatever. The molten wax was at first of a light yellow colour, which disappeared again after its getting cold.

The late Mr. D. Hanbury, in a paper on the insect-white-wax of China ("Science Papers," 62), quotes the statement of Capt. Hutton as to the properties of the wax of *Flata limbata*, an insect closely related to the Pœciloptera. It is said to "dissolve readily in water, while the attempt to melt it on the fire without water or oil proved altogether abortive, the wax merely burning and consuming away till it became converted into a hard and baked substance." This is certainly very singular, and it appears to me highly questionable whether Capt. Hutton's so-called *wax* was any wax at all.

I have not been able to find out what can be the benefit the insects derive from these copious secretions; but as they occur only in the females, there will probably be some connection with the egg-laying or hatching process. I observed no males, and could discover no eggs even in places where the females were thickly crowded on the branches. I should add that the insect is generally rather uncommon in our local fauna.

Carácas, July 15 A. ERNST

Spontaneous Combustion of Wasps' Nests

SOME time ago the house of General P. M. Arismendi (now Consul of Venezuela, in Port-of-Spain, Trinidad) in this city, had a rather narrow escape from being set on fire by the spontaneous combustion of a large wasps' nest (a species of *Polistes*) in a closet under a roof. The day was exceedingly hot; but this circumstance, I think, has a very slight connection (if any at all) with the outbreak of smoke from the nest. Roofs in this country are constructed of tiles supported by a thick layer of compact earth, which rests on the usual lath-work of dry canes (the stems of *Gynerium saccharoides*, or arborescent grass), both being substances that conduct heat very badly.

[1] An entomological correspondent informs us that the wing is that of *Pœciloptera phalænoides*, Linn., and agrees with Stoll's figure of the insect from Surinam.—ED.

The source of heat must therefore have been in the nest itself. In bees-hives the temperature rises sometimes as high as 38° C. (*teste*, Newport, as cited in Girdwoyn, "Anat. et Physiol. de l'Abeille," p. 23). We may be allowed to suppose that something similar happens occasionally also in wasps' nests. Such a heat might be caused by an alteration beginning in the wax, hydrocarbons being formed, which, on being absorbed by the paper-like, porous substance of the cell-walls, must get still more heated, so that a comparatively small access of oxygen would be sufficient to set the whole nest on fire.

I have been assured that the spontaneous combustion of wasps' nests is a well-known fact in the interior of Venezuela, and as I do not recollect having found it mentioned in books, it appeared to me worth while to inquire whether something similar has been observed in other parts of the world, and if so, whether my explanation will hold good in all cases.

Carácas, July 15 A. ERNST

Observations on a "Dust-Whirl"

WHILE making magnetic determinations at Kirksville, Mo., several "dust-whirls," or small whirlwinds, were observed, which, although not destructive in their effects, were unusually violent. The dust was carried by strong surface-winds, which rushed inward to the centre of the whirl, rising in a vast column 200 feet high.

One of these whirls crossed a pond of water, moving very slowly, and in a zigzag path. The water immediately became agitated, a *depression* was formed, which extended to the bottom of the pond (which was about five feet in depth). The top of the cup-shaped depression was about six feet in diameter, the bottom about four or five feet. There was no water drawn up from the pond, so far as could be observed, although a little of the rapidly whirling water at the edge of the depression was thrown outwards upon the surface of the pond.

Kirksville is situated in the northern part of the State of Missouri, and on the summit of the divide between the Mississippi and Missouri Rivers. During the present summer it has had the least rain in the State, and is yet parched by excessive drought. FRANCIS E. NIPHER

August 5

The Telephone

ON to the centre of a telephone vibrating disc, and perpendicular to its plane, a light needle ½ inch long was soldered, the disc held in a holder, mouth-piece, &c., the same as a telephone, and so arranged that the needle would vibrate in a small cylindrical ebonite cup, ¼ inch in diameter and ⅜ inch long, the top of the ebonite cup just free from touching the vibrating disc; a copper wire was let into the centre of the bottom of the cylinder, flush with the bottom; the cylinder was filled full with the finest dust of iron filings well shaken down.[1] A battery circuit was then completed with connecting wires, through the vibrating disc, iron filings, the copper wire let into the bottom of the cylinder, and through a pair of telephones in a distant room; after finding out by experiment the distance off the speaker ought to stand when speaking to this transmitter, and the proper degree of loudness he ought to give to his words, the voice came as clearly (and louder) as if a telephone had been used for a transmitter. If the speaker came too close or spoke too loudly the words were indistinct, and mixed up with a fizzing sound. In the experiment which was most successful the speaker was about 18 inches from the transmitter and spoke in an ordinary tone of voice. But this distance I found to vary with the thickness of the vibrating disc; a thin disc transmitted clearly only when the speaker was 3 feet off.

The ebonite cylinder was also filled with water (distilled) as an experiment, instead of iron filings. I thought that possibly the resistance of the circuit would be altered through the water, as the needle vibrated to and from the copper wire opposite to it; but no sounds were found to be transmitted. If the resistance of the circuit was altered when the disc vibrated in this experiment, it might tend to support the idea that alteration in the resistance of a circuit was not the only thing required to cause sound to be transmitted, but that "false contact" was necessary, such as would take place when the needle vibrated in the iron filings. G. R. R. SAVAGE

Roorkee, July 8

[1] The disc well tapped with the hand so as to loosen the filings round the needle.

The Electro-Magnet a Receiving Telephone

THE result I have arrived at whilst experimenting in this direction seems so interesting, and at the same time, I believe, novel, viz., *that a good receiving telephone can be made from electro-magnets alone without any vibrating diaphragm*, that I hope by prior publication, to prevent the possibility of a string of those patents which nowadays so greatly hamper true scientific invention.

In my earlier experiments I made an electro-magnet out of a piece of ⅜-inch iron gas pipe ⅜ inch long, filed flat on one side, and split sufficiently only to allow the wire (No. 24) to be wound on, which was done till it was full inside. The poles were therefore together about ⅜ inch square. This was fixed inside a small cigar box, under a ferrotype plate, covering a rectangular hole cut in the lid 2¼ inch by 1½ inch. With a Hughes' carbon-pencil-microphone tilted to an angle of 45° as a transmitter, a small musical box as a source of sound, in a distant part of the house, and one of Leclanché's cells in circuit, this box gave out tunes plainly heard by all sitting in the room.

Whilst experimenting with another similar magnet, I stood it loose, poles downwards, but still connected with the line wires on a flat tin gunpowder canister with the ends cut off, but still retaining the paper label on which the magnet lay; the tune of the musical box was given out loudly and resonant, but buzzing and jangling; also words spoken to the transmitter were heard, but confused together.

Now this was a very interesting result, which led to the next discovery; for, having a small ordinary electro-magnet with its armature in front (as used in electric bells), fixed to a piece of board, I was about to unship it to try experiments in various tin cans, &c., when it occurred to me to connect it as it was, to the line wires, placing only a slip of paper between the poles and armature to prevent actual contact. To my astonishment, on putting the ear close to the board at any part, the music of the box was heard clearly, every note from highest to lowest being distinctly given. Now here seemed to be a telephone without a vibrating diaphragm; but, to make more sure, the armature was unscrewed from its support and attached to the magnet only by an india-rubber band, with the slip of paper between it and the poles, so that it touched no other part of the apparatus. On listening to the supporting board, the sounds were heard as distinctly as before.

But even here forensic ingenuity might claim and attempt to prove that this ordinary armature was a vibrating diaphragm; therefore, an armature being itself nothing but an induced magnet, it was replaced by another electro-magnet, thus:—

Two ordinary electro-magnets (unscrewed from a couple of large electric bells) were fastened, by means of two little wooden saddles and a screw each, to a small piece of deal board about 4½ inches square and ⅝ inch thick, in such a way that the poles were all but touching. Their wires were then joined so that poles of opposite denominations faced each other, *i.e.*, north opposite south and *vice versâ*. This placed on an empty cigar-box and four Leclanché cells in circuit, gave out the tune of the musical box clearly and loudly in the room. When both poles were made to touch, the sound ceased; but with a thin piece of paper or stout tin-foil between them, without any intervening air space, the sound was heard. On gradually separating the magnets, the sounds grew fainter and fainter, till they became inaudible.

By putting the base-board close to the ear, whistling and singing to the microphone were very clearly and loudly heard, also the voice of the person speaking could be recognised; but words were hardly sufficiently defined to distinguish all that was said, though now and then parts were intelligible.

One of the electro-magnets was afterwards replaced by a small permanent steel horse-shoe magnet fastened to the board in a similar manner, the result was the same, but, I imagined, slightly louder, probably from there being less resistance.

By varying the strength of battery, size, or mode of mounting magnets, or adjustment of the microphone, I have no doubt that perfect definition can be obtained. The loudness and volume of the sound are ample; but before making further experiments, for which I have at present little time, I hasten to communicate the *fact* that *the electro-magnet, without any diaphragm whatever*, can be made a reproducer of sounds transmitted by a Hughes' microphone, and thus *a complete and practical telephone system produced without the possibility of infringing anybody's patent.*

I must add that the same arrangement is also a feeble transmitter, using a good Bell's telephone as a receiver, which is a very strange fact. I abstain at present from all theory on the subject. F. G. LLOYD

The Sea-Serpent Explained

On Monday, August 5, a number of geologists crossed in the Folkestone boat to Boulogne, to study the interesting formations of that neighbourhood, and, when about three or four miles from the French coast, one of these gentlemen suddenly exclaimed, "Look at that extraordinary object passing across the bow of the steamer, about a mile or a mile and a-half in advance of us!" On turning in this direction there was seen an immense serpent, apparently about a furlong in length, rushing furiously along at the rate of fifteen or twenty miles an hour; it was blackish in front and paler behind; its elongated body was fairly on the surface of the water, and it progressed with an undulating or quivering motion: *mirum erat spectaculum sane*.

Of course many suppositions were immediately started to account for this extraordinary phenomenon, but they quickly changed and settled into the fixed idea that the object before them could be nothing less than the great sea-serpent himself; for,—

> "Prone on the flood, extended long and large,
> Lay floating, many a rood, in bulk as huge
> As whom the fables name of monstrous size,
> Leviathan; which, God of all his works
> Created hugest, that swim the ocean stream."

The writer fortunately had with him one of Baker's best opera-glasses, and, after a few moments' use of this little instrument, the wonder was satisfactorily resolved. The first half of the monster was dark and glittering and the remainder of fainter hue, gradually fading towards the tail. The glass did not determine the matter until the extreme end was reached, and then it was seen to consist of a mass of birds in rapid motion; those that were strong on the wing were able to keep well up with the leaders, and so make the head appear thicker and darker by their numbers, whilst those that had not such power of flight were compelled to settle into places nearer and nearer the tail. Doubtless these birds were shags (*Pelicanus cristatus*) returning to their homes for the night from the distant waters in which they had been fishing, during the day; perchance it may be wrong to assert positively as to the variety of bird, but inasmuch as the writer has often seen shags on the Cornish coast in smaller numbers returning in single or double file to their roosting places, and since it is stated in works of natural history that they have been noticed occasionally flying in this peculiar manner to the number of a thousand or more, it does not appear an unwarranted liberty in supposing that they really were *Pelicani cristati*.

It is to be feared some of the geological gentlemen still doubt the interpretation of the lorgnette, preferring the fond deceit of a large and unknown serpent; but as in this case individual birds (scores of them) were distinctly seen flapping their wings, the writer has thought it his duty to report the circumstance to you that your readers who voyage across the seas may keep their opera-glasses in their pockets and verify for themselves, on the first opportunity, this interpretation of the great sea-serpent. JOSEPH DREW

4, Foxgrove Road, Beckenham, Kent

Parental Affection in Sparrows

I SAW a touching little incident showing the affection of sparrows for their young on the Kennington Oval cricket-ground last Thursday afternoon, a description of which you may, perhaps, think it worth while to record.

The afternoon was fine and the ground was surrounded by a dense ring of spectators, when a young pale-coloured sparrow, under the guidance of both its parents, was trying to acquire the use of its wings. A slight wind was blowing towards the spectators, and the poor little bird, in its weak attempt to fly, was, to the evident consternation of its parents, carried straight into the laps of the inner ring of spectators, one of whom caught it gently in his hand and held it.

When taken hold of the young bird gave two or three chirps or calls for help, and the old birds flew to within a few feet of the ring of spectators, and, alighting on the grass in front of them all, began to "beg" for the young bird in the most touching and beseeching manner. This they did by lowering their heads and making the peculiar flutter of the wings by which young birds beg for food from the old ones. This singularly touching appeal moved the hearts of many in the crowd, who called out—"Look! look at the old birds!"—"Don't hurt the young bird!"—"Give it back to them," &c. The anxiety and the boldness of the old birds and their humble beseeching for the young was so evident as to come home to the hearts of these somewhat rough spectators.

My own feeling certainly was that I could not have believed that a pair of sparrows could possibly have "begged" with such touching humility and tenderness for the safety of the young bird. Their manner clearly displayed their sense of their own want of power to help the object of their affection, they therefore prayed for mercy in their own way, and with so much feeling, as to excite the full sympathy of the crowd looking on, and to make them, for the time, forget the game of cricket they had come there to see. C. R.

Bristol, August 17

PHYSICS IN PHOTOGRAPHY

In taking a retrospective glance at the remarkable phenomena exhibited in photography, an endeavour will be made to explain them as far as possible by the light that may be thrown upon them by modern research, and at the same time to suggest extensions which probably may be given to this branch of science by further investigations. We may perhaps be open to rebuke from some for venturing to call photography a science; but surely as long as there are problems in it to be solved which require direct scientific solution, and which perhaps indirectly lead to the research in other directions, so long, at least, must it be something beyond a mere industrial pursuit. It is not the fashion to deny to electricity the honourable distinction of being a science, although it has become an industry in its application to telegraphy; why, therefore, it should be considered correct to consider the study of the chemical action of light upon compounds as something to be remitted to the intellect of those who are merely interested in it commercially, it is difficult to understand. It would surely be much better that men of science who employ photography in their laboratories and observatories, should endeavour to understand the science of attack with the weapon they are using, instead of regarding it as a simply mechanical agency, which is only worthy of the attention of, perhaps, a half-educated assistant. If our men of science who employ both were to be as ignorant of the principles of electricity as they too often are of those of photography, research would be very much restricted in its results; and it may, it is believed, be said with truth that a familiarity with even the first principles of photography would very much extend it. We may remark, by the way, that to instil a love of science into youth, an education in photography would seem to be of great value, as experiments can be made which have a real meaning to the experimenter, and which, by allowing an almost endless variation, offer an unlimited field for the exercise of the reasoning faculties. A study of photography, in short, must encourage the study of chemical and physical sciences, if a distinction may be made between the two.

Photography must undoubtedly be divided into two distinct branches: the direct production of the visible image by light itself, and the development of the invisible image by chemical means. The recognition of the former we owe to Wedgwood, and of the latter to Daguerre. The discovery of the former is much less remarkable than of the latter, since, without any particular research, a discoloration of a compound by light must have been noticed, whereas the development of an invisible image would have been a matter of theoretical reasoning, unless accident showed its feasibility. We know that the development of Daguerrean images was discovered accidentally by Daguerre, and we also know that the development of the image on paper was discovered accidentally by Reade. Without two such wonderful strokes of good fortune the growth of photography might have been retarded for years. The years which succeeded the discovery of the developable image were productive of research into many of the phenomena exhibited by the action of light on sensitive compounds, and, owing to the great intellects

who gave their attention to it, many important problems in photography were solved. Succeeding these years, however, were others in which little was done in the absolute science of the subject, though great progress was made in perfecting the processes which had been brought forward. Within the last few years a fresh start in research in all directions seems to have been made, and much that is valuable in elucidating the correct theories on which photography is based has been demonstrated, and it is to this to which attention will be drawn.

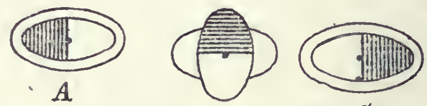
With the risk of being tedious, ground which has been well trodden must once again be briefly gone over, in order to estimate the progress which more recently has been made. Scheele, the Swedish chemist, as is well known, found that the blackening of silver chloride (which was the basis of Talbot's pictures) gave up chlorine on exposure to light, thus proving, as it were, that the blackening was due to the formation of a new chemical compound. As far as can be traced not much more was known regarding this compound; but it was a generally-received notion that it was a subchloride of silver; and up to the present time we find that such is the accepted opinion. In the second edition of Hunt's "Researches on Light," published in 1854, at p. 79, a remarkable experiment is noted. He says:—"The exposure (of silver chloride) in the water was, in another case, continued for several days, but no greater degree of darkening occurred; but a curious fact was noticed. It was found that during the night nearly all the chlorine which had been liberated during the day was recombined, and that the darkened powder became lighter" . . . . He then, after recounting other experiments, says (p. 123):—"From other experiments I am inclined to believe that the first action of the solar ray is to liberate one half of the combined chlorine, which is very readily, moisture being present, replaced by oxygen. By the continued action of the exciting cause the oxide is decomposed, and metallic silver in a fine state of division, is formed over the surface" (of the paper).

P. 125:—"The absorption of oxygen, or rather its combination, with the decomposing chloride is proved by another very easy experiment. Some pure chloride of silver was arranged in a bent tube closed at one end, and the other end immersed in a bottle of distilled water. In this state the chloride was exposed for many days to the action of sunshine, during which time it was frequently shaken for the purpose of exposing the whole of the powder and its influence. As the chloride darkened, the water rose in the tube, and it gave a precipitate of chloride of silver on the addition of the nitrate, thus appearing to prove the substitution of oxygen for chlorine under the agency of solar radiation. It was quite evident that some absorption of atmospheric air had taken place. This explanation will also serve for the iodide, bromide, and some other salts of this metal (silver)."

This last experiment has lain fallow for years, and it is only recently that it has had any meaning beyond that indicated in the quoted paragraph. It must be borne in mind, however, that the visible change in the chloride is here under consideration, and that the invisible effect of light was not mentioned.

With regard to the developable and invisible image, till within the last few years, it was a debatable point as to whether the action of light on a sensitive compound was really a chemical change, or simply a physical action; one school held that the sensitive compound was not altered in composition at all, but that in some mysterious manner the atoms of the molecules composing it were shifted, and possessed a new property which was denied to it in its original form. Diagrams were introduced to render this subtle change clear to the student, one of which is reproduced (Fig. 1).

In A we see two ovals slightly differing in size, each of which was intended to indicate one of the atoms composing a molecule of the sensitive salt. When the ovals coincided, the molecule was supposed to be in the ordinary state, but after light acted upon it for a certain time the ovals occupied the positions shown in B, and after a further action of light they occupied the positions shown at C, in which it again became incapable of proper development, and gave rise to what was known as *solarisation*, the part of the "*latent image*" formed by these solarised molecules refusing to *develop*. By solarisation was meant the phenomenon which occurred (more especially if silver films containing iodide were used), when any portion of the plate received a lengthened exposure to any very bright part of the lenticular image, such as to that of the sky. In solarisation we have have a term which is as unmeaning as is "polarisation" in some of its applications, but since it has passed into the technical language of photography, we are bound to employ it. By the term latent image was meant the invisible (and usually) developable image impressed upon a sensitive film, and it will be used, where convenient, with the reservation once for all, that its applicability is not admitted any more than is the term "developer," as applied to a solution which may cause the deposition of metallic silver from a solution of silver nitrate; since such a solution is effective whether applied to an exposed sensitive film or not. The advance of photography has literally been impeded from the neglect of using accurate language. As regards this peculiar condition which the molecule was supposed to have attained after its impact with light, there seems to be no ground for its adoption. The idea seemingly arose from a supposed necessity which existed for a difference in condition between the visible and the



A B C

Fig. 1.

merely developable image. By a strictly logical inference there need be no difference between the two beyond this that there should be a difference in the number of molecules absolutely altered, and in no other respect. Perhaps the most telling experiment giving *direct* evidence of the similarity of the two images was that made by Poitevin, in which he proved the dissociation of iodine from silver iodide, by placing metallic silver in contact with the film. After exposure to light, on separating the two, he found that the latter had absorbed iodine, as proved by treating it with mercury vapour. The *circumstantial* evidence of the truth of the chemical theory of the invisible image, however, is so strong, that on that alone we are bound to accept it, at the same time we are not prepared to say that there are not other physical forces which must play a part in its development; in fact, it must be so. We may say, then, that at its *first formation* the developable photographic image is formed by the reduction of the sensitive compound to one of a less complex nature. Thus, silver chloride (argentic chloride) is reduced to silver sub-chloride (argentous chloride) with the liberation of chlorine; and silver bromide to silver sub-bromide with the liberation of bromine; and so on.

We must now allude to the development of the photographic image. We may divide the methods of development of the image on silver compounds into three: (1) The condensation of mercury; (2) the deposit of metallic silver from a soluble salt of silver by means of a reducing agent such as ferrous sulphate; and (3) the reduction of the sensitive salt of silver itself to form the image.

The first method is the earliest, dating from the discovery of the Daguerrotype process, and till within very recent times the reason of its efficacy has been a subject of controversy. Quincké has lately thrown a light upon

it in one of his memoirs, and his explanation seems to account for it in a most perfect and philosophical manner.

In the Daguerrotype process, it will be remembered, a silvered plate is subjected to the vapour of iodine (or of iodine and bromine), and thus receives a fine layer of a compound which is sensitive to light. When a plate so prepared is exposed to a lenticular image in the camera, the light causes the iodide (or bromoiodide) of silver to throw off iodine (or this together with bromine), which is immediately seized by the silver beneath, and thus forms a deeper layer of the sensitive salt. The depth, almost immeasurable though it be, depends on the intensity of, and length of exposure to, the light. (That this is the case has been proved by the fact that, if the sensitive layer be removed by a suitable solvent, the surface beneath is shown by reflected light to be etched to a greater or less degree.) The invisible image thus formed is exposed to mercury vapour, and the dew condenses on it proportionately to the depth of the layer. Quincké, in his memoir "On the Edge Angle and Spread of Liquids on Solid Bodies,"[1] shows that the edge angle of a drop of liquid on a solid body varies from zero to a constant quantity, according to the thickness of any fine layer of impurity which may be on the latter. When this layer attains a certain value then the edge angle of the drop will remain constant. The thickness, or rather the thinness, of the layer may be appreciated when it is stated that it bears a relation to what is called "the radius of the sphere of sensible action of molecular forces," and is usually greater than ·00005 millimetre. In this case the sensitive plate is the solid body and the invisible image forms different thicknesses of impurity. By this difference in the edge angles of the mercury dew, condensed on different portions of the latent image, the light is reflected in different ways, which gives rise to the visible image.

This explanation entirely does away with the necessity, which previously seemed to exist, of the silver iodide (or bromo-iodide) being reduced to the metallic state, in order to cause condensation, or—perhaps it might be said—to cause the formation of an amalgam of mercury and silver.

The next method of development speaks for itself; the metallic silver is deposited in fine granules and is attracted by the salt which has been altered by the influence of light. Perhaps further investigation will show that development is dependent on what is known as the Brownian movement, or the rapid movement of small suspended particles in a liquid. If this movement be dependent on the electrical condition of the neighbouring body, as has lately been supposed; and if, as Dewar has shown, the condition of an exposed sensitive salt is electrical, then the deposition of the metallic particles of silver on the image is accounted for in a satisfactory manner.

The last mode of development is principally employed with silver bromide, and is known as the alkaline method. When a film of collodion or gelatine holds a sensitive salt on a plate, the portions exposed to light are reduced to the metallic state by the application of an oxygen absorbent such as alkaline pyrogallic acid. Since the image is invisible, it must be remembered that but a few molecules of the sensitive salt are reduced by the action of light to the less complex and developable form, we therefore must look for some further action between the developer and the rest of the unaltered compound. It has lately been proved that silver bromide or silver chloride cannot exist in *close* contact with metallic silver. It invariably forms the developable salt. Thus if we take a glass plate, silvered by any of the well-known processes, and expose it to the fumes of bromine or to hypobromous anhydride, it will be found that it is impossible to secure a film of argentic bromide until the last trace of silver has been attacked, after which the true colour of argentous bromide gradually gives way to the well-known colour of argentic bromide. We may try the experiment with bromine water and the same holds good. The action of chlorine on silver is the same as of bromine, but the action of iodine seems to be different, the fully saturated compound, argentic iodide, being formed at first. In other words, this compound is the more stable than argentous iodide.

Now the alkaline developer, when mixed with a soluble bromide of an alkali, has the property of much more readily attacking the argentous than the argentic bromide, presumably because the soluble bromide used in development combines with the former, giving rise to an apparently difficultly reducible compound, whilst it refuses to combine with the argentous salt. It is thus easy to see, if this property of the developing solution be connected with what was stated in the preceding paragraphs, how development takes place. The developer is applied to the exposed film, and the minute quantity of argentous compound is reduced to the metallic state, and at once this particle of silver which is in close contact with the unaltered compound combines with it and forms new argentous bromide. This is ready for attack by the developer, and thus the action spreads till the whole thickness of the sensitive salt is reduced to the metallic state where the greatest exposure has taken place. An interesting result[1] of this action is afforded by the fact that, if a film of unexposed argentic bromide be superposed over one that has been exposed, the image impressed on the latter can be developed in the former so long as close contact is secured. It has been said that this action is due to the solubility of the silver-bromide used in the alkaline development; and, to some extent, this is true; but it is evident that this cannot be explanatory of the whole phenomenon, since the same effect is produced by using, with the pyrogallic acid, potash as the alkali in which the silver-bromide is absolutely insoluble. We have been thus particular in showing the cause of this alkaline development, as it explains some phenomena to which attention will subsequently be called, and which otherwise would be inexplicable, except by reversing usually-accepted physical laws.

W. DE WIVELESLIE ABNEY

(*To be continued.*)

[1] *Phil Mag.*, May and June, 1878.

[1] *Phil. Mag.*, January, 1877.

MILITARY BALLOONING

THE matter of ballooning for military purposes appears to be once more attracting attention in this country. In France they have now a properly organised service under the command of a colonel of the National Engineers, who considers all novelties and proposals as they arise, and who sees, moreover, that the State has always a body of skilled aëronauts at its disposal. At the end of the Paris siege the Postal department, it may be remembered, possessed a large number of balloons, and these being handed over to the French war minister, constituted the *matériel* necessary in the formation of a military balloon service. Col. Laussedat, whose name as an energetic officer of the French Topographical Department, is well known, was placed in command, and he at once secured the services of one of the Messrs. Goddard to put the whole of the apparatus in a fit condition for service. Since that day ballooning in France has been considered as much a duty of the Engineers of the army as telegraphing and surveying, and classes both for officers and men are held for instruction. Lately, by the resignation of Col. Laussedat, the French balloon service has lost its chief support; but his place has just been supplied by Gen. Farr, who will, no doubt, take measures to maintain the high efficiency which has been attained by his predecessor.

In France, as in this country, the balloon is chiefly

regarded by military men as an important means of reconnoitring. The Paris photographer and aëronaut, Nadar, was successful on several occasions in securing photographic records from balloons, but he never published his *modus operandi;* and the problem of balloon photography is one which still excites a good deal of attention. Mr. Walter Woodbury, the well-known inventor of Woodburytype—the only practical photo-engraving process we know—submitted, during the last war, to the Russian government, a very ingenious method of securing pictures at an altitude. By his plan no one ascends with the balloon at all, and therefore the latter may be of very limited dimensions. It is captive, and twisted into the tethering rope are insulated wires in connection with a camera. The camera is weighted and hung upon a pivot so as to be always horizontal, and a fan attached to the balloon prevents the same from gyrating. It is easy to understand how a lens may be capped and uncapped from below with the aid of an electric current, and the photographs are secured—for a series may be taken at one ascent—upon a length of sensitive tissue which is unrolled for use through the medium of clockwork. The sensitive tissue and roller arrangement is that of M. Warnerke, which is known to all dry plate workers, and which permits of securing pictures without glass. Mr. Woodbury's invention has, so far, been tested only in respect to its photographic properties, but in cases where an aëronaut would run too much risk, or where a large supply of gas is not available, the apparatus would be well worthy of trial.

It is the difficulty of securing a sufficiency of gas for inflation that at present stands in the way of employing balloons in the field. The French balloons are all large ones, for they were constructed most of them for postal service during the siege, and, besides the mails and aëronaut, sometimes carried three passengers. With the exception of half-a-dozen, all the balloons which left Paris had a uniform capacity of 2,000 cubic metres, while one, in which M. de Fonvielle and three other persons travelled from Paris to Louvain, measured 3,000 metres. Such bulky balloons as these are unsuited for the field, where the problem is to send a single observer aloft with the minimum amount of time and trouble. The smallest balloon and the lightest gas for the purpose are what the soldier seems to require, and it is towards these two points that attention has lately been directed by Capt. Templar and the other officers who are just now occupied in the study of aërial navigation in this country. Naturally enough, hydrogen holds out the most promising features as a lifting medium, and it is with this gas that experiments are once more to be made. As our readers remember, the weight of hydrogen is calculated to be 2·14 grains per 100 cubic inches, while air on the other hand weighs 31 grains; and, as the lifting power is represented by the difference between these numbers, it stands to reason that theoretically, a balloon, if filled with hydrogen, need be of but comparatively very small dimensions. Unfortunately, in a practical affair like ballooning, a lot of accidental matters require to be taken into consideration, and two of these are the facts that it is difficult to secure pure hydrogen, and more difficult still to keep it in the balloon envelope when secured. Capt. Templar is sanguine that a 10,000 cubic feet balloon is quite capable of lifting an observer high enough for reconnoitring purposes, if filled with hydrogen, and well-nigh proved his case the other day when he overcame gravity, if he did not rise, with the aid of a light coal-gas with which this small balloon was filled. The coal-gas, specially manufactured for his balloon, had a lifting-power of 50 lb. per 1,000 feet, so that a total of 500 lb. was here at his disposal. As we have said, this was insufficient for an ascent, for, besides the weight of the aëronaut, there are, it must be remembered, envelope, car, tackle, cable, and ballast to be taken into consideration. Instead of 500 lb., hydrogen of the same volume would have supplied a lifting-power of 700 lb., and this of course would have been ample, and to spare, for an ascent.

To make this hydrogen recourse will be had, as in previous experiments undertaken by our military authorities, to the decomposition of water in the form of steam. The latter is to be passed through tubes filled with iron filings or turnings, and these, in becoming oxidised, set free the hydrogen. Unfortunately the hydrogen obtained in this way is impregnated with moisture, and unless submitted to the action of some desiccating agent like quicklime, for instance, is of little good for ballooning. The hydrogen it is proposed to obtain in the field, at any rate, in this fashion, and it remains to be proved whether Capt. Templar and his colleagues can secure it sufficiently pure and in proper quantity under these practical conditions. Although hydrogen is given off fast enough at the outset, previous experimenters have found the supply to fall off rapidly, for as soon as the surface of the particles becomes oxidised the decomposition of the steam ceases.

But perhaps the most interesting feature of the present ballooning experiment will be the trial of compressed gas. As our readers know very well, compressed gases are now a commercial article in this country, and you may purchase cylinders of oxygen or hydrogen at twenty atmospheres pressure. As our Royal Engineers carry about with them in the field such unwieldy things as pontoons, they can hardly grumble at a waggon load of hydrogen tubes, and with these it is suggested to fill a balloon just wherever a reconnaissance is to be made. On nearing the enemy the first convenient spot will be chosen for the manufacture of the hydrogen, and this will then be compressed, with the assistance of suitable apparatus, into the tubes, to be drawn off again when the ascent is to be made. In this way there is always to be gas at hand not only to fill the balloon but to keep up a constant supply for a limited period, since hydrogen, under the most favourable circumstances, rapidly exudes from a balloon envelope.

A military balloon, it appears to be decided, must be a captive one, and opportunity would of course be taken to place the observer in electrical communication with the earth through the medium of insulated wires twisted round the rope in the same way as in Mr. Woodbury's photo-aërial apparatus above described.

H. BADEN PRITCHARD

HYPNOTISM

THE phenomena of "hypnotism," "mesmerism," or "electro-biology," have of late years excited so much popular interest—not to say popular superstition—that their investigation by a competent man of science will appeal to the sympathies of a wider public than the purely scientific. My object, therefore, in writing the present article is to give a brief review of a monograph on this subject, which has just been published by the well-known physiologist, W. Preyer of Jena.[1]

In order to eliminate all possible effects of the imagination, Preyer performed his experiments only upon animals, and he begins his paper with an historical sketch of previous investigations of a similarly restricted nature. First we have the "Experimentum mirabile" of the Jesuit Athanasius Kircher, published by him in the year 1646.[2] This consists in taking a common fowl, binding its feet together, and placing it on a floor. As soon as it has ceased to struggle a straight line of chalk is drawn from the point of its bill along the floor. If the legs are now

[1] Die Kataplexie und der thierische Hypnotismus. (Gustave Fischer, Jena, 1878.)

[2] In a postscript Preyer states that he has found this experiment to have been published ten years earlier, by Daniel Schwenter, and the quotation which he makes from Schwenter's book goes to prove that Kircher probably derived his knowledge of the experiment from that source.

untied the fowl makes no endeavour to escape, but remains as it were transfixed, and refuses to move even when urged to do so. Preyer observes in passing that the chalk line constitutes no essential part of the conditions, inasmuch as a fowl may be equally well thrown into a state of hypnotism by simply holding the animal for a short time upon the ground so as forcibly to prevent struggling.

After Kircher no one seems to have investigated the phenomena of hypnotism, or, as Preyer calls it, kataplexy, till the years 1872-73, when some articles on the subject were published by Czermak. The most striking of his experiments were those which he conducted on invertebrated animals—crayfish, for instance, being made to lie on their backs motionless, or even to stand upright upon their heads. Czermak endeavoured to account for the facts which he described by supposing that in some way or other the act of fixing the eyes upon a certain object, or of gazing into space, caused the animals to become sleepy and stupefied.[1] So vague an explanation could scarcely in any case be entitled to rank as a physiological hypothesis, and Preyer showed, in 1873, that the act of gazing had nothing to do with inducing the state of kataplexy, inasmuch as animals fell into exactly the same state when their optic nerves were divided, or their eyes covered with a hood—provided that their bodies were at the same time held in some unnatural position. Preyer therefore propounded a theory of his own, which, as first published, was that the state of fear into which the animal is thrown by being held in some unusual attitude serves to inhibit the power of volition and so of spontaneity—the animal, therefore, when released remaining statue-like in the position in which it was placed. In order to sustain this theory Preyer pointed to other cases in which fear serves to inhibit spontaneity—as, for instance, the motionless horror which some animals exhibit in the presence of great danger, the fascination of birds by snakes, &c. The theory as thus stated was very justly criticised by Heubel, who, in 1876, published a paper detailing his own researches on the subject, and seeking to identify the state of hypnotism with that of ordinary sleep. The effect of this criticism was to make Preyer state his theory with greater clearness, and as we now have it (1878), it seems to be as follows. Any "sudden, strong, unexpected, and unusual stimulation of centripetal nerves" produces an emotion of fear, which in turn produces some inhibitory effect on the will, and eventually a state of stupor. It may, I think, still be questioned whether this theory is of very much value, for even granting that "deathly terror" is always present—which it certainly need not be when the subject of the experiment is a human being—we are not acquainted with any other facts which would lead us to connect the subsequent state of motionless stupor with the preceding state of active fear.[2] But, passing on to the facts, we soon find that an important exception must be taken to the above statement as to the conditions under which hypnotism occurs, for various experiments proved that "sudden, strong, unexpected, and unusual stimulation" of any of those "centripetal nerves" which minister to the *special senses*, so far from inducing a state of hypnotism, instantly aroused an animal which had been previously thrown into that state. So that, in point of fact, as we are afterwards told, we may more correctly state the conditions which produce kataplexy in animals, by substituting for the words "centripetal nerves" in the above-quoted proposition, the words "nerves of tactile sensation." But here I may observe that, so far as the experiments go, there is nothing to prove that special stimulation of even the cutaneous nerves is necessary (indeed thermal and chemical stimulation of the skin was specially tried and produced no results); and therefore, it seems to me, the possibility is not excluded that the special stimulus in question may really have reference only to the "muscular sense." At any rate, all these experiments go to prove that kataplexy can only be produced in animals, either by suspending them in the air, or by forcibly holding them in some unusual position. Most animals recover their normal state after a few minutes, but frogs when suspended in the air will continue kataplectic until they die. Horses become kataplectic while they are being swung from wharves to ships, as shown by the fact that they remain passive so long as they are suspended in the air, but again begin to struggle so soon as their feet touch the deck. Preyer has succeeded in rendering kataplectic various species of toads, newts, frogs, ducks, poultry, pea-fowl, partridge, sparrows, mice, guinea-pigs, rabbits, &c.; but has uniformly failed in the case of many other animals. On the whole he concludes that while among sundry species of reptiles,[1] batrachians, birds, rodents, and ruminants, the phenomena of kataplexy may be more or less easily produced, such is not the case with fish and the more intelligent mammals. Nevertheless in another part of his memoir he attributes to a state of partial kataplexy the period of motionless delay which is observable in children after they unexpectedly fall and before they begin to cry. He also states, on the authority of Dr. Genzmer, that a squalling child (not a young baby) may often be quieted by laying it upon its stomach, or by gently pressing its face with the hand—care being taken in neither case to interfere with the breathing.

Our author further maintains that the so-called "shamming-dead" of certain species of *Articulata* when in the presence of danger is probably to be attributed to kataplexy. But here, I think, it is difficult to agree with him. That the action in question is not a properly so-called *intelligent* one, no competent person at the present day is likely to dispute; but for my own part I cannot see any evidence to show that it is not of the nature of an instinctive action which has been developed in the way to which Preyer alludes. It being for the benefit of some animals that they should remain motionless, and thus be comparatively inconspicuous in the presence of danger, those individuals which endeavoured to escape would be destroyed, while those which ceased to move would survive. Natural selection would therefore soon fix the artifice of "shamming-dead" as an inherited instinct. To this view Preyer objects that, if we accept it, the *origin* of the instinct is difficult to explain; while on the supposition of the action not being instinctive, but purely kataplectic, there is no difficulty to surmount. But to this it may be answered that there is no more difficulty in explaining the origin of the instinct to remain passive in the presence of danger than there is in explaining the

[1] When we fix our eyes upon a certain object and then alter their adjustment for some more distant point, so that the eyes endeavour, as it were, to look through the object, there is no doubt that after a time a somewhat sleepy feeling may be produced. Some persons, I find, can perform this action more easily than others, and it does not seem to consist altogether in maladjustment. At least I have observed that when the action is performed by persons who can do it well the pupils dilate prodigiously, and this even when the eyes are fixed upon a bright light such as the naked flame of a moderator lamp. As the action is completely under the control of the will, one is thus able to observe the curious spectacle of the inhibition by the will of a reflex which under all other circumstances is beyond the control of the will—the pupils dilating or contracting instantly at word of command, and quite irrespective of the stimulus supplied by light.

[2] Indeed a very remarkable experiment which is detailed further on would seem to show that even in the case of animals the state of fear need have nothing to do with inaugurating the state of kataplexy. The experiment in question consisted in suddenly decapitating a fowl, and while the reflex convulsions were still in progress, holding the mutilated body firmly on its back. The convulsions forthwith ceased, and the headless animal became for a time kataplectic. Unless, therefore, we suppose that the spinal cord is capable of suffering fear, and that it is more alarmed by being held firmly down than by being severed from the brain, we must conclude that a state of fear is no essential antecedent to that of hypnotism.

[1] Preyer does not appear to have himself experimented on any species of reptile, but in another part of his monograph refers in this connection to a very old authority, viz., Moses, whose power of causing serpents to appear like rods he supposes to have been probably due to the sagacious Israelite having known something about the phenomena of kataplexy. But considering the number, variety, and general quality of the experiments which Moses is said to have performed, it would surely be desirable to repeat the one in question before accepting the result as a fact of modern physiology.

origin of any other instinct—that of running away from danger included. Moreover, one of the animals to which Preyer refers, viz., the *Armadillo vulgaris*, not only remains motionless when alarmed, but rolls itself up into a ball—an action which certainly cannot be explained on the hypothesis of kataplexy. The most, therefore, that can be said for this hypothesis is, that possibly in its first initiation the instinct may have been assisted by the occurrence of kataplexy.

The time during which the kataplectic stupor lasts varies in different species of animals, and also in different individuals of the same species. The maximum duration observed in the case of rabbits was twelve minutes; but fowls and guinea-pigs continue stupefied for a somewhat longer time. By watching carefully for the first indications of recovery, and by preventing the voluntary movements in which these indications consist, animals may be kept in a state of kataplexy for an indefinite time. Warm-blooded animals do not suffer from such prolonged experiments; but the latter are fatal to frogs. In mammals the most characteristic features of the kataplectic state, besides that of unconscious stupor, are violent tremblings of the extremities, blinking of the eyes, movements of the jaw and pupils, irregularity of the pulse and breathing, pallor of ears in rabbits, occurrence of defæcation and micturition. On recovery the abnormal state passes off suddenly, leaving the animal bright and brisk as before, and thus, as in so many other respects, the state of kataplexy differs from that of ordinary sleep.

One other point of interest must be noted. Preyer finds that it is impossible to produce the state of kataplexy in any animal that is "newly-born." In the case of guinea-pigs susceptibility to be thrown into this state only begins to show itself during the first week after birth, and then gradually increases through two or three weeks. This curious fact is explained by the hypothesis that the volitional centres—or the centres which are supposed to be affected by kataplexy—require some time after birth to be brought into functional relation with the lower centres.

On the whole, then, it will be seen the facts relating to the hypnotism of animals are much more definite than the theories by which it is sought to explain them; and although we may be prepared to agree with Preyer that these facts in some way depend on certain unusual stimuli acting in some peculiar manner on some inhibitory centre or centres, we must feel that this statement of the case brings us only to the threshold of an explanation. GEORGE J. ROMANES

HYDROGEOLOGICAL SURVEY OF ENGLAND

FLOODS, or water in excess above ground, form one of two extreme conditions, of which the other is drought, or water in defect below as well as above ground. The requirements of water-supply induce the necessity for storage. Out of these three simple facts arise several intricate public questions. Thus it is evident that, if floods are to be controlled, some one must have authority over the rivers, and inasmuch as floods are intensified by land drainage, that authority must extend over the whole of the watershed area if it is to execute measures of a sufficiently comprehensive character to be effective. As works cannot be constructed without money, it must also have rating powers over the whole river basin for the purpose of raising the necessary funds to cover the cost of such remedial works. But inasmuch as the flooded lands bear a small proportion to the contributary area, that is, to the rest of the watershed basin, the consequent preponderance of influence and capital is largely in favour of the unflooded portions. Therefore, if the case of floods rested for its remedy solely upon the loss sustained by riparian owners, it is doubtful whether the British public would ever be brought to see the desirability of moving in the matter. Drought, however, is felt by an increasing population, whose interest in having a proper water-supply is as deep as can be desired. The public looks to the engineer to provide proper storage, who is thus called upon to unravel at least two of the knots that surround the subject of rivers considered in relation to the storage of water. The first of these is of a purely physical kind, and is simply this: that whereas water for the purposes of water-supply is required at high levels, the pure rain which falls upon the declivities of the watershed area at once proceeds to find the lowest level or the deepest ruck in the valley, down which it courses, along the natural main drain of the basin, and below the level of all possible habitations, to the sea. Therefore, before it can be used, it must be lifted out of this ruck. Here steps in the second difficulty. Some one has a vested right in every yard of this water, and a real or supposed interest in obstructing every attempt to divert any portion of it. Waterworks having rivers for their sources have for these reasons proved too expensive for scattered populations in the past. Nevertheless, when fish was a necessary article of diet, the money and influence were forthcoming to cause the construction of a series of very noble ponds, and subsequently when the manufacture of iron flourished in the south of England, many more were added for the purposes of water power; while in some cases water was diverted from the main channel and carried in an open conduit, as in a mill race, with the same object in view. In the case of canals, much of the best and purest spring water the country contains has been degraded from its higher uses to the purpose of a common carrier, but now that the requirements of the population have changed, and it is no longer essential either for the one purpose or the other, but is wanted for drinking, it should be the aim of the engineer to do for water supply what has been done for water power, but on a more comprehensive and extended scale, viz., to keep the water as high as possible by diverting as much as he can take from the upper tributaries, and causing it to contour as far as possible along the ridges with a view of commanding the largest extent of country by gravitation, and to compensate the main channel by a series of storage ponds. Numerous instances may be found in the lower greensand districts in Surrey, formerly a seat of the iron trade.

As a whole, the country is more largely dependent upon subterranean sources, or upon wells, for its water supplies than it is upon rivers. Inasmuch as every well that is sunk increases by a small amount the storage capacity of the stratum, the tendency is in the direction of a gradual lowering of the water-line. The resources of the subterranean water systems cannot be taxed indefinitely. Under London an elliptical vortex has been pumped out whose dimensions below sea-level are twenty miles long, eight miles across, and 130 feet deep, the total amount of depression at the apex being about 150 feet. Yet we have very recent instances of destructive local floods in the Metropolitan area immediately above this great centre of exhaustion. These two considerations point to the multiplication of wells, coupled with a proper system of replenishment from flood waters, as a means of utilising these natural reservoirs. The restoration of the original levels under London would restore to upwards of one hundred square miles of country their lost property as Artesian areas of overflow, the value of which is such as to confer upon the surface its full value as building land.

Thus, as storage above ground is expensive, and generally in supposed conflict with the interests of rivers, few of the numerous natural sites for reservoirs in England have been utilised, except in some places in the southern counties, where they were dammed up for fish ponds and

water power; whereas storage below ground, excepting tanks, remains for the engineer of the future.

Since, then, the agricultural interest is an irresponsible flood producer, and makes no counter provision for the storage of the water prematurely taken out of the soil; and since existing Conservancy Boards have not the necessary powers to deal with floods; and since the claims of water supply are paramount, and, from being strongest in periods of drought, can only be met by provision from flood waters; and since again many of the subterranean water-systems are being steadily exhausted, it becomes evident that no existing authority has the powers necessary for the successful treatment of the various questions so interwoven.

Whatever shape or shapes this governing body may ultimately take, all authorities who have expressed their views upon these questions are agreed that a preliminary survey of the natural sources of supply is necessary. The collection of these essential premises to successful legislation and to successful engineering works lies within the special province of hydrogeology, which takes up the history of rain-water from the time that it touches the soil. The tangible product of the survey is a map, which shows at a glance the necessities and the capabilities of each river basin. By the execution of such a map and the mere exhibition of the facts, a great stimulus is given to engineering enterprise, and by the establishment of such a survey, as a forerunner to legislation encouraging the construction of all necessary works, and the consequent removal of the feeling of want of scope that has stood in the way of the engineer hitherto, Government will have earned the thanks of the engineering profession and of the nation at large. JOSEPH LUCAS

THE INTRA-MERCURIAL PLANET

WE publish the following three communications in reference to the observations and calculations of Prof. Watson on the intra-Mercurial planet, about the existence of which there now seems little doubt. It will be seen from the third communication that Prof. Watson has been led to slightly alter the place of the planet from that given in the foot-note to Mr. Lockyer's article last week.

Prof. Watson, it will be remarked, refers to a second object, which he considers probably new. The position of the nearest conspicuous star ζ Cancri, at the time of his observation, was in R.A. 8h. 5m. 14s.·4, and declination 18° 0′·9.

The following letter to Mr. Lockyer we referred to in the foot-note (p. 462) last week:—

"*University of Michigan, Observatory, Ann Arbor,*
"*August* 14, 1878

"Since my return I have placed the paper circles on a graduated circle, and have read off the marks made during the observations at Separation. The resulting place of Vulcan differs slightly from that which I inferred from mere estimation at the time of the observations.

"The place which I have now derived I consider to be trustworthy within 5′ of arc. It is as follows:—

Washington Mean Time.		R.A.	Dec.
1878, July 29 ...	5h. 16m. ...	8h. 26m. 54s.	+ 18° 16′.

"You are already familiar with the method which I adopted. If I were to do the work over again I would use the same method. It does not give the place so accurately as it would have been given by graduated circles and verniers, but it does away entirely with the uncertainty which might be attributed to an erroneous circle reading at the time. My circles are like the chronographic record of a star transit. They give the pointings for the planet and the sun, and the readings can now be made at will.

"You will be pleased to hear that the planet was seen a few minutes afterwards by Mr. Lewis Swift, who observed in the neighbourhood of Denver. Mr. Swift is known to astronomers by his discovery of comets. I do not know whether he obtained anything more than an estimate of the position; but the place in which it is reported that he saw the planet agrees with my observation. This corroboration is peculiarly fortunate, considering the negative results of other observers.

"JAMES C. WATSON."

The following has been forwarded to us for publication by the Astronomer-Royal:—

"*Keswick, September* 2, 1878

"I have received from Prof. James C. Watson the following communication in reference to the suspected intra-Mercurial planet:—

"'*University of Michigan, Observatory, Ann Arbor*
"'*August* 14, 1878

"'During the recent total eclipse of the sun, I devoted myself to a search for an intra-Mercurial planet. In order to expedite the record of position, I placed disks of cardboard on the circles of the equatoreal, and marked the pointings by means of a sharp pencil and a pointer. All danger of error from wrong circle-readings is in this way avoided.

"'In the course of the search, I came across a ruddy star of the 4½ magnitude, which had a perceptible disk, the magnifying power being only 45, and which was in a position where there is no known star. It was very much brighter than θ Cancri, which was seen a little further to the west. Its position was referred, by means of the circles, to the sun, and was as follows:—

Washington Mean Time.	Apparent α.	Apparent δ.
1878, July 29 5h. 16m.	8h. 26m. 54s.	+ 18° 16′

"'There was no appearance of elongation such as might be expected if it were a comet, and hence I feel warranted in believing it to be an intra-Mercurial planet. The details of the observations I will send you hereafter.'

"Prof. Watson's statement appears to render it very highly probable that the object seen is really an intra-Mercurial planet. I remark, however, that the reason for excluding the supposition of its possible cometary character does not seem quite conclusive, as, when the tail of a comet and the small appendages of its head are invisible, the nucleus is usually circular.

"G. B. AIRY"

The following letter to Mr. Lockyer, just received, contains Prof. Watson's latest statement on the subject:—

"*University of Michigan, Observatory, Ann Arbor,*
"*August* 22, 1878

"On account of a wrong value of the correction to be applied to Prof. Newcomb's chronometer, the place of the new star which I communicated to you last week was erroneous. Please substitute, in place of the numbers then given, the following:—

Planet − ☉
$\Delta\alpha$ $\Delta\delta$
− 8m. 21s. − 0° 22′

Washington Mean Time.	Planet's Apparent. α	δ
1878, July 29 5h. 16m. 37s.	8h. 27m. 35s.	+ 18° 16′.

"The more I consider the case the more improbable it seems to me that the second star which I observed and thought might be ζ Cancri, was that known star. I was not certain in this case whether the wind had disturbed the telescope or not. As it had not done so in the case of any other of six pointings which I recorded, it seems

almost certain that the second was also a new star. The position comes out

$\alpha = \odot - 27\text{m. } 18\text{s.} \qquad \delta = \odot - 35'$

And

Washington Mean Time.		Apparent. α	δ
1878, July 29	5h. 17m. 46s.	8h. 8m. 38s.	+ 18° 3'.

"JAMES C. WATSON"

Our Paris Correspondent writes that Admiral Mouchez has received a letter from Prof. Watson. M. Gayot has completed his calculations and finds that Prof. Watson's observations are in accordance with Dr. Lescarbault's discovery, so long denied by M. Leverrier's opponents.

GEOGRAPHICAL NOTES

THE Geographical Society of Paris took possession, on the 2nd instant, of their new hotel in the Boulevard St. Germain, No. 134. The ceremony took place at three o'clock, under the presidency of Admiral La Roncière Le Nourry, who delivered an address explaining that it was not an international congress, but merely a national meeting of the several French societies, to congratulate their eldest sister on the success which had crowned its efforts. M. Bardoux, the Minister for Public Instruction, who was seated at the right hand of the president, handed the papers of Officer of the University to the architect of the Society. He read a letter from M. de Ferri, the French consul at Zanzibar, intimating that excellent news had arrived from Abbé Debaize, the French explorer, now proceeding towards Tanganyika. The delegates of various French geographical societies afterwards gave addresses summarising the progress which has been made in the work which each is carrying on.

A MAP of France, for which a vote on account of 100,000 francs has been given by the Chambers, is being prepared by the parochial authorities on the scale of 1 to 100,000. It will be hydrographic, not orographic, levels being shown only by curves. Some of it will be issued by the beginning of next year, and two departments figure as specimens in the Exhibition. The road surveyors are to make any alterations from time to time so as to prevent its becoming obsolete. Names, railways, roads, and boundaries will be coloured black; water, blue; woods, green; and footpaths, red.

AN important and interesting discovery has just been made by Dr. A. Kirchhoff in the Library of the Halle University. It consists of a copy of a part of the original diary kept by Capt. Cook during his journey in the year 1772, beginning on July 13, 1772, and ending January 11, 1773. The volume was originally presented to the University by one of Cook's travelling companions, John Reinhold Forster, who died at Halle. Dr. Kirchhoff has communicated the contents of the volume to the Geographical Society of Halle, and proposes to compare the contents with the original diary, should the latter still exist.

M. MAYEFF, who was sent by the Russian Government for the exploration of the routes which lead through the land of Hissar and Amu-daria, has now returned to Tashkend. He has explored and surveyed the routes, 78 miles long, from Djam, a town south-west of Samarkand, to the great town Guzary; and two other routes from Guzary to the passage of the Amu river at Keliff,—one of them 98 miles long, and the other, through Shir-abad, 152 miles. The latter is the best, as there is plenty of fresh water and wood along the whole of the route, as well as two large settlements, Derbent and Ser-ob. At Keliff the Amu-daria is 1,170 feet wide, and steamers can go up the river as far as this place. There remains only 27 miles from Keliff to the Afghanistan town Akhcha, or Andho, and no more than 80 miles of a very good route from Akhcha to Sarypul.

M. PRJVALSKY, who returned some time since from his Central Asian travels, is now preparing for a second journey to Thibet, which was postponed because of the bad state of health of the indefatigable traveller, as well as because of the insecure diplomatic relations between Russia and China.

WE learn that the St. Petersburg Geographical Society and the Society of Naturalists are preparing a scheme for the scientific exploration of the little-known parts of the Caucasus.

THE principal paper in Guido Cora's *Cosmos*, Nos. xi. and xii., is a detailed account by Eugenio Parent of his voyage to Spitzbergen in 1872-73, in the Swedish vessel the *Polhem*.

AN expedition has been organised by the proprietors of the *Queenslander* newspaper for the purpose of making a flying survey of the territory beween Blackwall (Queensland) and Port Darwin, North Australia, a distance of 1,400 miles, with the view of determining the character of the country and the practicability of constructing a trans-continental railway. It was expected that the party would be fully equipped and start from Blackwall on July 12.

IT may be of interest at present to know that *Globus* is publishing the itinerary of Dr. P. Schröder's second journey in Cyprus in the spring of 1873.

AT Duisburg on Tuesday there was unveiled a memorial of Gerhardt Kremer, commonly known as "Mercator," and the author of "Mercator's Projection." Born of German parents in Flanders in 1512, he settled at Duisburg in 1552, and died there in 1594. The first stone of the monument was laid in 1869, but lack of funds delayed its completion.

BREHM'S THIERLEBEN[1]

THESE three fine volumes are in continuation of those reviewed in NATURE (vol. xvii. p. 43), and for the most part they maintain the popular and scientific character of this really great popular work. A. E. Brehm contributes all that was left of the mammalia, and gives a great volume on the reptilia and amphibia. The invertebrata have been wisely placed in the hands of Oscar Schmidt, of Strassburg, the insecta having been already completed by Teschenberg. A. E. Brehm's two volumes comprise nearly 1,400 pages, and they are about the average size of those which have appeared, but the invertebrata (without the insecta) are crammed into less than 600 pages. This is the only great fault we have to find, and it appears to be chronic in every country and under every editorship. The vertebrata take up so much space that the invertebrata must be "scamped;" and the "scamping" is the result not of the editors or authors, but of the publishers. Formerly this unfortunate elaboration of the idea of "first come first served," was limited to human history, and there is a well-known "History of England" which deals largely with the remote past, and which coming to the not unimportant reign of George III. at the close of the book, summarises it with the ejaculation, "whom God preserve!" We might, in a better spirit, say God bless some one who will do justice to the vast invertebrate sub-kingdom in a popular manner.

Oscar Schmidt has had a task of great difficulty to perform in giving anything like a general view of invertebrate life; and when the enormous advance of knowledge,

[1] Die Säugethiere, vol. iii., 1877; Die Kriechthiere und Lurche, vol. i., 1878, von A. E. Brehm; and Die niederen Thiere, von Oscar Schmidt, 1878. (Leipzig: Verlag des bibliographischen Instituts.)

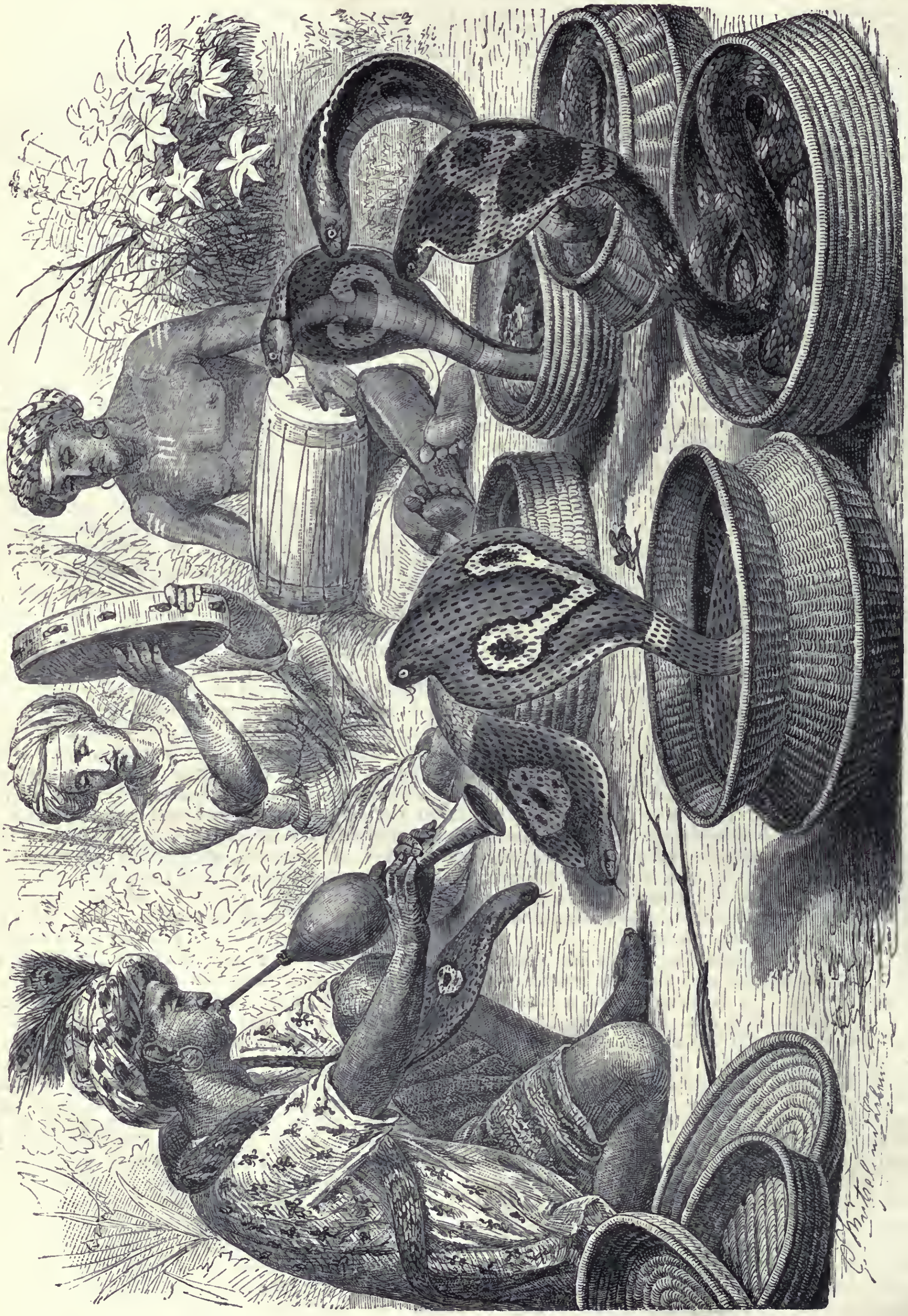
in many orders of it, during the last few years is considered, his contribution requires very equitable criticism. By choosing types of the great groups, by a free use of the observations and sketches made at the Naples and other aquaria, and by some happy selections of the results of the deep-sea dredgings of late years, a very presentable book has been put together. Nevertheless, the book will not satisfy the English reader who is likely to study it. Too good for the great mass of readers, it is so very deficient in the descriptive morphology and teleology of the lowest groups, especially, that the great want of the moderately-educated naturalist is not satisfied. The author's title-page dates 1878, but much of the best work of the world during the previous two years is not introduced. With regard to the authors who are quoted and utilised, there is a curious absence of some of the best English works, and the names of some of the most distinguished naturalists in the world are conspicuous by their absence. We protest very meekly, however, for it is good to be humble as the Germans were ten years ago, and when they did their best work and could think that their fellow-labourers who happened to differ from them were not absolute fools. Another peculiarity of this volume, is the highly diagrammatic nature of some of the views from the life; but it is compensated for by the elegance and artistic grouping of many of the objects in the larger plates, and by the introduction of many novelties. The book commences with the crustacea, the worms follow, the brachiopods and rotifera being in the midst, and the bryozoa concluding the group, and then come the *Weichthiere*, including the cephalopoda, pulmonata, prosobranchia, heteropoda, opistobranchia, and pteropoda. The bivalves follow, and then the ascidia. The great group of the echinoidea is despatched in twenty-four pages. The cœlenterata, including, according to the last morphological craze, the sponges, occupy less than one hundred, and the vast group of the protozoa less than thirty pages. Of this classification the less that is said the better; it is the age of novelties; but there are still some who do not yet fall down and worship the dross metal image Haeckel and others have set up. An interesting figure of the spider crab in its aquarium home gives the peculiar forward droop of the great claws and the daddy-long-legs appearance of the other members: and in another, one of the very opposite dromia group is bedecked with a sponge. A fine plate of pagurids shows one about to change its domicile and another with its usual sea anemone on its protecting shell. There are some interesting remarks on the parasitic amphipods, and the structure and the relations of *Phronima sedenteria* to doliolum and pyrosoma are noticed.

The smaller crustacea are illustrated by the life history and anatomy of Acanthocercus, and there is the queerest transparent *Leptodora hyalina* delineated; and it is to be hoped that it is more truthful than the Paradoxides on the next page but one, which has no facial suture, and whose cephalic shield is out of drawing. The chapter on the cirripedia is poor; and it would have been all the more complete if an abstract of the interesting paper on *Lepas fascicularis*, by poor von Willemoes-Suhm, had been given from the *Phil. Trans.* The successive nauplius stages, the cypris stage, and the absence of the Zoëa, were so splendidly worked out, that any modern natural history should contain them. On the other hand, the huge group of worms is very fully and ably dealt with. Nevertheless, it is to be hoped that some zoologist who may read Schmidt's *résumé* will speedily break it up into reasonable divisions, or rather separate and altogether reorganise the unwieldy, incongruous class. Amongst the rotifera *Notomata mysmeleo* is chosen as a type, and is carefully described and ably drawn from nature by Simroth, to whom the author is frequently under great obligation for exact and artistic illustrations. The exquisite Floscularia, however, is not satisfactorily given, and indeed to do so is hardly possible on wood. The five circum-oral prominences, armed each with a bundle of long protoplasmic filaments, which elongate and radiate, becoming like stiff hairs, and which diminish, become flaccid, and are retired as the creature contracts, are so well known to all observers of pond life, that good indeed must be the draughtsman who can faithfully convey the true impression. The parasitic worms are abundantly dealt with; but we miss the late researches on the land planarians. The general weakness of the chapter on the bryozoa is compensated for by the description and life-history of the extraordinary sponge-dweller Loxosoma, with its schwärm larvæ, and their metamorphosis.

Octopus vulgaris and *Eledone moschata* vie amongst the dibranchiata in ugliness in the illustrations relating to the cephalopoda, but there is nothing very new in the context. Amongst the shell-fish the most interesting forms noticed, are the heteropoda; and the gradation from the shelled Atlanta, whose description and delineation is very good, through carinaria and pterotrachæa to the naked phyllirhoe, resplendent with luminous spots by night, is well done. The pterotracheæ, so translucent and long and shell-less, with their tufted gills and absent tentacles, have evidently been carefully studied at the Naples aquarium, and all about them will be read with much interest. Doris and its neighbour *Ancula cristata*, and the extraordinary *Dendronotus arborescens*, are admirably given. There is much that will be new to the ordinary English naturalist, in the chapters on pteropoda, and the extraordinary larva of pneumodermon will excite as much attention as that of dentalium in the next chapter. Panceri's discoveries of the nature of the luminous organs of pholas are given, and the peculiar phosphorescent secretion is noticed. The chief merit of the chapter on the Ascidia is in the illustrations; but pyrosoma, with its wonderful luminous points, is well described, Panceri again being quoted from. The echinoderms are briefly treated, and the only two points worthy of notice are illustrations, one of urchins in the Neapolitan Aquarium, and the other of a comatula crawling over a sabella, with alternate legs, as is their wont. But there is a curious story in the context, relating to the fipssiarity of *Ophiactis virescens*, a six-armed Ophiurid. From this point the book is too short, and the important groups still unnoticed, are passed over too quickly. Beautiful engravings of hydrozoa, corals, and spongida abound, but the same cannot be said of those of the infusoria and amœba. Acineta is unlike nature, amœba does not show the peculiar head, and *Gromia oviformis* has its pseudopodia too moniliform; the last news about globigerina is not given. Finally, the drawing of the solitary radiolarian is wretched, and pretty noctiluca, with its vacuolated protoplasm, would have been all the better done if Allman had been studied and quoted. There are, however, very few shortcomings in this most interesting volume.

Dr. A. E. Brehm's volume on the reptiles and amphibia is magnificent, and combines good zoology with sufficient morphology and physiology, so as to make it a very useful book. Gustav Mützel, E. Schmidt, and Robert Kretschmer as artists have produced some wonderful plates, and the pages may be opened haphazard and good illustrations are sure to be seen. There is nothing harder to draw truthfully and artistically than a snake, and Mützel's *Morelia argus* (Python punctatus) on page 337 is the best realisation of a huge serpent on a tree-bough overhanging the water, and about to make its rush. When such ophidia partly cling, there is a remarkable flattening and angularity of the body at the spot; elsewhere the trunk may be as cylindrical as usual. This, so well known to the ancient sculptors, has generally been forgotten by modern draughtsmen, who generally draw a snake like a rope. In the instance before us, the truth is

carefully told with an able pencil, and the position of the head in relation to the first few feet of the body is admirably given. Without reflecting on British woodcutters, for they are for the most part beneath artistic contempt, the German engraving is often exquisite. A weird scene, where over water-lilies a moss-grown branch, half hidden in spiders webs and great orchid blooms, supports an angry *Draco rolans*, is capital. A butterfly is settling



COBRA CHARMING.

down within the creature's reach, but a fellow reptile has made its spring, and with expanded rib parachutes, is about to seize the insect's wing. The mimeticism of plant and animal is so fully developed by the artist, that really one must look hard at the picture before its faithfully rendered details strike the eye. A group of chameleons on a mimosa, one putting out its long club-ended tongue and the others glaring with their bulged

eyes and craftily creeping, is a capital picture to show a troublesome child, for it has the effect of a thorough scare. *Phrynosoma orbiculare*, with its sharp neck and back spines, the hideous moloch all bristling, ptychozoon with its fringes and odd-looking digits, are fairly comparable, in ugliness, with the chelonians, *Chelys fimbriata*, platysternon the big-headed, and the mischievous-looking snake-like tortoises—the hydromedusæ, and the artistic delineations and the descriptions are equal in merit.

The volume commences with a general introduction and then deals with the tortoises. It is interesting to find our White of Selborne quoted, in illustration of the habits of the common pet, and to notice that due credit is given to Darwin and Günther for their notices and elaborate

HORNED FROG.

descriptions of the gigantic land-tortoises. The flexible group are introduced by a description of cinixys, and then the terrapenes are described. Clemmys leads on to cinosternum, whose handy-looking beak, and active (for a chelonian) limbs, although small, give a very decided look to the animal. The turtles are noticed, and the curious dermatochelys completes the group. The crocodilia come next, and as it is the least popularly known, *C. acutus* strikes the reader at once. Its marine proclivities in and about San Domingo have so frequently been placed before the learned as indicative that the geological crocodilia were not necessarily fluviatile ani-

mals, that its habits, recorded by Humboldt, are very interesting. The sauria are commenced by a notice of hatteria, and it is to be hoped that by the next edition some one will have learned something of the habits of this extraordinary lacertilian with its dentate crest and back, palatine teeth, biconcave vertebræ, uncinate processes to the ribs and peculiar system of abdominal ribs—the living link with the oldest saurians. The Varanidæ and lizards are profusely illustrated, and Günther's work on pseudopus, more serpent-like, than Sincus in appearance, is noticed before this last genus. Trachysaurus, seps, and the Australian pygopus, might seem reasonably, from outside appearance, to lead to or to be parallel with Chirotes, with its small anterior extremities, and amphisbæna, and to merge into the ophidia. But Brehm has placed between the groups the highly interesting histiurus, chlamydosaurus with its ruff, uromastix, moloch the strange, basiliscus so heraldic in its contour, and the sea-lizard amblyrhynchus. There is a most comical drawing of two platydactyli meeting on a wall, one of them, the intruding party, clinging on to the steep face of a stone by its extended digits. These Gecko species conclude the part.

The ophidia have a very long and interesting introduction, which is more valuable in a natural history sense than in any other; the habits and archæology are capitally given, and some of the popular errors about snakes are exploded, commencing with the non-poisonous set, boa-constrictor and anaconda are given, the latter being rendered especially interesting by quotations from Bates, Humboldt, and Prince von Wied. Xiphosoma, a species of which, figured by Wolf in his usual style under Sclater's auspices, is familiar to us in England, has a spirited engraving by Mützel. It is disturbing the domestic felicity of a cock and hen, and chanticleer is grasped by the shoulder in the fangs of its enemy, whilst the hen is rushing off in dire alarm. The interesting long and slender snake from the Antilles is described with Brehm's usual care, and is drawn in its common position coiled up on a mass of sugar-cane leaves. Then passing on to the pythons, the author commences with a learned paragraph, in which quotations and opinions as to their size and gourmandising propensities are given from Megasthenes and Metrodus, whose vigorous imaginations were outdone by the boa seen by Regulus of one hundred and twenty feet in length. The snake which, in the time of Claudius, was found with a whole child in its stomach, comes nearer the truth. The Indian python (*P. molurus*) is shown strangling, in the folds of the first half of its body, a small ruminant, the hinder half being still on the ground to give a *point d'appui*. The hind-quarters of the prey are being grasped by the jaws preparatory to swallowing. A little further on *Python sebæ* is swallowing a bird: the gape is stretched to the utmost and the muscular tension of the neck is admirably given. There is a long and interesting history of the water snake (*Tropidonotus natrix*) with notices of its hybernation and habits, and the tree-snakes, well illustrated, lead to the water-loving and ugly Japanese achrocordus. The natural history of the poisonous snakes is carefully given, and the researches on the nature of the poison are carried down to the date of Brunton's and Fayrer's work. A large plate of the cobra charming is given. There is a wonderful drawing of *Hydrophis cyanocincta*, with its compressed body, and of the flat-headed, long-snouted sea-snake (*Pelamis bicolor*). There is much interesting information given about the vipers and the rattle-snakes, headed by a drawing of that curious happy family, the prairie-dog, owl, and snake, and the group ends with the genus Bothrops. In dealing with the amphibia, Günther is very generally followed, the hylidæ are elaborately illustrated, and *Nototrema marsupiatum* especially. The Antilles frog (*Hylodes martinicensis*), whose metamorphoses are shortened in the egg in which they have a very short tail, which is lost during the first day of liberty, is described, and then, passing on to the Ranidæ, the esculent form is noticed and figured. A pre-Raphaelite horned frog from the south of the Brazils, the obstetric alytes and the pipæ are admirably drawn and described. There is nothing very striking amongst the urodela, and the gymnophiona are curtly treated. There is no doubt that this volume will be studied by all who can read easy German, and we commend it to schoolmasters who usually teach the language out of the dreariest books.

The third volume of the mammalia, by Dr. A. E. Brehm, includes the horses, the ruminants, proboscidea; tapirs, rhinoscerides, hyracoidea, pigs, hippopotamidæ, and the marine series, and they are contained in nearly 700 large pages. The natural history of these groups is so profusely embellished with illustrations, and so replete with anecdotes relating to the habits, instincts, and methods of capture of the principal types, that it is a true popular encyclopædia of this part of nature. There might be more anatomy and physiology, but, as a book for common reading in all parts of the world, it is most excellent, and well up to the zoology of the day. It is refreshing to be reminded of the old school of zoologists, now almost extinct in this country, whose members, if they did not know all the anatomical minutiæ, the homologies, morphological affinities, and the terribly hard names of, and devoted to, the animal kingdom by modern *savans*, did know the outside appearance, and could tell interesting tales about the beasts of the field. It is true that they had not reached that standpoint where it is necessary to get at the base of the skull of a ruminant before its genus can be told, or to examine the knee, carotid, or some other *post-mortem* matter of a bird before it can be classified. But they could distinguish between living things by their outsides, and had a good idea of the notion of a genus and species from external characters. The horse, of course, affords great scope for Brehm's literary powers, and the archæology and the divisions of the group are well and carefully done. As everybody is the best possible judge of a horse, those who read this part will surely criticise the illustrations. They will please those observers who know the motions of a horse from plates, but they will not pass muster with horse-loving people. There is a cut of *Equus hemionus*, and, evidently from the position of the most prominent of the group, it is about to lie down at full gallop. Next comes a large plate of an Arab of pure blood, a stallion without a single good point about it: its ears are close together, it has small nostrils, and a jowl like a cart-horse; straight shoulders, small, long, upper fore-legs, and a great narrowing of the hock below the knee; it has a long barrel, no end of space between the rib and pelvis, and the off hind-leg is that of a rocking-horse. An Arab is admiring this beast, and an erect Black is probably discounting its value to that of twenty-five pounds. The tail is the best part. On p. 29 a mare and foal are trotting, and the mare is pointing her descending fore-foot after the manner of screws; artists rarely will see that the sound horse tries to get the back of its foot down in the trot, and then never falls. A cut of a quagga shows that this African animal does not require to touch the ground in its motion—motion we say, for whether the beast is trotting, cantering, or larking, it is impossible to say, for the near legs are wide apart, one under the nose and the other under the tail, whilst the off legs are within a foot of each other, and all are in the air. Burchell's horse is, however, properly drawn.

The ruminants will be more interesting to those readers who have not had the advantage of visiting large zoological gardens or of reading the stories of the great African hunters; but there are several types which are uncommon, and which are of much interest. There is nothing particularly new in the other groups, and the works of Murie, Brown, and others, have been carefully abstracted in dealing with the pinnipedia.

P. M. D.

THE PIKERMI AND SIWALIK FAUNAS PLIOCENE, NOT MIOCENE

THE best zoologists are in the habit of calling the mammalian fauna of Pikermi, in Greece, *miocene*. As instances, I may quote Mr. Wallace in the "Geographical Distribution of Animals," p. 118, where it is expressly stated that the mammalian remains in question are "from the upper miocene deposits of Pikermi," and Prof. Flower, in his paper "On some Cranial and Dental Characters of the Existing Species of Rhinoceros," P.Z.S., 1876, in which he mentions, p. 457, "the miocene *R. pachygnathus*, Wagner, from Pikermi."

I had occasion, a few days since, to consult Gaudry's "Animaux Fossiles et Geologie de l'Attique," in connection with the Siwalik fauna—also persistently, but, I believe, wrongly classed as miocene by European geologists and naturalists—and to my surprise I found it shown by the clearest evidence, that this Pikermi fauna occurs in pliocene beds, the age being proved by the occurrence of marine shells at the base of the bone-beds, and by the circumstance that the strata containing both bones and miocene shells rest unconformably on others with miocene plants. The latter fact is less important than the former, the value of plant remains for the determination of geological age being a disputed point, but no geologist hesitates to accept the indications afforded by marine annuals in preference to all others, and the tertiary marine faunæ of the Mediterranean area are particularly well known.

M. Gaudry proposes a hypothesis, *op. c.*, p. 431, to account for the presence of so many miocene forms in pliocene beds. It must not be forgotten that several Pikermi species are identical with forms found in undoubted miocene strata in Central and Western Europe. The theory proposed is briefly that the Pikermi mammals were driven into the hills by the breaking up of the miocene land, and starved to death, when their bones were washed down into the bed with pliocene mollusca. As there must have been, I think, a long interval of time between the deposition of the disturbed miocene beds and the formation of the unconformable pliocene strata, I think the following suggestion is preferable, as it avoids the idea of any sudden change. There is abundant evidence that the refrigeration of the earth's surface, culminating in the glacial epoch, commenced in pliocene times, and this may have led to a southern migration of the mammalia, so that animals which, in the earlier epoch, inhabited Central Europe, at a later period still survived in Grece, although they had been replaced by pliocene forms further north. It has already been shown by various writers that there is a connection between certain miocene faunas in Central Europe and the living mammalia of Africa on the one hand, and of the Malay countries on the other, and I think it not improbable that the remains found in pliocene beds in Greece, and at the base of the Himalayas, owe their miocene affinities to the same facts of migration to which similar affinities may be attributed in the living forms.

The evidence of the pliocene age of the Siwalik is simple. In Sind strata containing miocene marine fossils pass up into beds with a mammalian fauna, including some of the older Siwalik forms, such as *Mastodon*, *Chalicotherium*, *Dorcatherium*, &c., together with *Dinotherium*, *Hyopotamus*, *Hyotherium*, *Anthracotherium*, &c., which have never been found in the true Siwaliks. These Sind beds are apparently equivalent to the lower Siwaliks, which are unfossiliferous in the typical area. In the middle and upper Siwaliks, instead of the old forms just named, *Elephas*, *Loxodon*, cervine and bovine ruminants in abundance, and other recent types are found. Now, as the Sind beds cannot be older than upper miocene, the typical Siwaliks must be pliocene. The mammal *Bos* (*Bubalus*) *palæindicus*, found in the upper Siwaliks, occurs also in the Nerbudda alluvium, where it is associated with palæolithic implements. It is not credible that a mammalian species could have lived from miocene to post tertiary times.

The question of the true age of these later tertiary mammalian fauna is of such vast importance in the attempts now being made by many naturalists to work out the line of descent of living animals that I trust I may be pardoned for calling attention to the preceding facts.

W. T. BLANFORD

OUR ASTRONOMICAL COLUMN

SCHMIDT'S CHARTE DER GEBIRGE DES MONDES.—In the Introduction to the *Erläuterungsband*, accompanying Prof. Schmidt's lunar charts, he has given an interesting account of the progress of his great work, of the difficulties he encountered, and the assistance afforded him in a variety of ways, until the honourable and flattering conclusion. It was in the autumn of 1839, at his native place, Eutin in Holstein, that his attention, at first given to botany and zoology, was directed to the moon, by the circumstance of a copy of Schröter's work having come into his hands through a sale by auction. The engravings of the numerous craters and mountain-shadows made so strong and lasting an impression upon his mind that he appears to have resolved to make the study of the surface of our satellite the principal aim of his life. At fourteen years of age the possession of a small draw-telescope, constructed by his father, enabled him to commence his observations of the lunar features, and his first sketch was one of the well-known streaks of Tycho, made with this instrument, which was supported against a street lamp-post! A stand being subsequently provided, he began to draw whole phases, in which Mayer's charts were found of service. Thus he observed in 1840, his school studies, as he tells us, suffering thereby not a little. As so often happens in similar cases, young Schmidt's peculiar bent attracted the attention of one who had the disposition and the means to aid him, and State-Councillor Hellwag, a highly-educated man, with advanced knowledge of astronomy, provided him with a very perfect telescope by Dollond, and with it he observed at Hellwag's house. In July, 1841, he saw the moon for the first time in a large telescope, Petersen, assistant to Schumacher at Altona, having shown him the crater Gassendi and Bullialdus. He then learned first, as he says, the richness of the lunar formations, the more that he became acquainted at this time with the large chart of Mädler. In 1842 Schmidt went to Hamburg, and obtained access to the observatory under Rümker's charge; here, through the good-will of the director, he made use of various telescopes in furtherance of his lunar work during the years 1842-45. In June, 1842, he was also assisted by Herr Bartels, of Hohenfelde, near Hamburg, who allowed him the use of his telescope, and it was at this time that he made the earliest drawings which proved available in the construction of his great chart. In 1845 he went to Bilk, near Dusseldorf, the site of Benzenberg's observatory, but his progress was slow here, the principal instruments originally in the building not being then serviceable. During his residence at Bonn in connection with the observatory presided over by Argelander, this continued to some extent, the regular work of the establishment claiming attention; nevertheless in the period 1845-1853 he obtained sketches which proved of value, with written descriptions of many of the features of the moon's surface. On several occasions during this interval, by the encouragement of Profs. Galle and Bruhns, he had opportunities of making drawings with the aid of the Berlin 9-inch refractor. From 1853 to 1858, Schmidt had charge of the observatory of Herr v. Unkrechtsberg, an ecclesiastic at Olmütz, and here he undertook micrometrical measures for determining the heights of the lunar moun-

tains, &c. In March, 1855, visiting Rome, he made sketches with the large refractor of the Observatory of the Collegio Romano, and in the following month delineated many of the lunar landscapes with the telescopes at the Observatory of Naples. In December, 1858, he entered upon his present position as director of the Observatory of Athens, but from the state of the institution and the instruments, observations were not practicable for nearly a twelvemonth, so that it was only in November, 1859, that he was able to use, for his work on the moon, the 6-feet Plössl refractor, with which so much of the remainder of his long-continued labours has been completed.

In January, 1865, he formed the project of preparing a lunar chart, but his experience upon it for a considerable time was not satisfactory, for reasons which he details. In April, 1867, his final resolve was taken, and setting aside the results of his previous attempts at the construction of maps, he chose for his scale a 6-feet diameter, and, after the example of Lohrmann, arranged to subdivide his chart into twenty-five sections, as it has appeared. The work proceeded steadily until, in 1873 and 1874, those parts of it not strictly topographical and the colouring were completed.

In December, 1874, Prof. Schmidt proceeded to Berlin with his chart, for the purpose of exhibiting it to the authorities, astronomical and otherwise, in that capital. The interest which it excited here led to a fortunate combination of circumstances, through which the editing of the work was secured under the protection of the State, and the Crown Prince was instrumental in procuring the publication of the chart. At the suggestion of the Prince, the twenty-five sections were photographed in the department of the General Staff, and in April, 1875, Prof. Schmidt received from Field-Marshal Count von Moltke proofs of the same, which enabled him to bring the descriptive portion of his work to a close, while the original charts were retained at Berlin.

Such is a brief outline of the history of an astronomical undertaking that has occupied a lifetime. We must refer the reader to the explanatory volume for further interesting particulars of its progress and vicissitudes while under construction, at the many stations where its talented and indefatigable author has been located—from Altona to Naples.

NOTES

MORMONISM, whatever we may think of it as an institution, is so far in harmony with the times as to appreciate the value of scientific investigation; and its leaders seem to be heartily desirous to do what they can to further its interests. A recent number of the *Deseret News*, the leading organ of the Church party in Salt Lake City and Utah Territory, contains an account of the reception by President John Taylor, the successor to Brigham Young, of Dr. Thorpe, of the Yorkshire College, Leeds. Dr. Thorpe, accompanied by Dr. Schuster and Mr. Haskins, of St. John's College, Cambridge, is making a series of magnetic observations across the American Continent along the line of the Union Pacific Railroad, and Salt Lake City was selected as one of the stations. The president evinced considerable interest in the work, and showed a great desire to be informed respecting its character and objects. He offered to assign any spot within the city which might be deemed most suitable for the observations to the party, placed the transit instrument in the Tabernacle yard at their service, and suggested the laying of a special wire from his private telegraph office to the observatory for the purpose of exchanging time-signals with Prof. Safford, who is determining longitudes at Lieut. Wheeler's base station at Ogden. With such facilities the magnetic elements of Salt Lake City ought to be accurately known.

PROF. STRUVE, director of the Pulkova Observatory, is leaving Russia for a visit to Western Europe, for the purpose of giving directions for the construction of a new great refractor for the Pulkova Observatory. The old refractor, which some time ago was one of the best in the world, is now behind those of Washington, Chicago, and Gateshead (Mr. Newall's Observatory), and the Pulkova Observatory, according to the will of its founder, the Emperor Nicholas, should be maintained superior to all other observatories in that direction.

WE regret to announce the death, on August 15, at Kiel, of Prof. Emil von Asten, of the Pulkova Observatory, at the early age of thirty-six years. A pupil of Argelander, he is known by several remarkable researches,—as the investigation into the orbits of Uranus, and especially by his researches into the motion of Encke's comets with regard to the existence of a resisting medium. After ten years' study of the subject, he published a series of "Memoirs," in which he proved the existence of a medium, and showed why its influence could not be observed on other comets, as, for instance, that of Faye. His "Memoirs" have given rise, as is known, to one of those remarkable suggestions of Prof. Mendéleeff, as to the physical properties of this medium. Prof. Asten was, besides, one of the most valuable calculators of our time, and many of the Pulkova observations were reduced and calculated by him, or under his direction. His work on the sum of temperature, necessary for the development of several plants of the flora of St. Petersburg, was done during his leisure hours, as well as his discussions on the philosophy of Schopenhauer and Hartmann, of whom Prof. Asten was an admirer.

M. FIZEAU, the present president of the Paris Academy of Sciences, has been appointed to the Bureau des Longitudes, to fill the place vacated by the death of M. Leverrier.

WE have received the programme of a singular propaganda which seems to have been suggested by the recent opening up of the East to Western influences, and to which we cannot but wish all success. The circular is issued by M. C. Constant, of Smyrna, a member of the Asiatic Society of Paris, and his scheme is to form a society for the publication in Armenian of cheap popular scientific works, as one of the best means of enlightening and developing the people of the East, and of forming a bond of sympathy and union between those of all creeds. It is wished to implant in the East the great principles of the Experimental School, which form the greatness of the West. Special attention will be given to anthropology and the social sciences, which constitute the great problems of the age. The Eastern peoples are at a turning-point in their career, M. Constant says, and they ought to be prepared for the only definitive conquest to which they will submit sooner or later, "the conquest of science, that first European power." The proposed "Eastern Scientific Library" will be begun by the publication of an introductory volume treating of the positive method and its history; it will be a summary exposition of the intellectual evolution of humanity, from pre-historic times to the present. M. Constant makes appeal to the scientific societies of Europe for aid in this enterprise, and communications should be addressed to him as above.

THE Iron and Steel Institute holds its summer meeting at Paris this year, on September 16, 17, and 18, at the rooms of the Société d'Encouragement, 44, rue de Rennes. Besides the Introductory Address by the President, Dr. C. W. Siemens, papers will be read by Prof. Jordan, Paris, On the Mineral Resources of France; by Prof. Richard Akerman, Stockholm, On Some Studies Relative to the Present State of the Iron and Steel Manufacture, made at the Paris Exhibition; by Mons Euverte, Terre Noire, On Homogeneous Steel; by Danie Adamson, Manchester, On the Mechanical and other Properties of Iron and Mild Steel; by Mons. Marché, Paris, on Certain

Aspects of the Steel Manufacture; by J. S. Périssé, Paris, On the Ponsard Furnace for the Manufacture of Steel; by Sydney G. Thomas and Percy C. Gilchrist, Blenavon, on the Elimination of Phosphorus from Pig Iron in the Bessemer Converter; by R. P. Rothwell, New York, On the Low and Strong Water Gas Processes. Visits have been arranged for to the works of Creuzot, of Terre Noire, and of Hayange, Lorraine.

THE scientific facilities of Zürich are to be increased by the erection of a magnificent new chemical laboratory for Prof. Victor Meyer. For this purpose the cantonal government has granted an ample site and the sum of 600,000 francs, which amount will be increased by appropriations from the city. Prof. Meyer, although but 30 years of age, is one of the most popular and well-known professors of chemistry of the present day, and his present laboratory is thronged with students of various nationalities.

WE intimated some time since that a wealthy Danish brewer had set aside the sum of a million Danish crowns for the support of a laboratory in which to carry on scientific research. The first report of work done in this laboratory has just been issued under the title of "Meddelelser fra Carlsberg Laboratoriet" (Copenhagen, Thieles Bogtrykkeri). It is in Danish, with an abstract appended in French. The founder of the fund, from the application of which so much of scientific interest and technical value may be expected, is Mr. J. C. Jacobsen, a Danish gentleman who owns a large brewery in the neighbourhood of Copenhagen, and who is well known and honoured in his own country for his patriotism and for his intense love of science and art. The Carlsberg endowment is only the last of a series of munificent gifts to his country, or rather to the whole civilised world. The fund itself, as we have said, consists of one million Danish crowns, or about 56,000*l.* English. This sum is vested in the hands of five persons, who are nominated by the Danish Royal Academy of Sciences. A portion of the annual revenue is to be expended in keeping up the splendid laboratories attached to the brewery, in which chemical and physiological researches are carried on with a view to establishing as complete a scientific basis as possible for the great industries of brewing and malting. The other portion will, after the death of the donor and his wife, be expended in the advancement of the various natural sciences, mathematics, philosophy, history, and philology. In the administration of this latter portion much latitude is very properly allowed to the trustees of the fund, who will be enabled to apply it in almost any way they think proper. It becomes, in fact, a veritable research fund for all branches of science. The statutes of the fund will well repay a careful examination. The endowment has now been in force for something like two years, and the report embodies the results obtained in the laboratory during that period. The papers are of different degrees of value and interest, but on the whole form a very respectable contribution to our knowledge of the subjects on which they treat. A correspondent writes that last year he had the opportunity of inspecting the magnificent laboratories of Carlsberg, and was much struck with the complete and luxurious manner in which they are fitted up, and with the good style of work that was being carried on in them. The Report contains papers on the following subjects:—"On the rotatory power which beer-wort exercises on polarised light, and on its variations during fermentation," "Estimation of extract," and "Estimation of alcohol in beer," by M. J. Kjeldahl; "Researches on some factors which affect the propagation of the low yeast of *Saccharomyces cerevisiæ*;" "On the influence which the introduction of atmospheric air into fermenting wort exercises on fermentation;" and "Researches on the influence of temperature in the production of carbonic acid on barley germinating in darkness," by M. R. Pedersen.

MR. F. A. OBER, who has been engaged for several years in prosecuting investigations into the ethnology and archæology of the West India Islands, under the direction of the Smithsonian Institution, reached Martinique in the beginning of July, where he proposes to continue his labours. He has already explored the islands of Dominica, St. Vincent, Santa Lucia, Antigua, and Tobago, and has sent home a very good collection of the birds of these islands, including quite a number recently described by George N. Lawrence as new to science. He has also supplied many archæological objects of considerable interest. A list of the birds of Dominica and St. Vincent, prepared by Mr. Lawrence, is published in the *Proceedings* of the National Museum at Washington, and the notices of the remaining islands will soon appear in the same journal. Mr. Ober expects to return to the United States in September or October next.

A NEW meteorological society is in way of formation at St. Petersburg; its special aim will be the extension of meteorological observations in Russia.

THE committee of the Liebig Monument Association at Munich have given their decision regarding the various models sent in for competition. The first prize (100*l.*) was awarded to the sculptor, Herr M. Wagmüller, of Munich, and the second (75*l.*) to Prof. Begas, of Berlin.

THE International Association against the pollution of rivers, the soil, and the air, will hold its second meeting at Cassel on September 9 and 10, *i.e.*, two days before the meeting of the Association of German Naturalists.

THE general meeting of the United German Societies of Archæology and History will take place at Marburg on September 15-19.

AMONG the subjects expected to be discussed at the Social Science Congress at Cheltenham, October 23-30, are the expediency of increasing the number of universities in England, and the desirability of establishing free primary schools throughout the country.

THE Exhibition of Sanitary Appliances and Articles of Domestic Use and Economy, held in connection with the Autumn Congress of the Sanitary Institute of Great Britain, will take place at Stafford, from the 2nd to the 19th of October next.

AT Chalons-sur-Saone a committee has been formed for the erection of a monument in memory of Joseph Nicéphore Niepce.

IN the Fourth Report of the Underground Water Committee of the British Association, read at Dublin, Mr. De Rance comments on the scientific and practical importance of the continuance of the experimental well boring just carried to a depth of 1,000 feet, through the pebble beds of the new red sandstone, for the Liverpool Corporation, by Messrs. Mather and Platt. The boring is 26 inches diameter, and if continued, would not only prove the water-bearing capabilities of the new red sandstone, but its thickness, the character of the underlying rock, the nature of the coal-measures beneath, and the possible existence of productive coal-seams, and other questions of not merely local, but national importance.

AMONG the scientific novelties of the German book trade during the past month we notice the following works:—"Praktische Anleitung zum Bestimmen der Käfer Deutschlands und der Schweiz," J. Hoffmann (Stuttgart); "Vier Jahre in Afrika," E. von Weber (Leipzig); "Die Messung des Feuchtigkeitsgehaltes der Luft," Dr. K. Koppe (Zürich); "Theorie der Bewegung des Wassers in Flüssen und Canälen," O. Meissner (Bamberg); "Leitfaden der Physik," R. H. Hofmeis-

ter (Zürich); "Lehrbuch der vergleichenden Anatomie," Prof. A. Nuhn (Heidelberg); "Handbuch der Erdkunde," G. A. von Klöden (Berlin); "Beiträge zur Kenntniss der Orchideen," H. G. Reichenbach, Bd. 3 (Leipzig); "Die Alpenpflanzen nach der Natur gemalt," J. Seboth (Prague); "Die Rose; Behandlung, Zucht, und Pflege," Dr. A. Oehlkers (Leipzig); "Flora im Garten und Hause," H. Jäger (Hanover); "Anatomisch-physiologischer Atlas der Botanik," Dr. A. Dodel-Port (Zürich); "Lehrbuch der Botanik," Dr. C. Baenitz (Berlin); "Die Vögel," D. Kompfe (Leipzig); "Die Raubvögel Deutschlands und des angrenzenden Mitteleuropas," O. von Riesenthal (Cassel).

AT the recent Congress of Hygiene in Paris the National Health Society of London had three representatives. The National Health Society is composed of those interested in sanitary work of both sexes, and deals exclusively with matters affecting the sanitary condition of all classes, leaving medical questions to the doctors, and endeavouring to accomplish "prevention" rather than "cure."

Two guides and two German tourists recently lost their lives in ascending Cevedole, a mountain of the Tyrolese Alps. One of the victims was Dr. Sachs, the preparator of Prof. Dubois Reymond.

WE would direct the attention of our provincial readers to a useful article in this month's *Science Gossip* on "How to Start a Natural History Society." The directions are thoroughly practical and sensible.

ONE of the most interesting handbooks in connection with the Paris Exhibition is that to the British Indian Section, by Dr. Birdwood, the second edition of which is before us. It contains a great deal of geographical, antiquarian, and historical information, the Introduction treating of the geographical and physical features of the Indo-Germanic shore, on the Settlement of the Old World by the Human Race, the Antiquity of the Indian Trade, Routes of Indian Commerce, and of the Master Handicrafts of India. An Appendix contains much useful statistical information, and the handbook ought to be of permanent value.

THE *Cologne Gazette* gives details of the earthquake of August 26, as observed in that city. The earthquake passed from east to west. About three minutes to 9 A.M., a shaking and rising and falling of the ground, after the fashion of waves, began to be felt, and increased more and more until buildings rocked to and fro in a formidable manner. Tables, chairs, beds, stoves, &c., set up a dancing movement, which became so strong and tremulous that figures and ornaments resting upon them were knocked against one another and thrown down. Several persons declare that they felt as if an electric current were passing through their legs, and as if the earth were withdrawn from under their feet, and a sudden giddiness came over them. In some places the pendulums of the clocks ceased to beat. Towards the end of the vibration, which lasted about seven or eight seconds, there was heard a dull sound like the roll of distant thunder. Several persons state that soon after the first series of shocks there was a second but much lighter one. There was another (a third) shock at 11.10 A.M., which was not equal to the first either in severity or duration. It may be remarked that no fall of the barometer was observed to follow the earthquake. An interesting observation was made in the neighbourhood of Muhlheim on the Rhine:—"A very distinct curl was seen to cross the river obliquely, from south-south-west towards north-north-east, quite undisturbed by the current, while at the same time a deep roar of the water was heard. Even still (the afternoon of August 27) there is an audible rattling of the panes of glass, and domestic utensils, if close together, knock against one another, while a dull rolling sound is heard." The earthquake wave was felt from Hanover and Utrecht to Mayence in the south. The motion was sensible at Brussels and Liége, as well as Bonn, Cologne, and Aix-la-Chapelle. Some papers state that a seismograph at Cologne has proved the shocks to have had a duration of three-quarters of a minute.

IN the night of August 21–22 a meteor was seen to fall on a house in the vicinity of Butzbach, a small town in Hesse. On the following morning a small polished dark stone was discovered on the spot, and will be sent to the Berlin Museum.

IN consequence of the annexation of Alsace and Lorraine, the piscicultural establishment of Hüningen, which had been founded by the French government in the year 1852, passed into the hands of the German government, which, since 1871, has bestowed the greatest attention on the establishment, and spared no cost to make it as efficient as possible. Recent statistical reports state that the establishment, while under German control, has sent away no less than 23,500,000 ova of various species of fish, such as trout, salmon, carp, roach, &c. Some two million young salmon have been placed in the Rhine, and a similar number of other useful fish into the rivers of Upper Alsatia. The result has been that fish are now plentiful in those waters, that the rents paid for fisheries have considerably risen, that salmon can now be bought at about 6*d*. per pound in that neighbourhood, and that the time seems to have returned when fish was in those districts a cheap food for the people.

A NOTABLE improvement in watches is reported from Chaux de Fonds, Switzerland. By a peculiar process the figures on the dial are rendered luminous, so that if exposed once during the day to the sunlight they remain phosphorescent and visible throughout the night. Preparations are being made for the production of these watches on a large scale.

THE International Association for obtaining a uniform decimal system of weights, measures, and coins is holding a Congress in Paris at the Trocadéro this week. Though an unofficial gathering, several governments have sent representatives to it. On Monday, M. Tresca gave a survey of the question all over the world, and pointed out that the only countries which have as yet made but little progress are England, the United States, and Russia. It is thought the Congress will be able to agree on the general adoption of the metric system for weights and measures, and perhaps on a 10f. gold piece, nine-tenths fine, as an international unit.

AT the Birmingham Natural History and Microscopical Society's meeting on Tuesday last, Mr. Bolton exhibited the polyzoon, *Cristatella mucedo*, and the grouped rotifer, *Lacinularia socialis;* Mr. Slatter exhibited the polyzoa, *Fredericella sultana* and *Paludicella ehrenbergi;* and Mr. Levick exhibited the infusoria, *Actinosphærium eichornii* and *Spirostomum ambiguum*.

AN interesting series of experiments was lately instituted by Herr Muntz, in order to determine whether the living cells of the more highly organised plants, when entirely cut off from oxygen, are equally able, with the cells of fungi, to produce alcoholic fermentation. For this purpose he experimented with a variety of plants, beet, maize, cabbage, chicory, portulacca, nettles, &c. From each kind three equally healthy plants were selected. One was left in the open air, and the other two were placed with the accompanying soil, under capacious bell-glasses, containing an atmosphere of nitrogen, the oxygen being removed by pyrogallic acid. After a lapse of from twelve to forty-eight hours, they were removed from the glasses. One was placed in the open air in order to be certain that the power of development was retained after the imprisonment, and the other was cut off above the ground, distilled with water, and tested for alcohol. In all cases the plants which had been in an atmosphere free from oxygen showed appreciable quantities of alcohol, amount-

ing often to a thousandth of the entire weight of the plant, while no traces could be detected in the plant which had remained in the air during the same time. In this connection we should mention a detailed account in the *Journal für Prakt. Chemie*, of experiments instituted by Prof. Gunning, of Amsterdam, to settle the question of the ability of bacteria to exist in media free from oxygen. They consisted in inclosing in glass tubes easily decomposable substances, such as raw flesh, green peas, &c., infecting with a drop of a mixture of decayed peas and white of egg, which contains nearly all varieties of bacteria, —closing the tubes by fusion after freeing entirely from oxygen, and allowing to stand for periods ranging from four months to two years. The results of all these experiments showed that by exclusion of oxygen the bacteria were completely destroyed, the putrefaction being entirely stopped, and not continuing afterward, on the admission of filtered air free from bacteria.

THE appearance of phylloxera at Sachsenhausen, near Frankfort-on-the-Main, is officially reported. The appearance of the Colorado beetle at Jaratschewo in the district of Schrimm in the Prussian province of Posen is also reported.

AT Oderberg, in Austrian Silesia, we learn from a report to the Imper. Geol. Instit., March 19, some pile-structures, or rather the floors associated with them, were found in digging for the foundations of gas-works. Two rows of parallel, horizontal oak stems, 60 to 90 centimetres thick, 3½ metres apart, were met with. They were covered with peat, and a quantity of hazel nuts and seed of cereals lay under the peat. At 3 metres deeper more hard wood was found.

PROF. A. M. MAYER asks us to make the following corrections in his article on "Floating Magnets" in NATURE for July 4 (vol. xviii.):—On p. 258, 2nd col., line 13 from bottom, delete the sentence beginning "This is the only instance," &c. On p. 258, 2nd col., line 11 from bottom, for "This nucleus of 20 cannot be formed without the circumposed magnets as in Fig. 20," read "This nucleus can be formed without the circumposed magnets." On p. 259, 2nd col., line 11 from top, for "1*a*" read "8*a*."

THE additions to the Zoological Society's Gardens during the past week include a White-lipped Peccary (*Dicotyles labiatus*) from South America, a Golden Agouti (*Dasyprocta aguti*) from Guiana, presented by Mr. G. H. Hawtayne, C.M.Z.S.; a Bonnet Monkey (*Macacus radiatus*) from India, presented by Capt. Clarke; a Cape Bucephalus (*Bucephalus capensis*) from South Africa, eleven Spinose Lizards (*Agama colonorum*) from North-West Africa, received in exchange; a Common Marmoset (*Hapale jacchus*) from South-East Brazil, a Grey Parrot (*Psittacus erithecus*) from West Africa, deposited; a Red Deer (*Cervus elaphus*) born in the Gardens.

THE BRITISH ASSOCIATION

REPORTS.

Report of the Committee on Mathematical Tables.—Mr. James Glaisher has undertaken the calculation of the factor tables for the fourth, fifth, and sixth millions, similar to Burckhardt's and Dase's. Burckhardt's tables (Paris, 1814-1817) contain the least factor of every number not divisible by 2, 3, or 5, from unity to three millions, and Dase's tables (Hamburg, 1862-1865) give similar information for the seventh, eighth, and ninth millions. Dase undertook the calculation at the suggestion of Gauss, who urged him to begin at 6,000,000, as the three millions between 3,000,000 and 6,000,000 had been calculated by Crelle and presented to the Berlin Academy, and Gauss did not doubt that they would be published sooner or later. It appears, however, that the Berlin manuscript is too inaccurate to admit of publication, and therefore, in order to fill up the gap, it is necessary to undertake the calculation again, as the nature of the work is such that errors committed do not readily admit of discovery and correction. Mr. Glaisher has completed the portion from 3,000,000 to 4,039,500, which is ready for press, and the remaining two millions are being actively proceeded with.

Report of the Committee on Oscillation Frequencies of the Rays of the Solar Spectrum.—Mr. G. J. Stoney explained the objects of the Committee, and stated that in the table now published the oscillation frequencies of the principal rays of the visible part of the solar spectrum have been computed from Angström's determinations of their wave-lengths in air, combined with Ketteler's observations on the dispersion of air. Such a table and its accompanying map afford the most assistance that can be given towards the detection of harmonic relations, for rays that are harmonically related are therein represented in the simplest form practicable; in the table by an arithmetical series of the same type as the series of natural numbers, where the common difference is equal to the first term; and on the map by a series of equidistant lines.

Report of the Committee on Luminous Meteors.—Mr. James Glaisher read this report, which consisted of (1) an account of meteors doubly observed, with a table showing their real paths, velocities, and radiant points; (2) a detailed account of large meteors; (3) general directions and instructions to observers for recording meteors and aërolites, by Prof. A. S. Herschel; (4) the discussion of a meteor of short period (viz., the fireball of November 27, 1877, for which a short period, such as, say, 500 days, is found), by Capt. G. L. Tupman; (5) an elaborate analysis of the constituents of masses of meteoric iron and stone-falls, by Dr. W. Flight.

Report of the Committee on Underground Temperature.—Prof. J. D. Everett read this report. The principal novelty was the proposal to make observations in filled up bores by a thermo-electric method. Two wires, one of iron and the other of copper, each covered with gutta-percha, were to be joined at both ends, where a portion would be left uncovered. One junction would be buried in the bore, while the other would remain above ground available for observation. A current would flow through the circuit composed of these two wires whenever the two junctions were at unequal temperatures, and the observer would immerse the accessible junction in a basin of water containing a thermometer, and would regulate the temperature of the water until he found by a galvanometer that no current passed. He would then know that the temperature of the water as indicated by the thermometer was the same as that of the buried junction.

SECTION A.—MATHEMATICAL AND PHYSICAL.

Researches made at Dunsink on the Annual Parallax of Stars, by Prof. R. S. Ball.—The author stated that it was, of course, well known that up to the present time no parallax of a star had been detected which exceeded a single second of arc. In the great majority of cases the parallax was very much less, even if it was appreciable. But when they reflected that not one star out of ten thousand had yet been regularly examined for parallax it was obvious that it would be rash to conclude that there were no stars nearer to us than those of which the distance was already known. In selecting objects for investigation of annual parallax astronomers had generally chosen those stars which were exceptional either on account of their brilliancy or the largeness of their proper motions. Either of these features in a star afforded, doubtless, a *primâ facie* presumption that the star was comparatively near the earth. On the other hand, even Sirius had, according to Gylden, a parallax of only one-fifth of a second, while for another star, which had the enormous proper motion of seven seconds annually, Brunnow had found a parallax not greater than one-tenth of a second. The presumptions of nearness founded on great brilliancy or great proper motion, except, perhaps, in the case of 61 Cygni, could hardly be said to be justified by the results of observation. There was, however, a presumption that some of the red stars might be near the earth, and that some of the variable stars were really small, and therefore, as they were visible, comparatively near us. Before commencing the observations described and tabulated in the paper a working list was formed, containing red stars, variable stars, stars with large proper motions, and several other stars which were chosen on different grounds. The observations had the special object of seeing whether any of them had a large parallax. Forty-two different objects had been selected from this working-list, but in almost every case the observations con-

vinced him that the parallax was certainly less than one second, and most probably did not exceed half a second. It would therefore be understood that the results were purely negative so far as the immediate object in view was concerned, as they did not suggest the existence of any parallax worth following up. The principle upon which the reconnoitring observations were conducted was this :—The effect of annual parallax upon a star was to make the apparent place of the star describe a minute ellipse, of which the mean place of the star occupied the centre. The star was observed twice. At the first observation the star was at or near one of the extremities of the major axis of the ellipse; at the second observation it was at the other extremity —so that the observations were so arranged that in each case parallax would have the greatest effect it was capable of producing.

Lord Rosse gave a *Description of an Equatorial Mounting for a Three-Foot Reflector*.—The optical arrangements of the telescope recently erected at Parsonstown was exactly similar to that of previous telescopes, and it was only the mounting which was different. The wooden tube, however, which was formerly formed of staves, had been replaced by an iron tube, which was constructed after designs by Mr. Bindon Stoney. The leading peculiarities of the mounting were that the points of reversal were situated at the east and west instead of at the north and south. The bearings on which the instrument turned in right ascension were smaller than in the ordinary mountings. The motions in declination and in right ascension were effected by means of screws, so that on a windy night the instrument could not run away with the observer. The tube was square; the clock was connected with a strap, and the counterpoise was less than usual. The cage for the observer was independent of the mounting, moving on a circular rail, and with a second motion like that of a derrick crane. The only reflector of a similar size mounted equatorially was that constructed by Mr. Grubb for the Melbourne Government. Lord Rosse illustrated his explanation by means of models of his own and of the Melbourne reflector.

On the Stanhope "Demonstrator" or Logical Machine, by R. Harley, F.R.S.—Towards the close of the last century a logical instrument was constructed by Charles, third Earl of Stanhope. The present Earl found the instrument and some fragmentary papers on logic among the relics of his ancestor, and at the suggestion of Mr. Spottiswoode, placed them in the hands of Mr. Harley, who has made a careful study of them. Earl Stanhope (born 1753, died 1816) is known to science chiefly by his printing press, microscopic lens, arithmetical machine, the monochord, and steamboat. But of his logical speculations, which occupied his thoughts for thirty years, and of his curious contrivance for working logical problems, called by him the demonstrator, nothing has been known. The author did not attempt to give a complete or systematic exposition of the Earl's logical system; but he brought out those points which serve to illustrate the demonstrator as a means of performing logical inference. He noticed that Stanhope anticipated George Bentham, Sir W. Hamilton, George Boole, and others in the quantification of the predicate, and notably De Morgan's rule for the numerically definite syllogism. Stanhope states the rule as applicable to all syllogistic reasoning, and he constructed his demonstrator for the mechanical working of this rule.

On Edmunds' Electrical Phonoscope, by W. Ladd.—This is an instrument for producing figures of light from vibrations of sound. It consists essentially of three parts—an induction coil, an interrupter, and a rotary vacuum tube.

The action of the instrument is as follows :—Sounds from the voice or other sources produce vibrations on the diaphragm of the interrupter, which, being in the primary circuit of the induction coil, induce at each interruption a current in the secondary coil, similar to the action of a contact breaker, or rheotome; therefore each vibration is made visible as a flash in the vacuum tube.

The tube revolving all the time at a constant speed, the flashes produce a symmetrical figure, as the spokes of a wheel, as in the Gassiot's star.

The number of spokes or radii are according to the number of vibrations in the interrupter during a revolution of the tube, and the number of vibrations being varied to any extent according to the sounds produced, the figures in the revolving tube will be varied accordingly.

The same sounds always produce the same figures, providing the revolutions be constant. In case of rhythmical interruptions being produced in a given sound, as in a trill, most beautiful effects are noticeable, owing to the omission of certain radii in regular positions in the figure.

The uses of this instrument are the rendering visible of sounds and showing the vibrations required in their production, and is a mode of confirming by sight an appeal to the ear.

The phonoscope is the invention of Mr. Edmunds, partner in the firm of Ladd and Co., London, by whom it is manufactured.

On Byrne's Battery, by W. Ladd.—This is the invention of Dr. Byrne, of Brooklyn, U.S.A.—The chief features in this battery are a compound negative plate and a simple mechanical means for preventing polarisation.

The negative plate consists of the extreme negative element, platinum, backed up by a plate of copper to reduce the resistance, the copper being protected by a thin sheet of lead to prevent any local action that might occur owing to holes in the platinum, which might allow the exciting fluid to attack the copper, and a thicker sheet of lead on the back of the copper, which is japanned; so a plate in section would show as consisting of, first, a sheet of platinum, then thin lead, then copper, and last by the thick japanned lead, the whole being soldered together to form a solid plate. The batteries are built up with a zinc plate and two of the compound plates, the exciting fluid being a bichromate of potash and dilute sulphuric acid solution.

This battery would soon become polarised but for the injection of air between the plates, which action appears simply mechanical and not chemical, various gases producing no different effects.

When the air is pumped in the most extraordinary effects may be produced, the quantity being enormous, being more than double that of any other battery of the same size. It is much used in the States for surgical operations, its extreme portability and control rendering it peculiarly useful in this direction. The platinum loop can be raised to any temperature and kept at the same simply by the action of the foot on the bellows, leaving both hands at liberty for operating, there also being an entire absence of fumes or other disagreeable smells.

A battery of four small cells will heat nine inches of No. 16 platinum wire to redness.

There is also another form of this battery in which the platinum is platinised: the exciting solution is composed of one part sulphuric acid to ten of water. In this form no air is required to be pumped through the solution. This is used as a motor battery for driving sewing-machines. The inventor states he has driven a heavy Singer sewing-machine for eight hours a day at a cost of twopence, including everything. As yet nothing has been done in this direction in England.

Dr. Janssen gave an account of his method of solar photography, and exhibited some beautiful photographs of the sun, and Mr. G. J. Stoney explained his spectroscope of very large aperture. Mr. J. R. Wigham explained the quadriform group flashing gas light, as used at Galley Head Lighthouse, with illustrations, and also a gas gun, which might be fixed on a rock in the sea at a considerable distance from a lighthouse or a fog-signal station, and fired as often as required from the station, without the keeper leaving his post: a gas gun was placed in the college grounds, and was several times fired from the table of the room in which the section met. Prof. Haughton gave an account of his investigations on the sun heat received at the several latitudes of the earth, taking account of the absorption of heat by the atmosphere, and of his conclusions therefrom with regard to geological time.

The section was divided into two departments on the Monday and Tuesday. There were twenty-two mathematical communications which were read in the department of mathematics, including papers by Mr. Spottiswoode *On the Eighteen Co-ordinates of a Conic in Space*; by Prof. H. J. S. Smith, *On the Modular Curves*; by Prof. R. S. Ball, *On the Principal Screws of Inertia of a Free or Constrained Rigid Body*; by H. M. Jeffery, *On Cubic Curves*; by Mr. J. W. L. Glaisher, *On Certain Special Enumerations of Primes*, and *On Circulating Decimals*; by Mr. F. Purser, *On the Geometrical Treatment of Bi-circular Quartics*; and by Dr. Hirst *On Halphen's New Form of Chasles's Theorem on Systems of Conics satisfying Four Conditions.*

SECTION C.—GEOLOGY.

On the Influence of Strike on the Physical Features of Ireland, by Edward T. Hardman, F.C.S., Geological Survey of Ireland. —Although not often mentioned in geological works, the in-

fluence of strike in determining the lines of direction of the principal physical features of a country, is recognised by most geologists, but in few countries is the relation so distinct as in Ireland. The author was led to pay attention to this subject some years ago on reading Mr. J. F. Campbell's paper on the glaciation of Ireland,[1] in which that gentleman assumes that the south-west and north-east trend of some of the mountains of Ireland, *e.g.*, those of Donegal and Kerry, is due to the glacial action of a huge ice-sheet passing over Ireland from the south-west of Scotland. The author, after some years' examination, has found, however, that in most cases the trend of the hills, and course of rivers, &c., are determined by the strike alone, and wished to place the facts he had noted before the section.

1. *Mountains.*—The Donegal highlands trend to the south-west along the line of strike of the ancient crystalline stratified rocks. The basaltic plateau of Antrim follows in outline the windings of the outcrop of the underlying chalk, and consequently the strike of the basalt itself upheaved with it. The Mourne Mountains and Slieve Croob also coincide in direction with the stratified rocks on their flanks, except where joints or faults have given rise to minor lateral valleys, *e.g.*, Carlingford Lough. The same adherence to the line of strike is seen in the hills forming the flanks of the Wicklow Mountains; in the Kilkenny and Tipperary coal-fields; the Comeragh and Knockmealdown Mountains; and is most remarkably shown in that series of flexured carboniferous and old red sandstone rocks forming the hills of Cork and the Mountains of Kerry, the axes of which stretch from Dungarvan (co. Waterford) to Cape Clear, and Bantry Bay. The Reeks of Kerry are good examples. Its influence is again shown in the shaping of the high ground forming the Munster coal-field, and finally in the mountainous district of Connemara, although here in places obscured by the action of faults. The Twelve Pins, Muilrea, the mountains flanking Killary Harbour, and the country northwards around Nephin Mountain are striking examples. Toward the central plain the isolated mountains of old red sandstone and Silurian rocks rising through the carboniferous limestone, viz., the Slieve Bloom Mountains, the Devil's Bit, and the Galtees, conform to the same rule, the axis of strike and direction being parallel.

2. *Rivers.*—In the south of Ireland especially many of the rivers follow the windings of the strike. The Suir follows the line of strike for eighty miles, only beginning to cross it about ten miles from the sea. The Blackwater runs along the strike for seventy miles of its course, crossing it for only sixteen miles. The Lee is directed by the strike for fifty miles of its length, as is also the Bandon River for the greater part of its course, while the Shannon may be traced along the strike of the beds for by far its greatest distance.

3. *Inland Lakes.*—Most of the lakes are conformable to the strike in their greater outlines, the smaller details being determined by the jointage. Of these may be mentioned Lough Neagh, Loughs Corrib and Mask, Lough Erne—most notably—Lough Allen, Lough Derg, and the far-famed Lakes of Killarney.

4. *Sea Lochs, Bays, &c.*—The majority of these may be included—Lough Foyle, Belfast Lough, Strangford Lough, Lough Larne. The most notable examples are those on the south-west. Roaring Water Bay, Dunmanus Harbour, Bantry Bay, Kenmare River, and Dingle Bay. Also the mouth of the Shannon, Galway Bay, and Clew Bay. Farther north, the principal bays and indentations along the line of coast stretching from Broadhaven to Donegal; Killala Bay, Sligo Bay, and Donegal Bay, have been excavated in their great outlines along lines of strike.

In conclusion, the author pointed out that nature had adopted the least expensive method of working; since it is always easier to excavate along a line of strike than across the bedding. Usually cleavage, or incipient cleavage, is induced along the line of strike by the forces which upheaved the rocks, and denudation is most early effected therefore in this direction.

On the Correlation of Lines of Direction on the Globe, by Prof. O'Reilly, M.R.I.A.—The theory of correlation of the great lines of direction on the earth's surface had long been studied and applied, especially in mining; and had been generalised by Elie de Beaumont, and applied by him to the correlation of mountain chains with remarkable results, but his theory had found but little favour with English geologists. The author having been led to examine the question, based his system on angular relations actually observable in certain rocks, and in these he had found the angles 40° and 70° to bear a very important part. He gave details tending to show the relation of those angles to the main lines of direction on the surface of the globe, taking as a base line the east coast of Madagascar.

[1] *Quart. Journ. Geol. Soc.*, London, May, 1872.

On Hullite, a hitherto Undescribed Mineral from Carnmoney Hill, Co. Antrim, with Analysis, by Edward T. Hardman, F.C.S.—This mineral occurs in abundance at Carnmoney Hill, near Belfast, in the basalt forming the old neck of a miocene volcano. It has never before been described or analysed, and has been referred to on the Survey maps and in the Survey collections as obsidian, doubtless from its black colour and waxy lustre. In physical character it somewhat resembles the chlorophæite of Macculloch, but is entirely different in composition, which more resembles that of delessite. From this, however, it differs essentially in colour, hardness, and specific gravity, but it appears to belong, on the whole, to the ferruginous chlorite group.

Physical Characters.—Colour, black; hardness, about 2; lustre, waxy, but dull; before blowpipe, with difficulty fusible at edges to a black glass sometimes magnetic; very slightly affected by strong acids in the mass, but nearly entirely decomposed when boiled in powder, in strong hydrochloric acid, occurs filling and coating vesicular cavities in basalt of Carnmoney Hill, &c.

Chemical Composition.

Silica	39·43
Alumina	10·35
Peroxide of iron	20·72
Protoxide of iron	3·69
Protoxide of manganese	trace
Lime	4·48
Magnesia	7·47
Water	13·61
Carbonic acid	trace
	99·77

Formula—$(CaMgFe'')_3\ (AlFe''')_4\ Si_6O_{21} + 7H_2O$.
Specific gravity, 1·76.

SECTION D.—BIOLOGY.

Department of Zoology and Botany.

On the Stipules of Spergularia marina, by Prof. Alexander Dickson, M.D.—As is well known, certain genera of *Caryophyllaceæ*, of which *Spergularia* is one, are distinguished by the presence of stipulary appendages. On examining lately the stipules of *Spergularia marina*, I was struck with a peculiarity presented by them, which, if observed at all by descriptive botanists, has not received the attention it deserves on account of its remarkable character. The stipules are free from the petioles and wholly cellular in structure. From connation of those of opposite leaves they form interpetiolar stipules with more or less regularly, though slightly bifid, extremities. Lastly (and this is the important point), these stipules are united to each other round the backs of the petioles, so that a sheath is formed completely surrounding the axis and the two leaf-bases. This connation of stipules round the backs of the petioles is very interesting as being a rare phenomenon. Cases are not uncommon where the two stipules are connate on the inner side of the leaf-base, constituting the so-called "axillary stipule," *e.g.*, *Potamogeton lucens*, &c., or on the opposite side of the axis from the leaf, *e.g.*, *Ficus elastica*, *Ricinus*, *Astragalus alpina*, &c., constituting the "oppositifoliar" stipule; but the only reference to connation behind the leaf-base I can find is in the case of certain *Astragali*, by St. Hilaire, in his *Morphologie*. In those species of *Astragalus* which I have examined I have not seen any one in which the stipules are actually connate in this way; but in some, *e.g.*, *A. alopecuroides*, the bases of the stipules extend round the back till they meet—a condition just short of connation. In *Spergularia*, as we have seen, we have the interesting combination of the interpetiolar connation with connation round the back of the leaf. In *English Botany* I observe that the condition is fairly enough represented by the artists, but, as I have already indicated, the morphological peculiarity does not seem to have impressed itself on the botanical mind.

Dr. Bayley Balfour remarked that a good deal of confusion existed as to the application of the term stipule, and showed that in some cases it was applied to structures of very different

appearance, and perhaps even of varying morphological significance.

On the Inflorescence of Senebiera didyma, by Prof. Alexander Dickson, M.D.—When at Plymouth last August during the meeting of the British Association, I took the opportunity of examining *Senebiera didyma*, a weed which grows in great abundance on road-sides and waste places about the town, and I was much struck with a remarkable peculiarity to be observed in connection with its inflorescence.

The inflorescence is like that of the mass of cruciferous plants, racemose. The racemes are "oppositifoliar," and at first sight the arrangement seems to be analogous to that of the oppositifoliar inflorescences of *Vitis* or of *Alchemilla arvensis*, where the inflorescence is really terminal, but thrown to the side by preponderant development of a "usurping shoot," the axillary bud of the last leaf produced by the primary axis before ending in the inflorescence. This view seems further supported by the fact that of all the foliage leaves, that opposite the raceme is the only one apparently destitute of an axillary bud, which on the supposition would be represented by the "usurping shoot." If, however, the plant is more closely examined, a very remarkable condition is disclosed, one, indeed, which offers a morphological problem of considerable difficulty, and which, probably, can be effectually solved only by developmental study. The peculiarity consists in the constant occurrence of a solitary flower springing somewhere from the internode below the raceme either about half way down towards, or almost close to the level of the leaf below. So far as my observations go, the solitary flower is never quite as low as the level of the lower leaf. It might be supposed that from almost immediately above the second last leaf of the main axis, the bases of the terminal raceme of the "usurping shoot," and of the axillant leaf of that shoot had all become fused together. Now, although cases are known on the one hand, of adhesion between the base of a terminal flower and that of the usurping axis (*e.g.*, *Helianthemum vulgare*, Payer), and, on the other hand, between the base of an axillant leaf and that of the usurping shoot in its axil (*e.g. Sedum*, sp. Payer), we do not know of connation of all three together. It is possible, but I think improbable.

The view which, after careful consideration, occurs to me as most fully satisfying the conditions of this remarkable case, may be stated briefly in categorical form as follows:—

1. The racemose inflorescence is terminal and properly begins just above the level of the "second last" leaf. It would thus include the aforesaid solitary flower.

2. The raceme, after producing one ebracteate flower, produces at its second node a foliage leaf from whose axil the "usurping shoot" springs.

By such an explanation we can dispense with any cumbrous adhesion hypothesis such as I have indicated above. The peculiarity is that the main axis does not, *per saltum*, pass from the condition of a leafy axis to that of an axis of inflorescence, but begins by producing one flower and then developing a foliage leaf beyond which the series of flowers is uninterrupted. The "usurping shoot," as above indicated, represents the axillary bud of the foliage leaf by which the raceme is interrupted.

On the 6-celled Glands of Cephalotus and their Similarity to the Glands of Sarracenia purpurea, by Prof. Dickson.—Dr. Dickson pointed out that the peculiar 6-celled glands found on the external surface of the pitcher, both surfaces of the pitcher-lid, and both surfaces of the foliage-leaf of *Cephalotus* are very nearly identical in structure with the glands on both inner and outer surfaces of the pitcher of *Sarracenia purpurea*, which were originally described by August Vogl. Dr. Dickson suggested that the remarkable resemblance in this respect, taken in connection with certain correspondence in the details of the insect-trapping apparatus might suggest an affinity not hitherto suspected.

Exhibition of Plants of Isoetes echinospora.—Dr. Dickson exhibited specimens referable to this species which he lately found growing on muddy bottom among Potamogeton in about two feet of water in Loch Callater, Aberdeenshire. The plants were remarkable for the very slender and tapering character of the leaves which curve outwardly. The spores are very markedly echinate, and in diameter about one-fourth smaller than those of *I. lacustris*.

Dr. Moore, Glasnevin, exhibited remarkable specimens of an Isoetes from Lough Bray, co. Wicklow. They were of great size, much above the average of specimens of *I. lacustris*. Prof. Suringar and Prof. M'Nab suggested that it might be the same as the Italian species known as *Isoetes malinverniana*.

Mr. Britten exhibited, on behalf of Mr. J. H. A. Jenner, specimens of *Rumex maximus*, Schreb, from a new locality on the Cuckmere River, East Sussex. The specimen sent showed the various characters by which *R. maximus* can be distinguished from *R. hydrolapathum*, the species with which it has been confounded.

Dr. Bayley Balfour exhibited, on behalf of Mr. Sadler, specimens of *Salix Sadleri* and *Carex frigida* obtained at the beginning of August in Corrie Chandler, Aberdeenshire. These plants were discovered in 1874 by Mr. Sadler, and have not been met with again until this year.

Notes on Naiadaceæ, by Dr. Bayley Balfour.—Dr. Balfour more particularly described some of the peculiarities observed by him in the genus Halophila, an extremely interesting tropical phanerogamous plant.

Dr. Price, of Chester, sent for exhibition portions of the leaves of *Cardamine pratensis* producing numerous gemmæ.

On the Supposed Radiolarians and Diatomaceæ of the Coal-Measures, by Prof. W. C. Williamson, F.R.S.—Prof. W. C. Williamson called attention to the *Traquariæ* of Mr. Carruthers, found in the lower coal-measures of Lancashire and Yorkshire, with small spherical objects that observer believes to be radiolarians like those still living in existing seas. Prof. Williamson showed that the radiating projections with which these spheres are surrounded were not siliceous spines like those of the Radiolariæ, but extensions of a continuous membrane which enclosed the entire organism, and which therefore could not have the spicular nature attributed to them. He then demonstrated that within this external membrane is a second inner one, which latter is filled with numerous small vegetable cells, like others shown to exist in the interior of fossil spores and reproductive cryptogamous capsules, found in the same beds as those which furnish the *Traquariæ*.

These conditions are so different from those existing in any known recent species of radiolarian as to lead Prof. Williamson to reject the idea of their radiolarian character; whilst their close organic resemblance to some obviously vegetable conceptacles found in the same coal-measures suggest that the *Traquariæ* are also vegetable structures.

The mountain limestone deposits of some British localities contain a vast multitude of minute calcareous organisms which Mr. Sollas and other observers have regarded as radiolarians. These structures, however, seem to exhibit no satisfactory evidence of being so. In the first place these organisms are now calcareous instead of siliceous. It has been suggested that their siliceous elements were removed, and replaced by carbonate of lime, but this appears to be most improbable.

Prof. Roscoe and Prof. Schorlemmer agree in stating that they would require overwhelming evidence before they would be prepared to accept such an explanation of the present condition of these objects or of the fact of the substitution of carbonate of lime for silica, that such an explanation renders necessary.

Count Castracane has published an account of a process by which he reduced numerous specimens of coals to very minute quantities of coal-ash, and has stated that he found in these ashes numerous marine and fresh-water diatomaceæ. Prof. Roscoe kindly allowed one of his ablest assistants in his laboratory at Owens College to prepare analyses of a number of coals according to Count Castracane's method. The residual ashes of these preparations have been mounted microscopically by Prof. Williamson, and in no one of them can a trace of a diatom be found. Beyond stating the fact he is wholly unable to account for the discrepancy between his results and those of the Italian observer, so far as his present observations go, he finds himself compelled to conclude that we have no proof of the existence of radiolarians or of diatomaceæ in the British carboniferous rocks.

A short discussion ensued, in which Sir Joseph Hooker, Prof. M'Nab, and Dr. Bayley Balfour took part, the views expressed coinciding generally with those of Prof. Williamson.

On the Association of an Inconspicuous Corolla with Proterogynous Dichogamy in Insect-fertilised Flowers, by Alex. S. Wilson, M.A., B.Sc.—The majority of conspicuously-coloured flowers whose cross-fertilisation depends on their being easily seen by insects, are proterandrous. Such plants have their flowers placed in close inflorescences, as, for example, in Erica, Calluna, Vaccinium, Digitalis, Linaria, Gladiolus, &c., and occasionally the flowers are secund, or placed on one side of

the axis, thus becoming more conspicuous. In the indefinite mode of inflorescence the older flowers are placed at the lower part of the flowering axis; hence in the commonest form of inflorescence with proterandrous flowers, the lower flowers are in the second or female stage at the time when those in the upper part are in the first or male stage. In proterogynous dichogamy with indefinite inflorescence, the older flowers are in the second or male stage when the upper and younger flowers are in the female stage. In *Scrophularia nodosa* we have a plant in which proterogynous dichogamy is associated with an *inconspicuous* corolla. The stigma after fertilisation is removed out of the pathway to the nectar by the bending back of the style on the outside of the corolla, while the stamens straighten out to occupy the place formerly held by the stigma. The corolla is small and obscurely coloured, being greenish, tipped with brown. The inflorescence is lax, and the flowers scattered all round the axis. The odour of the flowers and the presence of a nectariferous gland shows that the plant is fertilised by insects, and not by the wind. Among such inconspicuously-coloured flowers, proterogynous dichogamy seems to prevail, just as proterandry is characteristic of brightly-coloured flowers. Hitherto it has not been shown how an entomophilous plant could advantageously possess a small uncoloured corolla, and be proterogynous. Watching the mode in which wasps visited the *Scrophularia nodosa* afforded the solution of the problem. The first flower visited by the wasp was the top one, and it passed irregularly downwards from flower to flower, and left the inflorescence by the lowest flower. Bees, when collecting honey, do the reverse, visiting the lowest flower first, and proceeding from flower to flower in regular succession from below upwards, leaving by the top flower. The order in which the flowers are visited is therefore of the greatest importance. In Gladiolus, for example, the bee begins at the lowest flower, and will deposit any pollen brought by it from a neighbouring spike, and as it passes upwards, it will get from the *upper flowers*, a fresh supply of pollen to apply to the lower flowers of another spike. In *Scrophularia nodosa* the wasps, which are less highly specialised as honey collectors, chiefly visit the flowers and proceed from above downwards, leaving the inflorescence with pollen from the lower flowers to apply it to the stigmas of the proterogynous upper flowers.

Wasps differ from bees in one important point, viz., that while bees are purely vegetable feeders, wasps add to a vegetable diet by preying largely on insects smaller than themselves. Throughout the animal kingdom carnivora are endowed with keener powers of vision and scent than vegetable-feeding creatures. That keenness of vision which enables a wasp to descry its prey at a distance, aided by its acute sense of smell, in all probability also enables it to discover these obscure flowers, without the guidance afforded by a coloured corolla, the materials that would be required for its production being employed more economically by the plant, just as in cleistogamic flowers. The wasp also gains an advantage, as it has a better chance of finding honey in these obscure flowers on account of their being easily overlooked by insects less highly endowed as regards powers of scent and vision.

Notes on Dimorphic Plants, by A. S. Wilson, M.A., B.Sc.—The author pointed out that *Erythræa centaurium* was probably dimorphic, as it exhibited heterostyly, and had two kinds of pollen-grains. *Silene acaulis* was shown to have three kinds of flowers, male, female, and hermaphrodite, thus resembling *S. inflata*, which Axel has shown to be triœciously polygamous.

Some Mechanical Arrangements Subserving Cross-fertilisation of Plants by Insects, by A. S. Wilson, M.A., B.Sc.—The plants considered were *Vinea minor*, *Pinguicula vulgaris*, and the foxglove, and the author described the various structural peculiarities in the different flowers.

THE FRENCH ASSOCIATION

As might have been expected, M. Krantz has been appointed president for the Congress of 1880; but, contrary to all expectation, the decision of the Council who had proposed Algiers as the place of the meeting for 1880 has been altered, and Rheims has been chosen by a large majority. This unexpected vote will create some dissatisfaction in the colony, where great expectations had been raised by the coincidence of the anticipated arrival of the Association and the celebration of the fiftieth anniversary of the conquest.

Dr. Janssen delivered, in the large room of the Sorbonne, a lecture on the present state of physical astronomy, which was completely successful; but it was deeply regretted that no direct news had come of the eclipse and the discovery of Vulcan.

A great *soirée* was given in the Conservatoire des Arts et Métiers, and M. Cornu delivered, in the large hall, an able lecture on Polarisation. The most important part of the display was a series of twenty Jablockhoff lights, exhibited in the gardens. The effect, although splendid, cannot be said to have been better than in the Avenue de l'Opéra and round the Arc de Triomphe.

A banquet of 200 covers was given on Thursday, at the Continental Hotel, to M. Bardoux, the future president of the Montpellier meeting, and present Minister of Public Instruction. On the following evening a great reception was held at the Ministry of Public Instruction, M. Bardoux having opened his *salons*, not only to the members of the Association, but also to the delegates of public schools now visiting the Exhibition at the public expense.

The Paris meeting, however, has been comparatively lost amongst the many special congresses which are taking place without interruption in the Trocadéro, and of which none attracts much public notice. Meteorology had its special congress, holding its sitting at the very same hour when the meteorological section of the French Association was deliberating. Although a large number of influential meteorologists had congregated, none of them could find the means of attending regularly both meetings.

No paper of real importance has been read in any of the sections.

In provincial cities the coming of the Association is always coupled with the inauguration of some public monument, library, museum, schools, &c. No similar ceremony took place in Paris, so that in that respect, as in many others, this thriving Association may be said to have lost a year.

M. Frémy, in his presidential address on soda and steel, traced the history of the improvements in the manufacture of these productions during the past century, showing that at every important stage science stepped in and pointed out the direction which practice ought to take in order to secure progress and success. Science was always at her post, ready to solve, to the advantage of industry, all the problems proposed to her. In showing the important services that science has thus rendered to industry and to the country, he wished to prove that in supporting men of science, by encouraging scientific production, we give to the country natural forces and accomplish a patriotic act. Blind and egoistic spirits have dared to say that science has no need of encouragement, that the true man of science forms himself all alone, that he knows how to triumph over obstacles, that the difficulties which he meets with are necessary trials which only arrest mediocrity, and that he who stops by the way, wanting the scientific inspiration, deserves his fate. Such affirmations are only maintained by those who have not known the difficulties of the scientific career, and who often owe their advancement to favour. We could, alas! cite many examples which prove that the most ardent and courageous man of science may be arrested in his labours by invincible obstacles. M. Frémy then referred in warm terms to the many generous individuals who in France have come to the aid of scientific research, and at the same time said it would be unjust not to recognise all the efforts which have been made in recent years by the state to maintain in France the higher scientific studies. Magnificent and well-endowed laboratories, new scientific chairs, the school of higher studies—these are some of the services rendered by the French government to science. But it is necessary to attract to these laboratories men who are capable of making good use of them, men who really possess the vocation for scientific research, and to prepare a scientific generation to succeed the present. M. Frémy then showed that the French Association might help greatly in promoting this service to science.

PROF. HAECKEL ON THE DOCTRINE OF EVOLUTION

On Thursday, August 28, a banquet was given to Prof. Haeckel, at the Grand Hotel, Paris, by a number of his admirers, on the occasion of his presence at the Paris meeting of the French Association. A congratulatory address was delivered in the name of the Reception Committee by M. Jules Soury, one of the Staff of the National Library, who said

that men of science were witnessing a secular movement of renovation in France as well as in Germany. Prof. Haeckel then gave an address, containing one of the most uncompromising statements of what he believes to be the logical consequences of his doctrine that has been given by any living evolutionist. We translate from the *Revue Scientifique*.

"The friendly support which I have received in your midst touches me more than I shall say, for it is not only the man and the works which he has been able to produce that you have wished to honour; our profound love of scientific truth, our philosophical beliefs, our faith in the theory of evolution and in that doctrine of descent for which I venture to say I have already fought well, this, gentlemen, is the secret of the sympathy which unites us to-day. I have seen with great joy in the meetings of the scientific congress how the theory of evolution has already, whatever may be said, penetrated the spirit of French *savants*. In the sections of Biology the theory of transformism has appeared to many speakers the only explanation of the phenomena of life which they have studied. The last consequence of that doctrine—the descent of man, not only from the apes, but from all the series of lower organic forms, has even been proclaimed and vigorously defended by M. de Mortillet in the Anthropological Section."

"Prof. Haeckel then proceeded to state the doctrine of the descent of man, so well known in connection with his name. "Certainly," he said, "man does not descend directly from any existing anthropoids. No serious naturalist has professed that doctrine, which has currency only among the general public and theologians. For a long time frivolous and ignorant people have found a subject of pleasant and innocent gaiety in the thought that we wish to pass them off as improved apes. No one dreams of this; but certain professors of philosophy, and a number of facetious preachers, nourish this prejudice, which brings them fine and easy successes. They do not seem to have any idea that they furnish the best argument in favour of that theory, if it is sustainable. Are not their *naïve* pride, their infantine vanity, weaknesses of character which the apes have left us as a legacy? There can be no doubt that man and the apes of the Old and New World are descended from a common ancestor.

"That which, sooner or later, will lead all good minds to transformist doctrines is the feeling, every day more profound among us, of universal causality, of development, of continuity in nature. The number increases every day of those who seek the truth, the whole truth, and who rest only in the clear vision of the universal connection of effects and causes.

"Reason, causality, mechanism, on one side; superstition, mysticism, teleology, on the other. The theory of evolution, which considers and embraces entire nature as one whole, has replaced final causes by efficient causes. This has already been accepted, at least by philosophical minds, the only ones of which we need take count, for the old doctrines of the final causes of the unwise, the immutability of species, sterility of hybrids, geological catastrophes and successive creations, the impossibility of spontaneous generation, and of the youth of man on the earth.

"We cannot say at what moment of time nor under what conditions the first living beings appeared at the bottom of the sea, but there can be no doubt that they have been formed chemically from inorganic carbon compounds. The primitive monads were born by spontaneous generation in the sea, as saline crystals are born of their mother-waters. There does not exist, in fact, any other alternative to explain the origin of life. He who does not believe in spontaneous generation, or rather in the secular evolution of inorganic matter, into organic matter, admits miracle. It is a necessary hypothesis, which cannot be ruined either by *à priori* arguments or by laboratory experiments.

"The time has arrived to replace the antique dualistic and neologicalconception of life and spirit by the monistic or mechanical conception of the universe. We have arrived at the boundaries of the old and new faith. Mystery exists, perhaps impenetrable; in any case, scholastic arguments will not pierce it. The doctrine of final causes has all the *naïveté* of the explanations which prevail among savages and children; the theories of Lamarck and Darwin have given the last stroke to that decripit doctrine. Modern morphology is irreconcilable, not only, I say, with the dogma of creation, but with that of a Providence or of a vague idealistic Pantheism, of the kind associated with the names of Hegel, Schopenhauer, and Hartmann. If there certainly exists in reality, as I have striven to show, an etiological connection between individual development and the development of ancestry, between ontogenesis and phylogenesis, the phenomena of human embryology are only *mechanical* and necessary effects of the evolution of our remote ancestors, conformably to the laws of heredity and adaptation.

"Seventy years ago, permit me to remind you, the great Lamarck created the theory of descent, which Darwin, half a century after, was to develop by fecundating it with his doctrine of selection, founded on the physiological properties of heredity and adaptation. Goethe had also conceived that doctrine very philosophically. For it is the honour of our conception of things to have seduced philosophers, poets, and critics, such as Kant, Goethe, and Strauss.

"These great and noble geniuses saw imperfectly, gentlemen, that which we see better to-day; I mean to say that the theory of evolution is only a particular case of the most vast of cosmical hypotheses, that of the transformation and conservation of the physical forces. This is what the best minds, the most judicious and wisest, such as the eminent naturalist of Montpellier, of whom France ought to be proud, Prof. Charles Martins, now admit with entire good faith. According to Prof. Martins, in fact, "the theory of evolution binds together all questions of natural history, as the laws of Newton have bound together the movements of the celestial bodies. *That theory has all the characteristics of the Newtonian laws.*"

"Certainly, the laws of life, morphological laws, the laws of transformation of living beings, under the influence of adaptation and heredity, of selection and vital concurrence, are not susceptible of the mathematical rigour of the laws of astronomy. We cannot, however, doubt that they exist, as we do those of psychology, ethology or science of character, and social science. It is, I think, somewhat *naïve* to insist, as is often done, on the numerous anomalies which are observed among living human beings. These anomalies are only apparent as are perturbations in astronomy. If we possessed all the elements of these morphological laws, the solution, at least in part, of which I have at heart, we should see that these apparent anomalies are explained by the general laws of mechanics. No one denies that the extreme instability of the elements constituting the woof of organised beings renders biological problems of an infinite complexity.

"Our mission—to which we have succeeded after the great heroic generation of *savants* of the eighteenth century—for they were heroes, gentlemen, and the greatest of all perhaps, the Laplaces, Lavoisiers, Kants, Lamarcks, Frederick Wolffs—our mission to all, naturalists, physiologists, physicians, philosophers, linguists, historians, is to continue those traditions of powerful thought and manly love of liberty which made our grandfathers almost the equals of those Greeks of Ionia and Attica whom we venerated in our infancy as the fathers of all human science."

PROF. NORDENSKJÖLD ON THE COMPOSITION AND COMMON ORIGIN OF CERTAIN METEORITES [1]

PROF. NORDENSKJÖLD on comparing the composition of the meteorites which fell at Ställdalen in Sweden on June 28, 1876 (NATURE, vol. xvi. p. 238), with that of a number of other meteorites, has found that a remarkable similarity, if not identity, is disclosed by excluding the larger or smaller quantities of oxygen and sulphur which enter into their composition, and taking into consideration only the metallic constituents, irrespectively of their being oxidised or not. This similarity in composition is found to exist between various meteors, which, according to the common method of giving the results of analyses of meteorites, that is, by stating separately the metallic iron, sulphide of iron, soluble and insoluble silicates, &c., appear to be of quite dissimilar nature and composition. The meteorites compared are:—

I. Erxleben, 1812, April 15, analysed by Stromeyer.

II. Lixna, 1820, July 12, analysed by A. Kuhlberg.

III. Blansko, 1833, November 25, analysed by Berzelius.

IV. Ohaba, 1857, October 15, analysed by Bukeisen.

V. Pillistfer, 1863, August 8, analysed by Grewingk and Schmidt.

VI. Dundrum, 1865, August 12, analysed by Haughton.

VII. Hessle, 1869, January 1. *a*, analysed by G. Lindström,

[1] Abstract of paper in *Trans.* Geol. Union of Stockholm, 1878, No. 44.

pieces of a large stone; *b*, average of two analyses by Nordenskjöld of whole stones, weighing 1,063 and 0·64 grm.

VIII. Orvinio, 1872, August 31, analysed by L. Sipöez. *a*, chrondritic ground-mass; *b*, black connecting mass.

IX. Ställdalen, 1876, June 28, grey ground mass, analysed by G. Lindström.

Composition of Meteorites, excluding the Oxygen, Sulphur, Phosphorus, and Chlorine found in them.

	Si.	Mg.	Fe.	Ni.	Co.	Mn.	Ca.	Al.	Na.	K.	Cr.	Sn.
I.	26·11	21·79	44·29	2·43	—	0·83	2·13	1·31	0·85	—	0·26	—
II.	26·70	23·61	42·90	2·68	—	0·66	trace	2·12	0·83	trace	0·50	—
III.	26·91	23·22	43·12	1·59	0·09	0·56	1·02	1·85	0·85	0·25	0·42	0·12
IV.	26·12	21·52	47·82	2·75	—	0·18	—	0·23	1·12[1]	—	0·26	—
V.	28·02	22·09	42·99	2·92	—	0·01	0·53	2·07	0·39	0·31	0·53	0·14
VI.	27·55	20·45	44·74	1·58	—	0·44	2·09	0·70	0·72	0·66	1·07	—
VII. *a*.	26·26	21·28	43·57	3·29	0·03	0·50	1·97	1·94	1·05	—	0·08	0·03
b.	26·43	23·07	41·37	3·30	trace	trace	2·28	1·27	1·78	—	0·49	0·01
VIII. *a*.	26·09	21·28	43·29	3·16	—	—	2·46	1·75	1·59	0·38	—	—
b.	26·65	20·18	42·55	4·71	—	—	2·56	1·91	1·10	0·34	—	—
IX.	25·66	21·41	44·83	2·73	0·26	0·29	1·77	1·74	0·71	0·18	0·42	—

Every one who has had experience of the analytical examination of meteorites, which is often very difficult, or at least tedious, writes Prof. Nordenskjöld, and who knows the difficulty of obtaining any proper average sample on account of the preciousness of the material, will perhaps see that here the question is no longer concerning an accidental similarity in the figures obtained, but an actual identity, showing that all those meteorites which have fallen in the course of more than fifty years form a natural group having a common origin. I have not yet been able to treat in the same way all the accessible analyses which, when those that are quite trustworthy are only in question, are less numerous than is commonly supposed. I consider it certain that it will be possible to arrange several other similar natural groups, and that very many other meteorites than those here enumerated belong to this group, which perhaps may be called Hessleites after the most abundant, most completely examined and analysed meteor fall.

It appears to me highly probable that all Hessleites belonged either in a completely metallic or in a *fully* oxidised condition to the same swarm of meteors revolving in our solar system, and that the differences in composition now exhibited by the meteorites belonging to the same group depend on changes to which the meteorites were afterwards subjected by being heated under the influence of oxidising or reducing substances.

With respect to the group now in question it is clear, from the microscopic structure of these meteorites, that the metallic iron forms their most recent constituent, and that it has thus arisen through reduction of the ferriferous silicates.

Where has this reduction proceeded? Probably not in the atmosphere of our globe, though the carboniferous substances which occur in a great number of fire-balls may very well form the necessary reduction material; possibly on the exploded heavenly body, of which these meteorites, according to a sufficiently hazardous and probably incorrect hypothesis, may form fragments; most probably, perhaps, in passing the perihelion, during the revolution of the meteor swarm round the sun.

That, besides, both reducing and oxidising influences, if on a smaller scale, make themselves felt during the short path of the meteors in our atmosphere, is shown on the one hand by the shining iron particles which are often found on the surface of the meteorites, and on the other hand by a comparison of the analyses of the large and small meteorites from Hessle; for while the large contain a considerable quantity of sulphur (1·88 per cent.), the small are nearly free of it (containing only 0·18 per cent.), clearly for the reason that the sulphur in them has been oxidised and driven off.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

FROM the Twenty-fifth Report of the Science and Art Department, we learn that the number of schools examined in 1877 was 1,348, the number of pupils under instruction being 55,927. These numbers are smaller than in 1876. This decrease was entirely due to the withdrawal of physical geography from the list of subjects for which aid is given; for in the remaining subjects a substantial increase had taken place. The subject added to the list when physical geography was withdrawn, viz., physiography, has already secured a large amount of attention; and it is probable that no less than 5,000 candidates will present themselves for examination in it in May next. The above-mentioned 1,348 schools comprised 4,635 different classes, from which 32,112 students came up for examination in May, in addition to 3,230 self-taught students and pupils in classes not taught by certificated teachers. From the results of the examination it is seen that the number of papers passed compares favourably with the statistics of previous years. The number of candidates who came up in honours was 1,029, of whom 85 passed in the first class and 192 in the second class. From the reports on the general character of the examinations which have been received from the examiners, it appears that the results of the examinations are generally of an encouraging nature. Some of these reports contain very valuable suggestions as to methods of teaching, &c., and they have been printed and circulated among the schools. The number of competitors for Whitworth Scholarships in 1877 was sixty-eight. Of these nineteen of the most successful in the theoretical subjects of competition were admitted to go forward to the examination in practical workmanship, which was as in previous years, held at the workshops of Sir J. Whitworth and Co., in Manchester. The Committee of Council state that they have received from the Council of the Royal Society a report of the work done by the gentlemen to whom grants had been made during the year 1877–78 out of the vote of 4,000*l*. for research. This vote was first made in the year 1876–77, the correspondence with reference to it being given in our last, the Twenty-fourth Report, at p. 7 of the Appendix. But owing to the period of the year at which the vote was finally sanctioned, the recommendation of the Council of the Royal Society was not received till March 16, 1877, and only 2,195*l*. 1*s*. 6*d*. came into payment out of that vote, the remainder being returned to the Exchequer. Considering the nature of these inquiries and the time necessarily devoted to preliminary experiments, it was not to be supposed that there would be much definite result to show in the first year. But on the whole very satisfactory progress has been made, and much good work already accomplished, several valuable papers having been contributed to the Royal Society.

WE would draw the attention of teachers in London and its neighbourhood to the admirably organised teachers' classes at St. Thomas Charterhouse School of Science. The session commences on the 28th inst., and those wishing for information should apply to the organising secretary, Mr. C. Smith, at St. Thomas Charterhouse Schools, Goswell Road.

AT a recent meeting of the council of the Yorkshire College, the following appointments were made:—As Lecturer in German and Oriental Languages, Joseph Strauss, Ph.D.; as Lecturer in French, John Willis, Ph.D. Both the new lecturers will commence their duties with the coming session, in October.

WE have received a very elaborate programme of the mathematical courses for the session 1878–9, under Prof. Sylvester, at Johns Hopkins University, Baltimore. Other circulars give a similar announcement in respect to the courses to be followed in languages, chemistry, physics, and biology.

THE University of Helsingfors will celebrate the fiftieth year of its existence during this autumn. The Finlandic University was originally at Abo, and was transferred to Helsingfors in 1828.

THE University of Zürich has just bestowed the title of Doctor of Jurisprudence on a young Russian lady, who obtained the highest honours in her examination for this degree.

AN institution for the higher education of ladies will shortly be opened at Kieff.

SOCIETIES AND ACADEMIES

LONDON

Entomological Society, August 7.—H. W. Bates, F.L.S., F.Z.S., president, in the chair.—A communication was read from Mr. M'Lachlan to the effect that, in the writer's opinion, the larva referred to by Prof. Westwood at the last meeting of the society, as boring in the stems of the potato, was in all probability that of a *Noctua-Gortyna flavago*, polyphagous in the stems of a variety of herbaceous plants.—Mr. S. Stevens exhibited some living specimens of *Teretrius picipes*, parasitic on

[1] And potash.

Lyctus oblongus, and also specimens of *Pachnobia alpina*, bred from pupæ found on the highest parts of mountains about Rannoch, N.B.—Mr. Enoch exhibited some remarkable varieties of British lepidoptera.—Mr. Rutherford exhibited some living specimens of an ichneumon (identified by Mr. F. Smith as *Cryphus formosus*), parasitical on the larvæ of a West African moth, allied to *Anapha panda*.—Mr. Rutherford also exhibited a series of colour varieties of the African butterfly, *Aterica meleagris*, as illustrative of the principles of protective assimilation and of some remarks he contributed thereon.—Mr. Jenner Weir exhibited five remarkable specimens of *Argynnis paphia*, and contributed some remarks on melanic variation in that species.—Mr. Wood Mason read a paper on the difference between the form of the antennæ in the males of *Idolomorpha* and other genera of *Empusidæ*, a sub-family of Mantidæ.—Mr. Dunning read a paper on the genus *Acentropus*.—The following papers were also communicated:—Descriptions of several new species of myriopoda of the genera *Sphærotherium* and *Zephronia*, by Mr. Butler; and descriptions of new genera and species of South American *Eumolpidæ*, chiefly from the Amazon region, by Mr. Baly.

GENEVA

Physical and Natural History Society, February 21.—Prof. Marignac having transformed into nitrates the gadolinite earths for the purpose of decomposing them afterwards by heat, obtained, after many successive experiments, products more and more pure, showing the existence of a third earth—terbene. The yellow tint of its oxide does not result from the presence of didymium in this oxide.—Prof. Soret described the principal results of his researches on the ultra-violet absorption spectra. Most of these spectra are continuous up to a given line, from which the radiations are more and more obstructed. The bases and the acids generally carry their absorbent properties into the salts which they compose.

March 21.—Prof. Alph. de Candolle read a memoir on the appearance and the falling of leaves of trees. He was not able to discover any direct and regular connection between the periods of the two phenomena. Among the species seen, individuals present great differences in this respect; we find sometimes that individuals earliest to get their leaves in spring are latest in autumn to lose them, but the exceptions to this rule are numerous. One specimen presenting singularities in this respect preserves, in general, its qualities from year to year (see *Arch. des Sc.*, t. lxii. p. 143).—Prof. Brun spoke of the causes of the movement and of the different modes of reproduction of diatomaceous algæ, which multiply by subdivision and by spores, and which live in the most diversely situated localities in the Sahara, as at altitudes of 2,600 metres in the Alps.

April 4.—Prof. F. A. Forel has studied the sculptured pebbles on the strands of the Lake of Geneva. Some are incised by a larva of the species *Hydropsyche*, others are covered with a tufoid incrustation, underneath which the calcareous stones are deeply sculptured. The incrustation results from the action of two algæ, *Enactis calcivora* and *Hydrocoleum calcilegum*.—M. Victor Fatio presented the report printed by him on the International Congress on Phylloxera at Lausanne, entitled "State of the Phylloxera Question in Europe in 1877."

April 18.—M. Alph. Favre read a note on the mode of formation of some stratified mountains and some valleys, which he explained by the ramming or lateral crushing of the geological strata. He has made experiments tending to prove his theory, by means of caoutchouc stretched out and covered with potter's clay, left to contract gradually. (*Arch. des Sc.*, t. lxii. p. 193.)—Prof. Soret, by means of the observation of the ultra-violet absorption spectra of gadolinite earths, has confirmed the conclusions of M. Marignac and M. Delafontaine on the existence of terbene and of another yellow earth besides terbene and yttria.—M. Arthur Achard indicated a peculiarity in the action exercised by a magnetic pole in a circular closed current. If we imagine the pole approaching nearer and nearer the plane of circumference by projecting beyond the latter, there will be an angular situation for which the component perpendicular to the plane of the current, from the action exercised by the pole on the latter, changes its sign. It follows that two opposite poles, the one on this side, the other on that of the situation thus defined, will exercise on one and the same circular current concordant actions.

PARIS

Academy of Sciences, August 26.—M. Fizeau in the chair.—The following, among other papers, were read:—Employment of the right ascension of the moon, corrected from tabular errors, for determining longitude at sea, by M. Faye. He indicates some modifications by which the errors in Hansen's tables may be corrected.—Comparison between the salivary and the sudoriparous glands, relatively to the way in which they are affected by section of their excito-secretory nerves, by M. Vulpian. Jaborandi still acts on the sub-maxillary gland several days after section of the excito-salivary nerves, whereas this plant, or its alkaloid, pilocarpine, from the sixth day after section of the sciatic nerve (which seems to contain all the excito-sudoral fibres of the posterior limb), has no longer action on the sudoriparous glands of the corresponding limb. M. Vulpian thinks the probable reason for the dissimilarity lies in the enormous quantities of nerve-cells, isolated, or in ganglionic groups, distributed throughout the secretory nerves which go to the sub-maxillary gland. These, after section of the nerves, probably prevent the fibres gradually losing their excitability as far as their peripheric extremities.—On the vibratory forms of solid and liquid bodies (third memoir), by M. Decharme. He finds that on circular plates (thrown into vibration) the breadths of the striæ are inversely proportional to the square roots of the numbers of vibrations of the corresponding sounds.—On pelletierine, an alkali from the bark of the pomegranate, by M. Tanret. Its mode of preparation and its properties are described; also the proportions got from the bark of different parts (most is got from the dry roots). Pelletierine is the tænicide principle of the pomegranate, not previously isolated.—Researches on strychnine, by MM. Gal and Etard. By causing hydrated baryta to act on strychnine under certain conditions, two new bases were obtained: they are called respectively, *dihydrostrychnine* and *trihydrostrychnine*.—Researches on the relations existing between the weights of the bones of the skeleton of a buffalo, by M. de Luca. The entire skeleton weighs about 29 kilogs. The lower jaw weighs a fifth of the cranium; the head (without lower jaw) as much as the vertebral column; the pelvis four times the sacrum; the bones of the head a fourth of the skeleton, the cervical vertebræ, the dorsal, and the lumbar, with sacrum and caudal vertebræ, about equal; the bones of the two anterior limbs double the posterior; the bones of the right side weigh more than the corresponding bones of the left. Of the vertebræ the atlas weighs most; the weight then diminishes on to the last dorsal, then increases and is stationary in the lumbar vertebræ; in the caudal the weight diminishes progressively, &c.

CONTENTS

THURSDAY, SEPTEMBER 12, 1878

OUR NATURAL HISTORY COLLECTIONS

THE question of the government of our Natural History Collections, to which we have more than once called our readers' attention, has been taken up, we are glad to say, by the British Association. It was moved in the Committee of the Biological Section by Dr. Allen Thomson at the Dublin meeting, and carried unanimously, that the attention of the Council of the Association should be called to the fact that in the Act lately passed to enable the Trustees to move the natural history collections to South Kensington, the recommendations made by the Royal Commission on Science as to their future government have been wholly ignored, and that the Council should be requested to take such steps in the matter as they might deem expedient. This resolution having likewise successfully passed the ordeal of the Committee of Recommendations, and having been adopted by the General Committee of the Association, will come in due course before the Council, who will, no doubt, take decided action upon the question. The Royal Commission on Science having been appointed mainly at the instigation of the British Association it is not likely that the Council will suffer some of its most important recommendations to be thrown overboard without remonstrance. Meanwhile we may be allowed to offer a few more observations upon this subject, which, we need hardly say, is of the utmost importance as regards the future progress of biological studies in this country.

The great object to be aimed at is the complete separation of the new museum of natural history, both as to control and as to finances, from the Secretariat of the Trustees in Bloomsbury. If this point can be assured it does not so much matter whether the nominal governorship is vested in the Trustees or in a Minister of State. We may well believe that the main object of the Royal Commission on Science in recommending the latter course was to make sure of a divorce of the new institution from the Secretariat in Bloomsbury, under whose blighting rule the natural history collections have so long languished. It must be recollected that the so-called "Superintendent" of the Natural History Departments in the British Museum has hitherto held merely a titular office, so far as authority goes. He can neither spend a sixpence nor deviate in the slightest degree from existing practice without reference to the "Principal Librarian," who is the sole executive officer of the Trustees, and through whom all communications from the different departments have to pass. What is now necessary in the interests of natural history is that the Trustees should (as a minimum) be required to appoint a different executive officer for the new museum of South Kensington, and to keep its finances altogether separate from those of the British Museum. If this change can be effected there will be no longer any temptation to starve the natural history in order to pamper the library, as is the natural instinct of the "principal librarian," who is always selected from one of the library officials, and is utterly ignorant of the requirements of natural history over which he reigns supreme.

Not only should an independent director be appointed for the new museum at South Kensington, but he should be appointed at once. It may at first sight appear rather extravagant to assign a highly-paid director to a museum before there is anything or any person in it to direct, but in the end this will turn out to be by far the most economical plan. If the appointment of a director be deferred until the fittings of the new building have been finished, and the time has arrived to move the collections, it will be found that there will be all sorts of alterations required which will cost large sums. If the director be appointed at once there will be one person responsible to see that the fittings are such as will be suitable for the arrangements of the new institution, and the architect will have one definite person to consult upon the almost endless details which are necessarily incident upon the conservation and exhibition of such a varied mass of material as composes the National Museum of Natural History. We do not doubt, therefore, that many thousands of pounds would be saved by the immediate appointment of a responsible director. Who that director should be is perhaps a more difficult question. A few years ago the recommendation of the Royal Commission on Science that the present superintendent of the Natural History Departments of the British Museum should become the first director of the New Museum of Natural History, would have been the obvious course to pursue. But time runs apace, and it may be doubted whether the present superintendent would now care to undergo the fatigues and anxieties involved in the arduous task involved in the transfer and re-arrangement of the natural history collections in their new site. But there is another distinguished naturalist less advanced in years and already domiciled in South Kensington, whose appointment to such a post—if he could be persuaded to undertake it—would command universal assent. We need not mention his name—it is in every one's mouth.

One other point in connection with the new museum requires immediate attention. The sapient officials at the British Museum who planned the removal of the natural history collections never appear to have realised the idea that nowadays a library is a necessary appendage to a museum. We are told, indeed, that there has been no space even reserved for a library in the new building at South Kensington. But, as every naturalist knows, the collections would be simply useless in their new site without a scientific library. To execute any scientific work without books would, in these days, be more impossible than the famous, if now somewhat antiquated, task of making bricks without straw. But how is a library to be provided? It is not possible to go into the market and buy such an article off-hand. Many, even of the more modern works on natural history, are out of print, and can only be picked up from second-hand booksellers at long intervals. If, when the trustees determined on the removal of the natural history collections ten years ago, they had set apart 1,000*l.* a year out of the 10,000*l.* which they devote to the purchase of books, to form a special library for the Natural History Departments, the required library would have been now ready. But nothing of the sort was thought of, and as books on natural history are growing scarcer every year in consequence of the

increased demand for them, it must be now *many years* before the necessary library can be provided, even if there is an unlimited grant of money provided for the purpose. In the meanwhile, how is the scientific work at South Kensington to be carried on? In the first place the "Banksian Library," as it is called, which originally came to the British Museum along with the natural history collections, should be transferred along with them bodily to South Kensington. This will provide a good set of the older publications on natural history for the new institution. In the second place for the modern publications, which are of still greater importance, we venture on a suggestion—which will, we fear, make the principal librarian's hair stand on end—namely, that all the works and periodicals habitually used in the four departments of natural history should be temporarily sent on loan to the new institution at South Kensington. They should be returned to the British Museum by degrees so soon as duplicates of them can be obtained by purchase out of a fund to be annually devoted to the purpose. In this way the work at South Kensington might be carried on without interruption, and the National Library at Bloomsbury would at the same time suffer no permanent loss. It would be no doubt an occasional inconvenience to the readers at Bloomsbury to find that some of the books they require for reference are at South Kensington. But this inconvenience will diminish year by year, as the new set of books is purchased, and by this plan alone, as far as we can see, can the whole of the important scientific work performed in the natural history departments be prevented from coming to a standstill. In point of economy also there can be no question of this plan being the best, as the attempt to purchase at once all the books required for the new Natural History Museum would raise the value of them twenty-fold. Convenience and economy are therefore alike on the side of our suggestion, although we fear that it will be bitterly opposed by the principal librarian and his satellites, who object strongly to see the Banksian Library being removed from the hallowed precincts of Bloomsbury.

THE FENLAND

The Fenland, Past and Present. By S. H. Miller and S. B. H. Skertchly. (Wisbeach: Leach and Son. London: Longmans, Green, and Co., 1878.)

THIS book is practically the joint production of several, many chapters being contributed by writers whose names do not appear on the title-page, though given at the head of their respective chapters. The work contains about 650 octavo pages and is therefore cumbrous; it is divided into fifteen chapters, very unequal both in length and merit, which embrace a wide range of subject, including, among others, dissertations on History, Geology, Botany, Zoology, Archæology, Biography, Engineering and Sanitary Problems, &c. The printing done at Wisbeach has not been carefully revised, as shown by a long list of corrections, which however call attention to but a very small proportion of the errors. Many of the illustrations are presented by patrons, and these are fairly good, except the chromo-lithographed frontispiece which is very inferior either in workmanship or drawing.

The book, whatever its excellence may be in other respects, is certainly objectionable in its treatment of those subjects on which discussion is far from closed among men of science. A vein of dogmatic infallibility is particularly apparent in dealing with geological problems. The book, including so wide a scope, is evidently intended to pass into the hands of readers the majority of whom are quite unacquainted with geology, and to bid for support in this manner from the general public, instead of appealing to those who from their own experience would be able to estimate its real value for views and claims not yet recognised by fellow-thinkers and workers, seems unworthy of science. When original work is laid before specialists, the theories built upon it may be stated boldly and decidedly; but in a popular book, intended for the general reader, that which is accepted as fact and that which is still under discussion or not yet argued, should be distinctly separated, and in the latter case the relative value of each kind of evidence should be clearly defined. Too frequently of late the result of others' work has been incorporated by writers and set forth as their own and in a positive manner not claimed by the discoverers themselves. In this case we have not an account of the geology of the Fens, but an exposition of the opinions of Mr. Skertchly and others on geological questions, introduced as undisputed fact. It is questionable whether the readers to whom the book appeals care for or expect individual opinion, but would not rather desire an easy "coach" to the ascertained facts of Fen-geology.

To illustrate, we select first the treatment of Prehistoric Man. We are told that the "Old Stone Folk" —the term is preferred in the book to Palæolithic—are concluded to be related to the living Esquimaux, and that Prof. Boyd Dawkins is of opinion that they are lineal descendants. This inference, based as it is on very slender grounds—is the only foundation for the following positive assertion, made a few pages further on:—"The Old Stone Folk, on the other hand, belong to the Mongoloid class of *Leiotrichi*, of whom the Lapps and Eskimos are modern examples; hence we see that even in the Old Stone age there were no signs of the fusion of the crisp-haired *Ulotrichi* and smooth-haired *Leiotrichi;* and it is from such striking facts that we are justified in ascribing to mankind an antiquity far greater than that of the earliest relics at present known." It is needless to point out that so far from there being any reason to suppose that Palæolithic man was differentiated, the sameness of type—differing only where different material is used—of all the oldest stone implements is evidence against it. Where so much is predicated of the "Old Stone Folk," it is not surprising to find the "Newer Stone Folk" are minutely described even to their complexion and eyes as if they were still a living tribe. They are termed Iberian, which, as explained not to mean a people indigenous to or even coming directly from the Iberian peninsula, is a misleading term, and has no advantage over that of Black Kelt. The Basque people may be descendants of Neolithic man, but Neolithic men were not Basques. To say that they were a Turanian people means nothing more than that they were not Aryan.

The third chapter, by Mr. Miller, is devoted to a historic sketch of the Fenland people from the time of the Kelts

to the reign of Henry III., and is written on naturally surer ground and the interest is better sustained. The stories of the Saxon and Danish conquests are well told according to the most modern versions, and introduce the newest approved spelling of historical names. We can almost follow the exact steps by which the Normans took possession of the Fenland and how they kept it by building fortress dwellings, simply massive round or square towers, which the more civilised Saxon noble would never have made his home. This chapter is illustrated with engravings of coins and of one of the rare British circular bronze shields.

Chapter IV., on Language, and Chapter V., on the Dissolution of Monasteries, are also by Mr. Miller. Chapter VI., by Mr. Skertchly, treats of the attempts made to drain the fens, a subject well worked out by Sedgwick and his scientific assistants. The author's views, although probably correct, are put very decidedly, and in places plentifully sprinkled with notes of exclamation. The following from p. 158 will serve to illustrate the style :—

"How can it be shown that these districts on the same level, with interweaving watercourses and co-equal desiderata, were so distinct that they should be set at variance like a trio of mongrels over a meat biscuit? Yet such has been the disastrous result."

It appears to be Mr. Skertchly's opinion that the one essential to an engineer who undertakes drainage works is to understand "Mr. Tylor's laws" (Mr. Alfred Tylor, F.G.S.). In Chapter VII., a continuation of the last, the writer goes out of his way to object to the *absurdity* of the use of the time-honoured expression, "lands watered by rivers." Yet the term is right, for a land of many rivers is more moist and watered than a land without, and rivers do literally *water* the lands through which they flow. They do it by percolation, overflow, and mist. For instance, not only does the Nile, but rivers all over the world, the Thames itself among them, water their level lands by flood at certain seasons, by mist at night.

The Wash, a subject on which we naturally looked for a good deal of information, is too briefly disposed of in a chapter of only five pages. The next, on Meteorology, is sixty-seven pages long, and bristles with tables which in a popular work would have found a more appropriate place in the Appendix, since their presence in the middle of the work cuts it in two. The botanical sketch by Mr. W. Marshall would have been more welcome had it been longer, and we should have been glad to have seen more of the Fen rarities illustrated. The history of the spread of Anacharis is likely enough to be the correct one, but why is the name of the plant in the illustration *Elodia canadensis*, and *Anacharis alsinastrum* in the text. The Fungi, although not very numerous, have appropriately a section to themselves. It is strange that the writer should speak doubtingly of the occurrence of any fungi in the Carboniferous, since their presence there is now a well known fact. The eleventh chapter treats of the prehistoric fauna of the Fenland, and is so full of errors that it is to be regretted that, as in other instances, a specialist was not intrusted to write it. Space will only permit to notice a few of the inaccuracies. At p. 326, Hipparion is said to be "a horse-like animal with antlers like a stag," and this is the whole description. The table, p. 327, is not a complete list, and we know on the authority of Prof. Boyd Dawkins, that it contains besides, a number of species which have not hitherto been found in the beds to which they are ascribed; it separates *Ursus ferox* and *priscus* which are synonyms, and persists, which is the case throughout the book, in calling Lemmus, "Lemnus." The table at p. 328 is a marvel of careless spellings, none of which are included in the list of corrections at the beginning of the book, which we are "earnestly requested" to make with pen and ink. In another table *Bos brachyceros* is said to be "a variety" of *B. longifrons,* although these are admittedly synonyms.

The limits of a review, however, compel us to pass on at once to the chapter on Geology, with which especial fault is to be found. In the first place, from the nature of the book it is evident, as already intimated, that it is not intended to be specially consulted by geologists, and the fact that a survey memoir on this district, in which Mr. Skertchly was concerned, had already appeared, renders it quite unlikely that it would be. Mr. Skertchly recognises this by prefacing his subject with a perfectly elementary treatise on the science. Instead of this circumstance inducing him to guard his statements with more than ordinary care, he absolutely revels in the opportunity of airing his infallibility, as if without fear of contradiction. The theories of those whom he mentions as friends are everywhere brought in, those of his opponents mostly ignored. Thus Evans's "Ancient Stone Implements of Great Britain," a work in which implements from the Fenlands have been described, is not even alluded to, although the author appears to have made use of it. General readers should in fairness have been cautioned that Croll's theory is not supported by geological evidence, thousands and thousands of feet of consecutively deposited strata showing no trace of cold periods, much less of glaciation; that Geikie's theory of an ice sheet is not generally accepted by the Geological Society, as even this session's discussions show; that Tylor's Pluvial periods have but few adherents. By the way, the Pluvial period is here ingeniously reduced to local showers produced by the evaporation of melting snow and ice, although Mr. Tylor himself disclaims for it all connection with ice action, and claims on the contrary that it was of great intensity and long duration. Mr. Skertchly is so fully impressed with the correctness of the view he happens to take of things that he announces that his alleged discoveries have made the Brandon Beds of "surpassing interest" (a favourite term with him), "for ever setting at rest the question of whether man did or did not exist during the great cycle of the glacial period." This climax is worked up to by pages of *ex parte* reasoning which non-geological readers are not in a position to follow. Considering that this evidence has not yet been brought forward in any scientific publication, and that his repeated promises to bring it before the Geological Society have not yet been kept; that Professors Hughes and Bonney purposely went over part of the ground with him and have publicly thrown grave doubts on the value of the evidence; that Professors Prestwich, Boyd Dawkins, Mr. Evans and others do not admit its value, and that at the Conference held last summer on the Antiquity of Man, the weight of evidence was rather against his interglacial age in England,—it is little less than wantonness, whether

the evidence, only known to himself, is or is not conclusive to him, to introduce it as undisputed fact in this manner in the present publication. After this we have not enough interest to read the remainder of the book, and besides it is so full of mistakes, as Urus for Ursus (p. 505), shorter for longer (p. 511), &c., that it is a wearying effort to understand in places what the author really means.

J. S. G.

AMERICAN GEOLOGICAL SURVEYS

Geological and Geographical Atlas of Colorado and Portions of Adjacent Territory. By F. V. Hayden, U.S., Geologist in Charge. (Washington: Published by the Department of the Interior, 1877.)

IN the magnificent Atlas just issued by the Department of the Interior we have the consummation and crown of all the labours which Dr. Hayden and his staff have carried on so triumphantly for the last five years, and of which they have already given us so much interesting and important information in a series of Annual Reports. Before examining the work from a scientific point of view, no reader can refrain from expressing his admiration of the style in which the Atlas has been produced by the United States Government. As a specimen of cartography, typography, and lithography, it is altogether worthy of the highest praise. For beauty and indeed sumptuousness of execution, it may be classed with those *livres de luxe* which from time to time have been issued from the National Imprimerie of France.

The Atlas consists of two series of maps, the one of a general, the other of a detailed kind. The first series, on the scale of twelve miles to one inch, comprises four sheets, each embracing the whole State of Colorado and part of the neighbouring territory. The first of these illustrates the system of triangulation adopted in the survey; the second shows the drainage system of the area; the third by a simple and clear arrangement of colours, exhibits at a glance the economic features of the whole region—the agricultural land, pasturage, forests, and woodlands, sage and bad lands, mineral tracts, and the portions rising above the limit of timber-growth; the fourth contains a condensed and generalised geological map of the same territory. Nothing can surpass the lucidity of expression and artistic finish of these maps.

The second series—twelve in number—is on the scale of four miles to one inch, and consists of six topographical sheets and six identical sheets, coloured geologically. The topographical details, though numerous, are so selected as not to neutralise each other, or mar the broad, clear picture which the maps were designed to be. By means of contour-lines of 200 feet vertical distance, the surface-configuration of the whole region is depicted as in a model. We can follow the lines of the broad valleys, of the deep, narrow cañons, and of the hundreds of minor tributaries which have scarped out their courses on either side. Here we look down upon a vast table-land, deeply trenched by stream-channels; there upon a succession of bold escarpments or mesas which bound the table-land and hem in the neighbouring valley. Huge mountain-ranges rising out of the plateaus are so vividly drawn that they seem to stand out of the paper. Yet no shading is employed. All the effects of inequality are produced by contour-lines, so faithfully set down that a single line may be tracked in its sinuous course along the whole of a mountain front until it comes out upon the table-land beyond. When will our map-makers learn to use this, the only true method for expressing the surface of a country? The best of our atlases are disfigured by strips of shading running across the map like so many caterpillars, to represent mountain-ranges. Even our Ordnance maps, so admirable in most respects, are sometimes so loaded with shading, that a steep hill-side only a few hundred feet high is made as black as our highest mountains, and the topographical names can hardly be read, even with a magnifying-glass.

But, above all, welcome are these six geological maps. In the previously published maps and charts accompanying the Annual Reports, only small detached areas were represented, and even from the careful descriptions of the various geologists of the staff it was hardly possible to frame a satisfactory conception of the geology of Colorado as a whole. Ever since the marvels of its deep gorges and vividly painted cliffs were made known, that region has possessed a high interest to the geologist. He has now the means of gratifying his desire for further knowledge. With the help of these maps and the two accompanying sheets of sections he can realise most satisfactorily every great feature of Colorado geology. The ancient Archæan ridge—the nucleus or back-bone of the American continent—may be traced running north and south nearly along the present hydrographical axis of the country. Flanking that ridge comes a series of palæozoic deposits, the oldest of which have been identified palæontologically with Silurian formations. Rocks, regarded as of Devonian age, overlap the Silurian beds, and repose against the ancient crystalline ridge on the south-west side of the San Juan Mountains. They are soon buried under later accumulations, and they seem to be of but local development, since in most places where the rocks are found in juxtaposition, the Silurian are directly succeeded by Carboniferous strata. These last-named rocks cover large tracts of country, running as bands round the Archæan area, and lying in basins across it. Far to the west where the Grand River has so deeply trenched the Utah plateau, the flat Carboniferous beds appear from under the brilliant red Triassic strata. The difficulty of drawing any line between Triassic and Jurassic formations in that region is again acknowledged on these maps, the lower red series being doubtfully assigned to the older, and the upper variegated deposits to the later system. Cretaceous rocks are abundantly developed, and cover a vast extent of territory. In particular they spread over the wide plateaux between the San Juan and Gunnisen rivers, and form the platform on which the enormous volcanic outbursts have been piled up from the West Elk Mountains southward into New Mexico. It is more easy to trace on these maps, too, the area respectively occupied by the Laramie, Wahsatch, Green River, Bridger, and Uintah formations which represent post-cretaceous and tertiary times. Glacier moraines, lake-deposits, drifts, sand-dunes, and recent alluvia, all find adequate expression on the maps. Especial care, too, seems to have

been bestowed upon the eruptive rocks which form so important and interesting a feature of Colorado geology. The more characteristic varieties are represented by distinct shades of crimson or orange, and they have been mapped in such a way as to convey at a glance, and even without the aid of sections, a tolerably clear notion of the volcanic phenomena of the region. On the one hand we see the great lava-sheets capping the mesas and spreading far over the plateaux, on the other we notice the great centres of volcanic activity, with their abundant flows, dykes, and breccias.

Two sheets of sections, drawn across all the more interesting and important portions of the geology, complete the vast fund of information given by the maps; while, that nothing may be wanting to enable readers to realise what has been done by the Survey, and the conditions under which it has been accomplished, two large sheets of sketches are given, which most vividly represent the forms of the mountains, plateaux, mesas, and river-channels, as seen from various commanding heights.

Dr. Hayden, with whose personal supervision this great work has been accomplished, has increased tenfold the obligations under which he has laid geologists all over the world for the number and value of his contributions to geology. He now furnishes us with new light whereby to read his former researches and those of his able colleagues. May we venture to hope that he may find leisure to confer yet one further benefit before the progress of his Survey plunges him into a new whirl of work? If he could be prevailed upon to sketch out a plan for digesting the materials of his published Annual Reports, he could doubtless find among his staff some competent writer who, under his guidance, could produce a well-arranged systematic guide-book or text-book to complete the value of the work of his Survey. Such a book of reference as would give a reader who has never had access to the Annual Reports a clear and comprehensive view of Colorado geology, would be of very great service.

These remarks may be fitly closed with an expression of the warmest admiration of the liberal spirit in which the United States Government has conducted these Surveys of the Territories and has published their results. This costly atlas has been distributed gratuitously all over Europe. That this is a wise policy cannot be doubted. Whether actuated or not by a desire to diffuse scientific information, the authorities at Washington do well to make as widely known as possible the geological structure and economic resources of their country. They cast their bread upon the waters, and the harvest comes to them in the form of eager, active emigrants from all parts of Europe.

ARCH. GEIKIE

OUR BOOK SHELF

Forest Flora of British Burma. By S. Kurz, Curator of the Herbarium, Royal Botanical Gardens, Calcutta. (Calcutta: Office of the Superintendent of Government Printing, 1877.)

BY the completion of the work whose title is given above, we have the third valuable contribution to a knowledge of the rich vegetation of our Indian forests. In all three works, namely, Col. Beddome's "Flora Sylvatica of Southern India," Brandis's and Stewart's "Forest Flora of North-West and Central India," and the book now before us, there is much in common, and the plans of the two latter are very similar. There is, however, one great difference between Beddome's and Brandis's Floras and the present issue; while the first two are most profusely illustrated, the work under consideration is entirely without plates. This, perhaps, is not to be regretted considering that the work in its present form constitutes two good-sized volumes; and further than this, Indian plants have of late been very well represented, notably in the two forest floras just referred to. Another distinction, and perhaps one more affecting foresters generally, for whose benefit these floras are ostensibly prepared, is the meagre information regarding the uses of the plants mentioned. Mr. Kurz excuses himself for reducing this portion of his work to a minimum, and refers to Brandis's "Forest Flora" for information on this head. We regret that Mr. Kurz did not see his way to greater condensation in his descriptions, and, if need be, the use of smaller type, so as to reduce the bulk of the book. At the same time its efficiency would have been much increased had he followed Dr. Brandis in giving extended notes as to the uses, for to no similar work can we point with so much satisfaction in this respect as to that of Dr. Brandis.

Regarding the nomenclature of genera and species, it is a pity that some kind of uniformity should not prevail amongst the different authors. Many forest officers would, to say the least, be somewhat confused as to the use of a proper name when he finds in two books published by authority and appearing within a year or two of each other a different generic distinction for the same plant; thus Brandis keeps up the rubiaceous genus *Adina*, and figures *A. cordifolia* of Hook. fil. and Benth., placing *Nauclea cordifolia*, Roxb., as a synonym. Kurz, on the contrary, retains *Nauclea* as a genus, sinking under it *Adina cordifolia*, which is spelt *Andina*, and attributed to Roxb. On this subject of nomenclature, however, Mr. Kurz says: "I confess myself an admirer of, and adherent to, the botanical laws as laid down by the International Botanical Congress at Paris in the year 1867, and published by Prof. Alph. de Candolle. These are translated into nearly all modern languages, and are now generally adopted in Europe, except at Kew. However, I have deviated in several cases in favour of Hooker's 'Indian Flora,' or kept up old-established names, not because I assent to such irregularities, but simply because I thought it not fair that I, a German, should introduce my individual convictions into a practical work written solely for the use of English people."

Notwithstanding the remarks which we have been obliged to make, Mr. Kurz's Flora is one of very great value, and, taken in conjunction with those we have before referred to, forms a pretty complete forest flora of British India. We are reminded by the passing of this work through our hands of the loss Indian botany has sustained by the lamented death of its author.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

American Storm Warnings

THE author of the papers on the American Storm Warnings (NATURE, vol. xviii. pp. 4, 31, 61) seems well acquainted with the storms and storm-warnings of America, and at least with some of the results arrived at in Europe, and if he had confined himself to what he really knew, and to the description of the means

which are used by the *Herald* Office (with which Mr. Collins seems to be connected) to issue storm-warnings from the United States to Europe, no objection could be made against him. But Mr. Collins is more ambitious, and makes some assertions which run against the most authenticated facts known to meteorology, and others which may be true, but ought yet to be proved, while Mr. Collins, without any proof whatever, seems to consider them quite well established.

I must first object to the absence of distinction between the seasons, which is so important a feature in storms, especially in lower latitudes. Mr. Collins seems not to know that the West India hurricanes and other destructive tropical storms are frequent only at certain seasons. This is quite enough to dispose of the author's assertion "that the conditions which combine to develop nearly all areas of low pressure are of equatorial origin." The most violent storms of Europe and the United States happen in the colder months of the year, when there are no storms in the tropical belt north of the equator (very few exceptions are known); besides the use of the word "equatorial" must be objected to as, so far as I know, no cyclone has ever originated between 5° N.L. and 5° S.L., at least,[1] so that we may call the storms of the West Indies, the South Indian Ocean, about the Mascarenes, of the Bay of Bengal, &c., *tropical storms*—because they certainly originate in the tropical belt—but certainly not equatorial. So far as Europe is concerned, there are some few cases in which West India hurricanes have reached it, but this is confined to the months of July to October. At the same time of the year it is not impossible that cyclones originating in the tropical belt of the Pacific may strike the Pacific coast of the United States. As to the storms mentioned by Mr. Collins, which strike the west coast of Mexico, pass over the plateau, and thence into Southern Texas, I very much doubt their existence. In any case no storm of this kind has ever been followed on this route, and so Mr. Collins ought to be rather careful in speaking of them. So far as I know, from books published about Mexico, and from personal information, no storms are experienced on the Mexican plateau.

The same absolute want of facts and general improbability can be urged against the storms which Mr. Collins takes from the Asiatic continent to the Pacific and thence to the American continent. Here the distinction of the seasons is especially necessary, as all Eastern Asia is under the influence of monsoons or periodical winds.[2] In winter, when pressure is so enormously high in the interior of Eastern Siberia,[3] and the winds are north-west and north on the coast, that is, bring the cold dry air of the interior towards the Pacific Ocean, these conditions are favourable neither to local depressions nor to the propagation of European storms, which generally die out in Eastern Russia or Western Siberia. In summer the pressure is low in the interior of Asia, and air is constantly drawn from the Pacific Ocean to supply the deficiency towards the end of the rainy season or summer monsoon—in August to October is the time of the typhoons, that is, of the cyclones of the China Seas; but they do not originate on the Asiatic continent, and only strike it on a very limited area, that is, the coast of Southern China. These typhoons may perhaps reach California, as the West India hurricanes reach Europe, but it is not yet proved that this has ever been the case.

I admit that in autumn, that is, September and October, storms may perhaps pass from the Asiatic continent to the Pacific, and thence to America; but in latitudes far to the north of those visited by the typhoons. At Yakutsk, in North-East Siberia, the prevailing winds of that season are west and south-west, the amount of cloud great, and rains frequent, if not abundant, while the temperature is generally above freezing-point to the middle of October. I consider it possible that Atlantic (European) storms may, at this season, travel over the whole of Northern Siberia and reach the Pacific. In winter this is impossible, on account of the low temperature and high pressure then existing in Siberia.

I resume a few facts either well authenticated or very probable about storm-centres (cyclones) of the northern hemisphere.

1. By far the most of them originate in the middle latitudes (35°–65° N.) in Europe, North America, the Atlantic, and Pacific. As to the three first-named regions it is abundantly proved by the observations we have already. As to the Pacific, we want the direct proof, because observations are too few. But nobody will doubt that, in conditions of climate so analogous to those of the Atlantic cyclones do also originate.

2. Cyclones are of much rarer occurrence in Asia, except the great summer depression, which is of a different nature, and remains the whole summer over the dryest parts of the continent.

3. Tropical cyclones are confined to a few months of the year, and even these seldom reach the latitudes north of 35° N.

Lastly, a few words about Mr. Bennett's storm-warnings. I do not doubt that some storms may reach Europe from America. But it is not at all certain that every storm that has passed from the eastern coast of America should reach Europe. This is the first difficulty in storm-warnings from America. The other is, that neither the path the storm will take nor its rate of progress can be known with certainty. Every one who has examined European and American synoptical maps will have noticed how different the paths of the centres are. So long as the storm can be followed on land, by means of numerous stations, a great approximation to certainty in predicting it is possible, as the durations are caused by certain pre-existing states of pressure, temperature, humidity, &c. But how is this to be done on the ocean?

Meteorologists of great ability, especially Prof. Buys Ballot, have often advocated telegraph lines to the Azores and Iceland, so that these islands might serve as advanced guards to predict storms in Europe. At such a distance as they are from our continent they certainly could serve this purpose, as is clearly shown by the French Atlas Météorologique and Hoffmeyer's synoptical maps. As to American predictions for Europe, I must confess that most European meteorologists are very doubtful about it. It is to be noticed also that, as storms are very frequent in western Europe, and as the rate of progress of storm-centres over the Atlantic is not accurately known, there may be a seeming success in American predictions which the facts, when accurately known, would not justify.

This is not meant to cast a shade on the spirit of enterprise of Mr. Bennett in organising the *Herald* weather predictions. The observations thus collected, or saved from oblivion, will certainly be useful, even if it be proved that storm-warnings from America are not reliable. A. WOEIKOF

St. Petersburg

[1] It would be too long to state why there are no equatorial cyclones. I would advise Mr. Collins to consult "Études sur les Mouvements de l'Atmosphère," by Guldberg and Mohn.

[2] See "Winds of the Globe," by Coffin.—Smithsonian Contrib. vol. xx.

[3] See *Petermann's Mittheilungen*, July, 1878, p. 259, and the short notice in NATURE, vol. xviii. p. 288.

A White Grouse

WHEN shooting, yesterday, on the moors near Dunrobin, I fired at an ordinary grouse and killed it; just as it fell, another bird rose that seemed to be a ptarmigan, from the complete whiteness of its plumage; a third bird then rose, and was shot. The three were picked up not far from each other, and were all very fine birds. It seemed strange that a ptarmigan should be so low; we were not very high above the sea, and far below the elevation affected by these birds. On examining it, it proved to be a very fine grouse, snowy white, with a few dark feathers in the tail and wings. It was not an albino; I think the eyes were dark. It is a very beautiful bird, has been sent off to Inverness to be stuffed, and will be preserved in the Dunrobin Museum.

No one here had seen a specimen of the white grouse before, and it excited considerable interest. No doubt it is only an accident, and its progeny, if it had any, would have been the ordinary grouse.

It may be less rare than I suppose, but you may deem its occurrence worthy notice in NATURE. J. FAYRER

Dunrobin Castle, Sutherland, September 8

Brehm's "Thierleben"

IN last week's NATURE you have copied a drawing, "cobra charming," from Brehm's "Thierleben," presumably for its excellence. Permit me, however, to point out a most serious defect in its truthfulness—the relative proportions of the snakes to the charmers.

Take the youth blowing the horn to be 4 feet 6 inches in height (he could not be much less), the hoods of the cobras must be 8 to 9 inches across. Now I will venture to say that a hood of $4\frac{1}{2}$ inches across has never yet been measured, in a live specimen at least.

I cannot now lay my hands on a cobra skin I have, and give

exact measurements, which I am sorry for, as the cobra in question measured 6 feet 3 inches in length, a size Col. R. Beddome—no mean authority—assured me is seldom or never surpassed.

In a work such as Dr. Brehm's, exaggerations in illustrations should be as carefully avoided as misstatements in letterpress. A Natural History that depicted horses the size of elephants would be scoffed at, yet, strange to say, equally glaring absurdities, such as "cobra charming," frequently pass muster.

Bath, September 10 E. H. PRINGLE

The Sea-Serpent Explained

IF you have space for the following, it is so confirmatory of Dr. Drew's experience of an opera-glass dispelling "fond deceits" concerning a sea-serpent, that it may be worth recording.

One morning in October, 1869, I was standing amid a small group of passengers on the deck of the ill-fated P. and O. ss. *Rangoon*, then steaming up the Straits of Malacca to Singapore. We were just within sight of the coast of Malacca, and quite out of sight, so far as I remember, of Sumatra. One of the party suddenly pointed out an object on the port bow, perhaps half a mile off, and drew from us the simultaneous exclamation of "The sea-serpent!" And there it was, to the naked eye, a genuine serpent, speeding through the sea, with its head raised on a slender curved neck, now almost buried in the water, and anon reared just above its surface. There was the mane, and there were the well-known undulating coils stretching yards behind.

But for an opera-glass, probably all our party on board the *Rangoon* would have been personal witnesses to the existence of a great sea-serpent, but, alas for romance! one glance through the lenses and the reptile was resolved into a bamboo, root upwards, anchored in some manner to the bottom—a "snag," in fact. Swayed up and down by the rapid current, a series of waves undulated beyond it, bearing on their crests dark-coloured weeds or grass that had been caught by the bamboo stem.

Ignorance of the shallowness of the straits so far from land, and of the swiftness of the current, no doubt led us to our first hasty conclusion, but the story, with Dr. Drew's, shows how prone the human mind is to accept the marvellous, and how careful we should be in forming judgments even on the evidence of our senses. E. H. PRINGLE

Bath, September 10

DR. DREW'S letter in NATURE, vol. xviii. p. 489, recalls to my mind a similar phenomenon witnessed by myself and a friend on August 3, while crossing from Grimsby to Rotterdam. It was towards evening, when, looking ahead, we saw, about a mile distant, what appeared to be a long, low, black hull, without masts or funnel, moving through the water at enormous speed. After a minute or two it undulated and rose from the surface, and we saw that it was a flight of birds.

The deception was so complete that I can well believe that at least *many* of the stories of the sea-serpent have so originated, though I doubt whether *all* can be explained in this manner.

Grammar School, Bradford, September 7 C. BIRD

THE communication of Dr. Joseph Drew in your issue of yesterday regarding the serpentine appearance of a flock of shags in the English Channel is extremely interesting even as a mere fact regarding the habits of these birds. Will you kindly permit me, however, to point out that Dr. Drew's statement cannot be regarded as explanatory of the sea-serpent's personality? At the most the incident only explains one of a number of *serpentine appearances* of which porpoises and sunfishes swimming in line, pieces of wood with trains of sea-weed, &c., are also good examples. There have been placed on record numerous incidents of serpentine forms having been closely inspected (as in the well-known case of the *Dædalus*, or later still of H.M.S. *Osborne*) where the hypothesis of the serpentine appearances assumed by flocks of birds or fishes could not be held as explanatory in any sense. It is with the view of showing that the exact personality of the "sea-serpent" cannot be accounted for by such an incident as Dr. Drew relates, that I venture to pen these remarks; and as a firm believer from the standpoint of zoology that the large development of the marine ophidians of warm seas offers the true explanation of the "sea-serpent" mystery, I would also ask your readers to distinguish carefully between cases in which serpentine appearances have been assumed by ordinary animals, and those in which *one* animal form has presented itself in the guise of the "great unknown." I am far from contending that a sea-snake developed in the ratio of a giant "cuttle-fish," presents the only solution of this interesting problem. A long tape-fish, or even a basking shark of huge dimensions, might do duty in the eyes of non-zoological observers for a "sea-serpent." The following cutting from the *Scotsman* of September 6, indeed, seems explicable only on the tape-fish theory which I have advocated with the persistence of firm belief within the past few years. At the same time zoologists cannot but feel indebted to Dr. Drew, and to those who, like that gentleman, note unwonted appearances in ordinary animal life, and communicate such incidents to your columns. ANDREW WILSON

Edinburgh School of Medicine, September 6

The following is the extract alluded to:—

"A BABY SEA-SERPENT.—From Van Diemen's Land comes news of the capture of a queer fish. It is fourteen feet long, fifteen inches deep from the neck to the belly, tapering two inches to the tail, and eight inches in diameter in the thickest place. There are no scales, but the skin is like polished silver, with eighteen dark lines and rows of spots running from the head to the tail each side. There is a mane on the neck twenty inches long, and continues from the head to the tail; small head, no teeth, protrusive mouth, capable of being extended four inches like a sucker; eyes flat about the size of a half-crown, and like silver, with black pupils. There are two feelers under the chin, thirty-two inches long. The fish was alive when captured."

Alpine Flowers

IN the Alps I have found some instances of *different forms of flowers in plants of the same species*, which, as far as I know, have been hitherto undescribed, and of which, therefore, I will give a short notice here.

Geranium sylvaticum is in one locality near the Albula Pass *gynodiœcious*, with large-flowered hermaphrodite, and small-flowered female stems. *Veratrum album*, *Dryas octopetala*, and *Geum reptans*, are in all the localities where I have examined them, *androdiœcious*. *Astrantia minor* offers a quite peculiar sort of androdiœcism, some stems bearing, as in other Umbelliferæ, in the same umbel hermaphrodite flowers and male ones, other stems producing solely male flowers. *Dianthus superbus* seems at first sight to exist in three forms: (1) stems with hermaphrodite flowers, being perfectly proterandrous and producing a moderate quantity of *whitish* pollen; (2) stems with female flowers containing very conspicuous rudiments of stamens but pollenless anthers; (3) stems with pistils remaining imperfectly developed and with anthers containing abundance of a *brown* powder. At first sight I thought their flowers to be male, and the brown powder to be pollen grains, but under the microscope the latter proved to consist of grains, the diameter of which is only about one-eighth of that of the pollen grains of the hermaphrodite flowers. I suppose, therefore, these grains to be the spores of some species of fungus, and *Dianthus superbus* to be *gynodiœcious*.

Berninahaus, August 29 HERMANN MÜLLER

The Microphone

WHILE studying the relation between the battery power and the sounds heard through the microphone, I found, when the latter was included in the circuit *between* two pairs of elements, that the sound first amplified by the microphone underwent further amplification by the action of the second pair of elements, and when heard through the telephone the volume of sound was considerably augmented. This new fact may perhaps open up a fresh avenue of research and lead to further development of Prof. Hughes' beautiful discovery.

Hull THOMAS ROWNEY

A Meteor

WHILE directing a small telescope towards Jupiter, at 9.35 P.M. on the 2nd inst., my attention was attracted by the bright light of what proved to be a large meteor, falling towards the south-western horizon. Its apparent size was two or three times that

of Jupiter, its colour being green (very similar to that of burning silver), suddenly changing to a dull red on falling to pieces at the end of its course.

The meteor appeared at about R.A. 18h. 10m., Decl. S. 10°, and travelled slowly till it broke up in R.A. 17h. 0m., Decl. S. 16° (or nearly coinciding with the star η *Ophiuchi*), being visible for about three seconds. SYDNEY EVERSHED

Wonersh, Guildford, September 4

OUR ASTRONOMICAL COLUMN

THE INTRA-MERCURIAL PLANET.—The correction applied by Prof. Watson to his first position of the supposed intra-Mercurial planet from more complete reduction of his observation brings the object somewhat nearer to the ecliptic, the longitude being 124° 46′ and the latitude 0° 51′ S., at 10h. 24m. 49s. G.M.T. on July 29. Close and continued search along the ecliptic with large refractors, provided with long "dew-caps" blackened inside, for 10° or 12° on each side of the sun, may now afford the best chance of recovering the planet previous to the next total solar eclipse, or probably until the eclipse in May, 1882; for it will be seen that in the eclipse of January, 1880, the duration of totality is short, and, which is of still more consequence, the central line runs mainly amongst the Pacific Islands without touching any, where observations would be likely to be very practicable. With respect to the precautions that may be usefully adopted in a search for the planet, the reader may consult the remarks of Prof. Julius Schmidt in *Astron. Nach.*, 878, bearing upon his daylight observations of the great comet of Klinkerfues in 1853.

VARIABLE STARS.—Prof. Schmidt notes the variability of the star Lalande 46090, rated 6m. in the catalogues of Argelander and Heis, but which was invisible without the telescope on July 9 and 10, 1878. It is 7m. in Bessel, and only 8·9 in Lamont. The position for 1880 is in R.A. 23h. 26m. 12s., N.P.D. 101° 39′·6.

The Athens observations, last May and June, establish a material increase in the period of the well-known variable δ Libræ. The error of Schönfeld's ephemeris in May, 1877, was—32m.; in the present year it was—48m., from the naked-eye observations, considered preferable to those made with the comet-seeker. χ Cygni was at a maximum (hardly 5m.) on March 14, 1878, 408·6 days having elapsed since the preceding maximum. Prof. Schmidt refers to a star in the nebula of Orion, No. 822, of Bond, which exhibited marked change of brightness in April last, having been seen as faint as 12 8 and as bright as 9·7. This star follows θ, 34·3s., and is south of it 5″·1. In previous years he had not noticed any sensible difference between No. 822 and No. 784, which precedes 8·5s. and 0″·3 to the north.

Mr. Tebbutt writes from Windsor, N.S.W., with recence to our notice of his newly-discovered variable in Ara, that in his communication to the *Astron. Nach.* the field of view should have been given as about half, instead of three-fourths of a degree. Although Brisbane 6,142, is still visible as a star of the eighth magnitude, 6,183 of the same catalogue cannot now be found; but an error of two minutes in the observed R.A. will identify it with 6,196, exactly on the same declination.—Prof. Bickerton, of Canterbury, N.Z., sends us an outline of a lecture delivered by him at the Philosophical Institute of this place, on the explanation of the phenomena of variable stars. Amateurs in such latitudes may do excellent service, with comparatively little expense of time, and with very small optical means, in furthering our knowledge of the variable stars of the southern heavens, providing themselves for the present with Behrmann's Atlas, the magnitudes in which are recorded from recent observations; while at no distant time it may be expected the charts of the Cordoba *Uranometria*, prepared under much greater advantages, will be available to them. The popular atlases are useless in such investigation. We hope the result of attention being directed to subjects of this nature at the Antipodes may be to greatly increase the number of observers in departments of practical astronomy, peculiarly within the scope of the amateur, as, compared with what is now to be effected with limited means and appliances in the northern hemisphere, a promising field is open to them.

SWIFT'S COMET OF JULY 7.—Elements of the comet discovered by Mr. Lewis Swift, of Rochester, N.Y., on the morning of July 7, have been calculated by Dr. Holetschek, from observations to July 23, made at Clinton by Prof. Peters; they do not resemble those of any comet previously computed. The failure to observe this object in Europe, though it was closely sought after at several observatories, appears to have arisen from an error in the telegram, which stated its motion to be slow, whereas it was pretty rapid towards the south-west horizon. It might have been well observed during June, the position at the beginning of the month being in the vicinity of ψ Cygni, and on June 21 just south of 13 Lyræ. The elements are in *Astron. Nach.*, No. 2,213.

THE SATURNIAN SATELLITE MIMAS.—The series of observations of the satellites of Saturn, in 1877, made with the great refractor at Washington, includes but few of the closest satellite *Mimas*, which, indeed, has not been frequently observed of late years. A combination of the later Washington measures, with observations made by Mr. Lassell in 1847, and a few intermediate ones, indicates, on the assumption of a circular orbit in the plane of the ring, a period of 22h. 37m. 6·82s.; but there appear grounds for suspecting a very slow increase in the length of the period. Mädler, from his reduction of the observations of Sir W. Herschel, considered a very sensible eccentricity was shown by them, which more recent observations support. In the case of *Hyperion*, the perturbations as indicated by the measures must be very large, but we believe Mr. Marth has found the same result some time since from his investigations on the theory of the satellites, the chief disturber being, of course, *Titan*.

GEOGRAPHICAL NOTES

WE have received three new maps of Cyprus. The first, to which we have already briefly referred, is by the well-known German geographer, Dr. Kiepert, and is a valuable original compilation based upon the English Admiralty chart, but containing many more names of villages, &c., than any other modern map. A noticeable point of this map is the hill-work, which has been very artistically rendered. Stanford's Cyprus is also an original work, with some useful features not found in Kiepert's. The Kaimakamliks and Kazas are all distinctly bounded and named, and although there are not so many names as in Kiepert's map, yet those that find a place appear to have been judiciously selected. The spaces around the map are utilised by the insertion of supplementary maps presenting the agricultural and geological conditions, a section showing the interdependence of these conditions and plans of the chief coast towns and roadsteads. Wyld's Cyprus is on exactly the same scale as Mas Latrie and the Admiralty chart, and appears to be a combination of these two. We notice in one or two places where the positions of villages do not agree in these two authorities, that both positions are inserted on Wyld's map, as though there were two places of the same name. The hills on this map are inferior to Kiepert's; there are few English lithographers that can equal the German in this art. This map, like Stanford's, introduces agricultural and geological insets, and plans of the chief roadsteads and coast towns. We need not

be surprised at the alleged unhealthiness of Cyprus, when Mr. Wyld tells us that the average temperature in February is 52·8° *Centigrade*, and that of July and August 82° of the same scale!

LIEUT. KITCHENER, R.E., has handed over to the Committee of the Palestine Exploration Fund the whole of the memoirs, special plans, and lists connected with the great map of Western Palestine. These materials, now in the hands of the Committee, consist of a map in twenty-six sheets, on the scale of one inch to a mile; a map in three sheets, on the scale of three-eighths of the large map; and an immense collection of memoirs from the note-books of Lieut. Conder and himself. The Committee have already taken steps for the publication of the maps, and will at once proceed to consider that of the memoirs, a part of the work as important as the map. Lieut. Kitchener exchanges the work of the Palestine Fund for the important charge of the survey of Cyprus, to which he has been appointed by the Foreign Office. He achieved in Palestine what may be called the unparalleled feat in survey work of surveying 1,000 square miles for 1,000*l.*, and in eight months.

WE learn from Washington that Capt. Tyson is expected in America with the *Florence* in which last year he made a preliminary trip, with the view of establishing a polar colony at Lady Franklin's Sound. The scheme, which has been devised by Capt. Howgate, of the Signal Service, has not been given up, but postponed for one year, Congress having terminated its session without any resolution having been taken on the necessary grant of credit. It will be proposed again when the Congress meets in 1879, and the report drawn up this year by the special commission will receive an additional force from observations taken by Capt. Tyson and his able scientific staff.

THE *Geographical Magazine* for September describes the equipment of two expeditions from the United States for the survey of the Amazon. One of these is in the *Enterprise*, under Commander Selfridge, of the U.S. Navy, which will survey the river as far as Manaos, and the Madeira as far as San Antonio, the point of departure of the line of railway around the Falls of Madeira. The other expedition is sent out by Messrs. Mackie and Scott, of Philadelphia, its object being to arrange a route by way of the large rivers which connect Bolivia and Brazil, over which trade can be carried on. As a preliminary measure a surveying party will go to Bolivia to study the country, and will be accompanied by a naturalist, Mr. Ernest Morris, who has already done good work on the Lower Amazon.

THE just published June number of the *Bulletin* of the French Geographical Society contains M. de Ujfalvy's account of his official journey to Zarafshan, Ferganah, and Kuldja, which contains a good many original ethnological observations. Dr. Hamy has a paper on Manoel Godinho de Eredia, the Portuguese whom Mr. Major and others had accepted as the earliest discoverer of Australia; fuller evidence, however, convinced Mr. Major that Godinho had no claim to this honour, and Dr. Hamy endeavours to show what were the real services rendered by this "Descobrider" to geography. The number contains the letter from Savorgnan De Brazza, describing his journey on the Ogové, to which we alluded some time since.

FROM the *Bollettino* of the Italian Geographical Society we learn that Romola Gessi had been furnished with a formidable equipment for the exploration of the Sobat, by Gordon Pasha, who, at the last moment, was compelled to stop the expedition, on account of a formidable rebellion in Darfur.

M. PAUL SOLEILLET, who, it will be remembered, was to cross Africa from Senegambia to Algeria, reached Kuniakaro on June 23 by way of Bakel. This was 1,250 kilometres beyond St. Louis, and thitherto M. Soleillet had few difficulties. After Kuniakaro, however, the real work of the expedition will commence, and not a few dangers will have to be faced. His next point was Yamina, a small town on the banks of the Joliba, about 50 kilometres from Segou.

LETTERS have been received in Holland from the members of the Dutch North Polar Expedition, and their contents are said to be highly interesting. The expedition, after leaving Bergen, had proceeded to Jan Mayen, where they arrived on May 9. On June 27 they reached Amsterdam Island, where a simple monument was erected in memory of the Dutch sailors buried there. The expedition then visited the other principal points of Spitzbergen, and eventually sailed for Vardö, on the north coast of Norway. It was then their intention to cross the Barentz Sea in order to reach Novaya Zemlya in the middle of August. The letters state further that numerous scientific observations have been made.

DR. GERHARD ROHLFS, after finding that his efforts to form a large society with a view of organising an exploring expedition to Africa upon a grand scale, have not met with the success he anticipated, has now resolved to start alone as on former occasions.

AT the meeting of the French Geographical Societies in Paris a number of resolutions were adopted, bearing principally on the teaching of geography and topography in the public schools, the creation of regional geographical museums and congresses, the means of multiplying the number of geographical societies, and fostering intercourse between members of the several European societies. It was decided that all the members of the different French and Algerian societies should have the right to be admitted to each others meetings. It was proposed to advise foreign societies to do the same, and to adopt an universal geographical society's ticket. The next national geographical exhibition will take place at Montpellier in 1879, on the occasion of the meeting of the French Association. A conference of all French societies will also be held at the same time.

THE gold medal for English maps, charts, &c., at the Paris Exhibition, has been awarded to Mr. Stanford.

BREWING IN JAPAN

AT the present time, when the history of the origin and development of the lower forms of life is occupying a great deal of attention, any facts which increase our knowledge of the growth of such bodies should be welcomed. In our breweries the growth of the yeast-ferment is tolerably well understood, or, at least, has been well observed and described. Under ordinary conditions the yeast-fungus exists only in the aquatic form, as it may be termed; and only under special circumstances, and with considerable difficulty in preventing putrefaction, is it enabled to produce spores. The internal substance of the cell becomes differentiated; granulations form and collect round certain points, and these ultimately become invested with a membrane, upon which the spores are ripe. The production of spores is thus unattended with the formation of a mycelium, or, if formed, it is so minute as to have been overlooked. This, however, is not a normal process of reproduction: the principal one, and indeed under the usual conditions, the only mode, is by budding.

Those living in Japan, however, have the opportunity of seeing a mode of fermentation which differs in many particulars from that employed in Europe. The subject is now under investigation, and at present I am not able to explain accurately what takes place; but as the process followed is interesting from its novelty, as it appears to consist in the previous practical use of a discovery

made by De Bary, and afterwards confirmed by Rees and by Fitz, that alcoholic fermentation can be effected by the growth of a species of *Mucor*, I am induced to give an account of a visit made, in company with some scientific friends, to the saké breweries situated about thirty miles away from Tokiô, the capital of Japan.

Saké is the general name given to the alcoholic liquid prepared by the fermentation of rice. There are many varieties of it prepared in different parts of Japan, each receiving some special name, either derived from the district in which it is prepared, or from some fancy of the manufacturer. It is a clear liquid, of a colour varying from the palest yellow to that of the darkest sherry, and containing from twelve to fifteen per cent. by weight of alcohol. There are some special kinds which contain much less alcohol—from four to five per cent—but they do not form the usual drink of the Japanese. It is almost always served hot, being placed in porcelain bottles, which are immersed in hot water and left there until the whole has attained the proper temperature.

This liquid is prepared on the large scale only in certain parts of the country, the most famous district being that near Ôzaka, one of the Treaty Ports. It is, however, often prepared on the small scale in private houses. The winter is the only season during which brewing operations are conducted, but this is not because the fermentation temperature is to be kept low as in the Bavarian method, but, I believe, in order to prevent the action becoming too tumultuous, for the temperature of fermentation is, in reality, even higher than that adopted in England. But, from the fact that the largest breweries are situated nearly 400 miles from Tôkiô, and the operations being carried on during a period when the University session is at its height, I have been compelled to confine my inquiries to the smaller breweries at Hachiôji, near this city.

The main room consists of a large wooden building about 120 feet long by 50 feet broad, and 25 to 30 feet high, running along the middle of which, in the direction of its length, is a platform about 12 feet from the ground, upon which some of the preliminary operations are carried out. Upon this a number of wooden tubs are placed, which serve for the preparation of the ferment, an operation which requires to be repeated several times during the brewing season. On the ground, ranged along the two long sides of the building are large tuns used for the storing of the saké when made, and some of which are also used for the actual processes of fermentation.

The brewing commences with the preparation of the ferment. For this purpose at the end of the previous brewing season a quantity of a green mould is produced upon rice by exposing steamed rice mixed with a certain proportion of the ash of some tree, and over which the spores of this fungus have been scattered in a well-closed chamber, which I may term the "fungus-chamber." This is a small room about 7 ft. high by 6 ft. broad, and 8 ft. long, well lined and covered with straw and matting, so that its high temperature may be kept up for a considerable time. In this chamber the rice and spores are left for about ten days, the atmosphere being kept quite moist by the vapour given off from the steamed rice, and at the end of that time the grains are found to be covered with a green fungus full of spores, and apparently the same kind as is found growing upon putrefying organic substances. The temperature of the chamber when examined was 25° C., that of the external atmosphere being 13° C. This product is called, in Japanese, *tane* or seed.

When prepared at the end of the season it is preserved until the next by being placed in bags, and inclosed in wooden boxes between layers of a mixture of equal parts of lime and wood-ashes.

When it is required to commence operations, a similar method is adopted to that just described, that is, a quantity of steamed rice is placed on wooden trays in the "fungus-chamber," but not mixed with any wood-ashes, and then *tăné* (spores) is scattered over it, and the chamber kept closed for a period varying from two to four days. At the end of this time the rice-grains are found to be covered with large quantities of fine hair-like threads, the mycelium of the fungus added. In this state it is called "kōji."

If this were left for a longer period in the fungus-chamber, it would produce spores, and the brewer calls it "the friend of tane," but in order to carry on the development of the mycelium most vigorously, it is necessary to use wood-ash in addition, which thus seems to act as a fertiliser.

Having thus obtained the "kōji," or mycelium, the brewer uses it for effecting the preparation of his yeast. For this purpose he mixes steamed rice with 30 per cent. of its weight of "kōji" and a sufficient quantity of water to make a thick mud, in small shallow wooden tubs, which are kept on the platform previously mentioned. In these it is frequently stirred and rubbed round with wooden tools, during a period of about ten days, in the course of which the grains of rice appear to be broken down, and the whole assumes a much thinner consistence, while at the same time the liquor becomes decidedly sweet. This is a change which is anything but clear; it would seem that it is connected with the development of an organism derived from the "kōji;" as on the small scale, I have noticed the production of minute cells, apparently budding, but whether they have any connection with the air-fungus, the mycelium of which covers the rice in "kōji;" or whether they have been developed from germs accidentally present in the "kōji," I am not able to say, though I hope that further experiments will make this point clear.

After the end of the ten days this product is mixed with fresh-steamed rice, water, and "kōji," and introduced into larger wooden vessels, in which the mixture is heated by means of closed wooden tubs, containing hot water, and in order to prevent too rapid radiation, the whole is covered with matting. The hot-water tubs are replaced day by day, so that the temperature is kept up for a period varying from eight to thirteen days. The average temperature seems to be about 35° C. (95° F.). During this time there is a continuous development of gas, and a scum gradually forms upon the surface until it has a thickness of a little more than one inch, and, when examined under the microscope, presents the usual appearance of brewer's ferment—saccharomyces. At the end of this stage, if the operation has been well conducted, five tastes are to be distinguished: sweet, bitter, astringent, alcoholic, and sour; but of these five, all of which are quite distinct, the bitter, astringent, and sour tastes are most marked. The product of this operation is called "moto," which means "source" or "origin," referring to the fact that it is from this ferment that the saké is subsequently formed. All the previous part of the brewing process has thus had for its object merely the preparation of the yeast, but it is certainly the most interesting, from the obscurity which surrounds it.

The actual fermentation is divided into three stages, called respectively *beginning*, *middle*, and *end*, the proportions of steamed rice and ferment varying slightly in each stage, but giving a final result of 100 parts of steamed rice to 30 parts of ferment. This mixture, together with the proper quantity of water, is placed in one of the large tuns before mentioned, and allowed to remain for about fifteen days in all, during which time fermentation actively proceeds, and the liquid becomes strongly alcoholic, at the end of which time it is drawn off from the grains of rice which have subsided, and introduced into other tuns, where it is allowed to remain to permit the remainder of the rice to be separated. The residue is placed in bags and subjected to pressure in a

lever press, the clear liquid which is expressed being added to that which has been clarified. It is now placed in boilers and heated up to about 60° C., after which it is kept in the store vats, carefully sealed up.

The residue left in the press is subjected to a process of distillation in a current of steam, by which a spirit containing about 42 per cent. of alcohol is obtained.

The saké in the store vats contains about 15 per cent. of alcohol, and this fact shows that the fermentation is different to that effected by the *Mucor racemosus*, as described by Fitz. In his experiments he found that the presence of $4\frac{1}{2}$ to $5\frac{1}{2}$ per cent. of alcohol killed the ferment, whilst in the process above described, we find the ferment acting in such a way as to produce 15 per cent. There is, however, nothing improbable in the supposition that different species may possess different degrees of sensitiveness to alcohol, and that the species used here may be less easily affected than the one employed by Fitz.

There are, however, many points about the process which are obscure, and about which I cannot say anything at present, but further experiment will, it is hoped, throw light upon the obscurity now enveloping the subject. The above account has been given in the hope that it may prove of some interest to those engaged in the study of fermentation, and that it may lead to a more extended examination of the action of various species of fungus upon amylaceous substances.

R. W. ATKINSON

University of Tokio, Japan

THE ORIGIN AND DISTRIBUTION OF ORGANIC COLOUR

COLOUR, throughout the realms of organic nature, is a factor hitherto held to be the most capricious in its distribution and the least amenable to any finite law. So uncertain and variable indeed are its manifestations that its claims for the purposes even of specific diagnosis have long since been ignored by the comparative anatomist. Nevertheless, when examined more attentively, an amount of evidence may be adduced sufficient to warrant further inquiry as to whether there is not existent beneath the superficial stratum of apparent disorder, a harmonious under-current indicative of a derivation in the abstract from one of nature's simplest physical laws, namely, that of polarisation.

Directing brief attention first to the subject of colour as distributed among the animal world, it will at once be recognised that, with few exceptions, it is only amongst the classes lower in the scale than the mammalia that this element either attains or retains its full exuberance. Even within such limits it will be further found that the beautiful and recently-discovered law of natural selection, for the purposes of concealment or protection, has in many instances so influenced and subordinated all pre-existing characters as to have entirely masked or eliminated them. The lepidopterous order of the class Insecta most prominently illustrates this latter case; the brilliant and varied hues of many members of this tribe being, as has been ably demonstrated, more especially by Mr. Alfred R. Wallace, in accord either with the flowers they most frequent, the objects upon or adjacent to which they rest in repose, or, still more remarkably, mimetically identical with those of other perfectly distinct species which owe their immunity from the attacks of birds to their peculiar acrid flavour.

The types among which have been first observed those peculiar colour-characteristics now to be submitted, are more particularly associated with aquatic life, and from these latter it has been found possible to extend and institute comparisons amongst almost every terrestrial group. Reference is here made to the dominance among the animal types in question of those so-called "complementary colours" familiar to all acquainted with the working of the polariscope. Of these colours in question, the combinations red and green, and blue and yellow are the most important, and it is surprising to find how frequently these reproduce themselves in nature.

Enumerating on this occasion merely a few instances, reference may be first made to those forms in which blue with its complementary hue, yellow, are found associated. Blue, as a rule, enters but to a comparatively trifling extent into the coloration of our indigenous fish fauna, but a remarkable and very gorgeous exception is afforded by the male of the Cuckoo Wrass (*Labrus mixtus*), which in its adult condition is resplendent with equally-distributed tints of the purest azure and most brilliant orange. These same complementary hues of blue and yellow obtain again in the male of another British fish, known as the Gemmeous Dragonet (*Callionymus lyra*). A conspicuous exotic example of the same colour-combination is presented by the Tesselated Parrot Fish of Ceylon (*Scarus hariæ*), characterised by its groundwork of azure blue decorated with an hexagonal network of golden yellow. Among the invertebrate division of the animal kingdom, the class of the Crustacea affords several interesting instances of a similar combination. Two of these belonging to the Decapodous order, *Galathea strigosa* and *Scyllarus arctus*, are of considerable size, having the deep orange hue of the general surface of their carapace variegated with bands and markings of brilliant blue. The Common Lobster (*Homarus vulgaris*), again, often "sports" into a bright blue variety, variously spotted and mottled with yellow. The legs of the Common Prawn (*Pandalus annulicornis*) are also most usually decorated with alternating rings of blue and orange. The marine copepods, more especially those of tropical seas, abound with instances of the association of these same two complementary hues. The class of the Mollusca is one among which blue as a pure colour but very rarely presents itself. There are, however, two Nudibranchs (*Goniodoris cœlestis*, Desh., and *G. elegans*, Cantraine) conspicuous for their ground colour of azure, accompanied in each instance by spots or lines of yellow. The last-named and finer of these two species, attaining a length of $2\frac{1}{2}$ inches, and usually classed as a Mediterranean type, was collected by the writer on the rocky shore of the Cies Islands, Vigo Bay, in association with the dredging expedition of Mr. Marshall Hall's yacht *Norna*, during the spring of the year 1871.

Pursuing the investigation among terrestrial types, the bird tribe—although it is necessary here to cite almost entirely tropical forms—produces abundant instances of the association of the same blue and yellow tints. The large Blue and Yellow Macaw (*Ara ararauna*) of South America forms a most conspicuous illustration, and the same two complementary hues will be found coupled together among innumerable other representatives of the parrot tribe, and likewise among the toucans. The most exquisitely beautiful blue bird yet known to science, the Azure Cæreba (*Cæreba cyanea*), is not altogether deficient of the complementary tint of yellow, several feathers of this colour appearing in the wings. One of the Cassowaries (*Casuarius kaupi*, Sclater), as figured in the Zoological Society's *Proceedings* for the year 1872, is represented as having the skin of the upper and front portion of the throat coloured azure-blue, while immediately behind and adjacent to this succeeds a patch of bright yellow. A still later and highly characteristic example is likewise afforded by the newly named *Euphonia insignis* of Messrs. Sclater and Salvin, figured at Pl. lii. Fig. 1 of the third part of the same *Proceedings* for this current year. Although our indigenous avifauna produces very few species in which the colour blue occupies a prominent position, the little Blue Tomtit (*Parus cœruleus*) is an exception which at the same

time altogether conforms to that law of polarity here brought forward. Thus while the feathers on the head and back of this familiar species are of a bright cerulean blue, those of the breast and under-surface are mostly yellow. Higher than the birds it seems useless to seek for the association of the two complementary tints under notice, though singularly enough one of these, the blue, turns up where it might be least anticipated, namely, amongst certain of the baboons, or Simiadæ, whose cheeks and remarkable posterior callosities are not unfrequently coloured bright cobalt. So late as the commencement of historic times our noble ancestors are reported to have supplied what nature had denied them in this department by the adorning of their persons with the juice of woad.

Turning now to the second complementary combination, that of red and green, it will be found to be far more extensively distributed even than that just discussed, birds, reptiles, fishes, and almost every class of the invertebrate kingdom supplying an important quota in which these colours are placed in juxtaposition. From among the first-named, the parrot tribe, woodpeckers, certain pigeons (*Ptilonopus*), trogans, and humming-birds, furnish abundant examples. Among lizards may be mentioned the Indian genus *Calotes*; while the ballan and corkwing wrasses, the Connemara sucker, and even the common stickleback in its breeding-dress, illustrate familiar examples of our indigenous species of fish in which green and red constitute the two dominant hues. To this last-named list of fishes might be added innumerable exotic types. With the crustacean class green and red appears to be a scarce combination, but one marine Isopod (*Spheroma*), remarkable for the variation in colour of different individuals, supply an exceptional illustration, I having by me alive at the present moment two examples, one of which is pale green, and the other scarlet. A spider, with whose technical name I am not familiar, but which often occurs on laurel bushes, has its body apple-green, supplemented with a bright red spot in the posterior region. Aphides, whose bodies are usually green, have frequently associated with the same scarlet or crimson eyes, and the same two colours will be found to be the prevalent tints of the larvæ of many lepidopterous insects. Green and red ascidians, worms, and zoophytes might likewise be enumerated, and the same combination is further traceable down to the Protozoa, and within the precincts of that debatable ground from whence both plants and animals take common origin.

Here, indeed, the subject assumes its most interesting and important phase, it being evidently at this initial point that the key and origin of the whole attendant phenomena are to be sought. Among a number of these humble unicellular organisms, including apparently as undoubted animals the Euglenæ or Astasiæ, and as true plants the Thecomonads and Volvocinæ, it will be found that the brilliant green hue of the general cell-substance is supplemented at one point by a brilliant scarlet speck, with which, in the case of animal organisms, the properties of a visual organ have been frequently associated. Though this interpretation does not meet with general support, it is remarkable how persistent is the presence of scarlet eyes among the more minute representatives of several invertebrate classes, including Insecta, Crustacea (some of these *monocular*), Annelids, and even Echinoderms, as typified by the starfish tribe. Advancing further on the vegetable side, the complementary colours of red and green, as first foreshadowed in unicellular Phytozoa, become still more conspicuously associated. Altogether apart from the floral elements the colour red constantly accompanies the more general green cellular structure of the stems, petioles, and leaf venation of ordinary plants. One remarkable Mexican species, indeed, *Pointsettia pulcherrima*, affords an instance in which the whole foliage forming a rosette at the terminal portion of the plant is the most brilliant scarlet, while that of the lower part is simply green. *Calladiums*, *Begonias*, and many other types might be quoted, in which the same two colours are equally blended in the leaves. Among the flowerless cryptogams, again, the group of the Bryaceæ, or moss tribe, yields further analogous and corroborative data. The peculiar fimbriated structure or peristome, which upon the dehiscence of the operculum guards the aperture of the spore capsules in this group, being in the majority of instances bright scarlet or crimson. The brilliant floral elements of the phanerogamic class are not so easily subordinated to that law of polarity, which appears to have left its impress on the simpler vegetative parts. Cross-fertilisation and numerous other external conditions and surroundings have no doubt exercised their influence in this case to an extent parallel to, though not identical with, what obtains among the Lepidopterous group of the animal series. Even here, however, it is a matter of question whether red is not the colour most extensively distributed. Certain forest trees, for example, such as the larch, poplar, and hazel (female flower), form interesting exceptions among a group more usually altogether wanting in brilliant floral decoration, and in all of which instances red is the characteristic hue of the flowers they bear.

How, or in what manner, the varied colours of nature were first called into existence is a problem that yet remains to be solved. Without presuming to put forward or propose any arbitrary interpretation of this primeval derivation, the following data may be tentatively submitted. The initial term of the series, as represented amongst the lowest animals and lowest plants, and in the latter instance continued throughout the higher forms, is evidently the colour green. Associated with vegetable life it takes the form of chlorophyll, and as such is altogether dependent for its origin and existence upon the influence of solar light. Shut off from such influence, this element fails to produce itself, and vegetable tissues remain white, as instanced in the artificial cultivation of seakale and other culinary herbs. Prolonged isolation from such light, however, results in the arrest of the vital functions, and this circumstance fully explains the absence of chlorophyll producing vegetation below that depth in the ocean to which solar rays have access.[1] Green again, is not a simple colour, but a combination of two others—blue and yellow. May not therefore its origin be remotely related to the effect of the solar rays, technically white and colourless, but yellow to the external senses as represented by ordinary sunshine and upon the artist's canvas, acting in concert with the blue ether of which our outer atmosphere is composed?

Given this initial colour green, the three primary hues of nature follow as a necessary consequence:—Blue and yellow by the resolution of the initial factor into its constituent parts, and red as its direct or reflex product in abeyance to the law of polarity. That this latter law exerts a considerable influence in the origin and distribution of those glorious tints of nature which may be said to constitute its most potent charm, will scarcely fail to recommend itself to the attention of those specially conversant with the physics of colour, and in whose hands this subject may prove susceptible of important development.

W. SAVILLE KENT

[1] Reference may be appropriately made here to the predominant colour of fixed deep-sea organisms, such as sponges and corals. In the hexactinellid sponge-form *Phæronema* (*Holtenia*) characteristic of abyssal depths, the colour, as observed by the writer in association with examples procured from a depth of from five to six hundred fathoms off the coast of Portugal, and in connection with the dredging expedition of Mr. Marshall Hall's yacht the *Norna* recently referred to, was the most brilliant orange. This hue was likewise observed to be characteristic of the soft parts of the large deep-water branching coral *Dendrophyllia ramea* obtained in the same expedition. It will at once occur that this colour, orange, is directly complementary or polar to that of the superincumbent mass of water, always distinguished when overlying such profundities by its deep cerulean hue.

ELECTRIC DISCHARGE IN GASES

I.

IN a paper read some time since at the Royal Society,[1] Drs. Warren De La Rue and Hugo W. Müller gave the first part of an account of their researches on the electric discharge in gases. This part, of which we shall at present give some account, consists mainly of a discription of the apparatus employed in this research, and of the results of their experiments with gases at atmospheric pressure and pressures down to 141 millimetres. They have since communicated to the Royal Society a second part, which treats of the discharge in highly rarefied gases (vacuum tubes).

The source of electrification used by Messrs. De La Rue and Müller was a battery of chloride of silver elements; commencing with 1,000, the authors have from time to time increased the number of cells to 11,000 joined in series (11,330 volts). In spite of its containing a somewhat costly material, this element, when compared with the many other forms of voltaic cell at present in use, possesses points of advantage such as render its general adoption extremely probable; we therefore transcribe in considerable detail the authors' description of it in its present improved form.

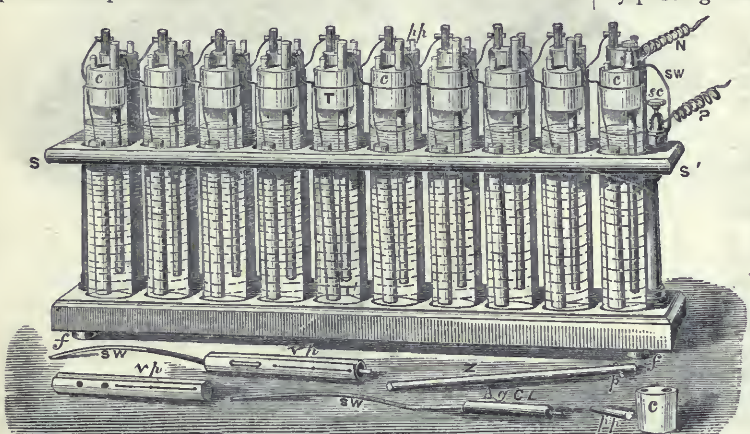


FIG. 1.—Nest of Cells.

Fig. 1 shows a nest of twenty cells of the most recent construction; the several components of the cells are given separately at the base of the stand. The glass tube T is $5\frac{1}{2}$ inches high, and $1\frac{1}{2}$ inch in diameter: it has a flat bottom. The stopper *c* is of paraffin, and is perforated with two holes, one for the zinc rod (*z*) to pass through, the other for letting in the liquid; the latter is ultimately closed with a paraffin plug (*pp*). Cork and india-rubber stoppers were tried and abandoned. The zinc is obtained of the Belgian Vieille Montagne Company. When it is intended to keep a battery more than a year in action the zincs should not be amalgamated, as the silver wires in contact with them would gradually be rotted through. Platinum, which would not become amalgamated, might have been substituted for the silver wires; but it would have cost 55*l*. extra per 1,000 cells. The zinc rods are 15 cm. long, 0·56 cm. in diameter, and perforated at the top with a hole 0·25 cm. in diameter. The chloride of silver (AgCl) is cast in the form of a rod on a flattened silver wire S W; the rods are 5·4 cm. long, 0·762 cm. in diameter, and weigh 12·97 grms. The silver wires are 20 cm. long, 0·127 cm. wide, and 0·0229 cm. thick, weighing 0·88 gms. each; they protrude slightly beyond the bottom of the rod of chloride, as will be seen from the figure. The wires and rods were obtained from Messrs. Johnson and Matthey, of Hatton Garden; the cost, including labour of casting, amounted to 2*s*. per cell. *v p* is a cylinder of three folds of vegetable parchment; at its lower end the folds are stitched together with thread, and at the upper part they are interlaced with the silver wire, and thus prevented from unfurling. The object of the parchment cylinder is to prevent the reduction of the chloride of silver rod, which would result from its accidental contact with the zinc. By giving proper bends to the silver wire before making up the cell it is easy to cause the chloride rod to lie vertically and press gently against the glass wall.

The liquid used for the cells is a solution of chloride of ammonium, 23 gms. to 1 litre of distilled water; by making use of a glass siphon with a long arm of india-rubber tubing, provided with a pinch cock, and terminating in a glass tube drawn down to enter freely into the hole in the paraffin stopper, it was found that 2,400 cells could be charged by one person in ten hours.

In making up a battery the glass cells are first arranged in their nests; next the paraffin stoppers are fitted with zincs; then the chloride rods are inserted and the cell closed with the stopper, the thin silver wire passing between the glass and the paraffin. When these operations have been performed for all, the cells are joined up by passing the silver wire of each cell through the hole in the zinc rod of its neighbour, and securing it there by pressing in a taper brass plug *p* with a pair of pliers. Ultimately, the cells having been charged with fluid, as already described, and closed by the insertion of *p p*, a hot iron is run round the outside of the stopper and round the zinc rod to secure the latter in its place, and make tight the joints by the melting and re-setting of a little paraffin.

Thus set up, the battery has an electromotive force of about 1·03 volt and internal resistance of not more than 5 ohms per cell: the former remains remarkably constant, but the latter increases, especially in cells that are left long idle. This rise in internal resistance is caused by a skin of oxychloride of zinc, which gradually forms on the zincs in all batteries where the zincs are immersed in a neutral chlorine compound. The battery may be restored to its original resistance by removing and scraping the zinc rods, but a more expeditious mode is to withdraw the small paraffin plug used to close the hole in the stopper through which the cells are charged, and to introduce into each cell, containing 50 c.c., 1 c.c. of pure hydrochloric acid, sp. gr. 1·16, containing 31·8 per cent. HCl gas, equivalent to 0·3689 gm. acid. Before introducing it into the cells, as is conveniently done by means of a graduated pipette furnished with a stop-cock, it is better to dilute the acid with an equal volume of distilled water. An effervescence takes place, and it is therefore necessary to allow the tubes to remain open twenty-four hours before the small paraffin plug is replaced, in order to permit the hydrogen, which is generated, to escape. It required two days for one person to perform this operation on a battery of 1,200 cells. The acid dissolves 0·3295 gm. of zinc or its equivalent of oxide.

After having been in almost daily use on circuits comparable with those occurring in overland telegraphy, it was found that in two batteries the amount of AgCl reduced in sixteen months averaged 4·57 gms. per cell, while in two other batteries which had been worked for ten months the reduction amounted to 3·57 gms. per cell.

An accident enabled the authors to give precise information as to the loss in working up the reduced silver; 600 cells having been accidentally allowed to run

[1] *Phil. Trans.*, clxix. pp. 55-122.

down, the loss in extracting and fusing the silver was 1·38 per cent.

Chloride of silver is charged at three-quarters of the price of silver; and the allowance made by Messrs. Johnson and Matthey for returned silver is about 6 per cent. less than the price at which they sell it. The charge for fusing and casting the chloride is 1*d.* per rod. Hence it appears that though the prime outlay is considerable, the cost of renewal is small, and a battery of these elements represents a certain amount of capital which might be realised at any moment. The labour required for setting up this battery is comparatively very little, and, this done, the element, from the insolubility of one of its electrolytes, is capable of standing idle for any length of time without other detriment than increase of internal resistance; this increase, too, occurs but slowly, many months elapsing before it rises from, say, 5 to 20 or 30 ohms per cell. Moreover, the element is notably clean and compact—a case 141 cms. high, 107 cms. wide, and 43 cms. deep, will hold 1,200. The details carefully given by Messrs. De La Rue and Müller of their very great experience of this valuable cell will probably be hailed as a boon by the many who have occasion to make use of voltaic batteries.

The accompanying diagrams show some of the apparatus which were specially devised by the authors to meet the insulation requirements of the high electromotive force they employed.

For instance, the ordinary form of double-reversing-key could not be used with this great battery, in consequence of the formation of an arc when the key is raised with the object of breaking the circuit. Figs. 2 and 3 show the new form devised by the authors. H is the handle fixed to the ebonite axis, which has metallic collars A Z at its extremities; these are connected to the springs S S′ by wires inserted in the axis. The battery wires are led to A and Z. The standards B and B, B′ and B′, are respectively connected by *diagonal* wires between the ebonite plates E and E′. Fig. 3 shows the open circuit position. In Fig. 2 the zinc pole is connected through S to B′ and the leading wire N′; the silver pole being similarly connected to P′. If now the handle H be thrown over to the right-hand side of the Fig. 2, Z will be connected to B on the right side, thence through the diagonal wire to B on the left side of the figure, and to P′, while A*g* will be similarly connected to N′. In this key, on breaking contact, the arms S S′ can be removed from the standards B B′ by a distance greater than that to which the arc can be drawn out.

Fig. 4 shows the micrometer discharger, by means of which the authors are able to measure the length of the spark to within $\frac{1}{10000}$ of an inch. Its construction is sufficiently obvious; it need only be remarked that the nut fixed in the cross-head at the top of the frame through which the screw works is in metallic communication with the clamp C, and is divided horizontally into two parts, which are pressed asunder by three spiral springs in order to prevent shake or play of the micrometer screw.

For special experiments it was necessary to design and have constructed a commutator capable of reversing the current many times in a second; that shown in Fig. 5 represents the most convenient form; it is capable of reversing the current 352 times in a second when the handle is turned 240 times in a minute. It will be seen that each revolution of A, B, C, D reverses twice. The figure is so distinct as scarcely to require any description; B and D are of one piece of metal, and also A and C of another, the spring conductors making contact at 90° distance from each other; each of the uprights supporting the axis of the revolving disc is in metallic connection with its respective insulated clamp. In the position shown in the figure the positive current passes from A*g* to the upright supporting the axis of the revolving disc, and through the right hand spring to the wire plate A*g*′; the negative current from Z to the upright on the other side of the revolving disc, only partly seen, thence through the upper spring to Z′.

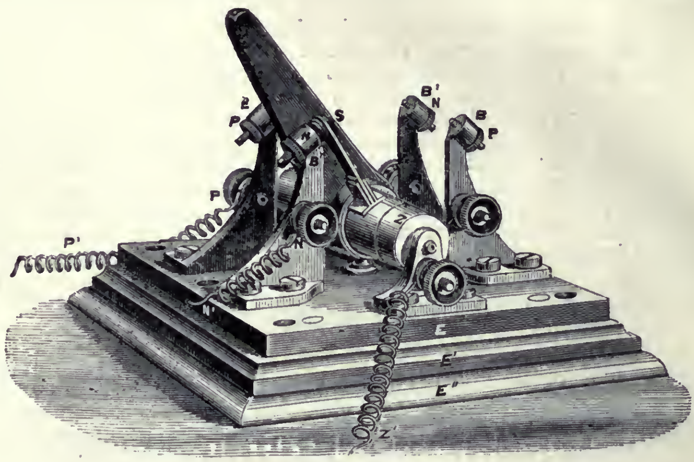


FIG. 2.

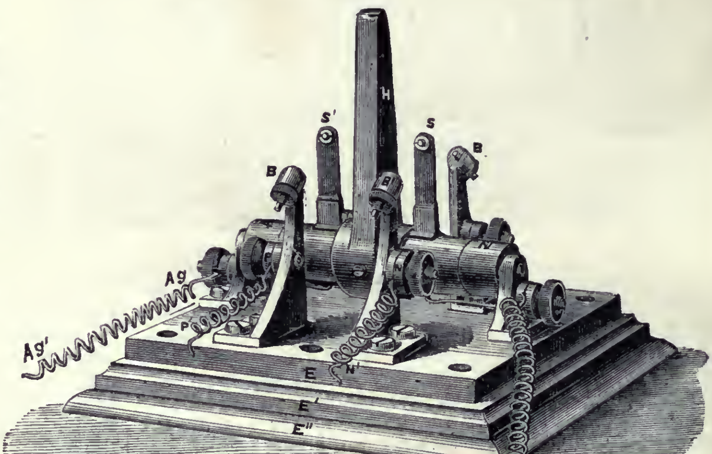


FIG. 3.

FIGS. 2 and 3.—Double Reversing Key.

Besides this, the authors had a contact breaker very similar in appearance, and shown *in situ* in Fig. 7 on the top of the dwarf cabinet of battery No. 1, containing 1,080 powder-cells; this cabinet top is of ebonite, and forms their ordinary working bench. M M′ represents a revolving mirror, which has a multiplying wheel, and in which the reflection of the discharge in a vacuum tube can be seen. In the circuit was a set of coil-resistances from 1 to 1,000,000 ohms, specially insulated, the wires running in grooves on insulating cylinders made of paraffined cardboard, in order that they may be kept at a distance; besides this set of resistances there were four

tubes of liquid having a maximum resistance of about 2, 4, 6, and 33 megohms respectively; all but one were furnished with adjustable wires by which their resistance could be diminished; two are charged with equal parts of water and glycerine, two with distilled water; each has a plug to throw the resistance out of circuit.

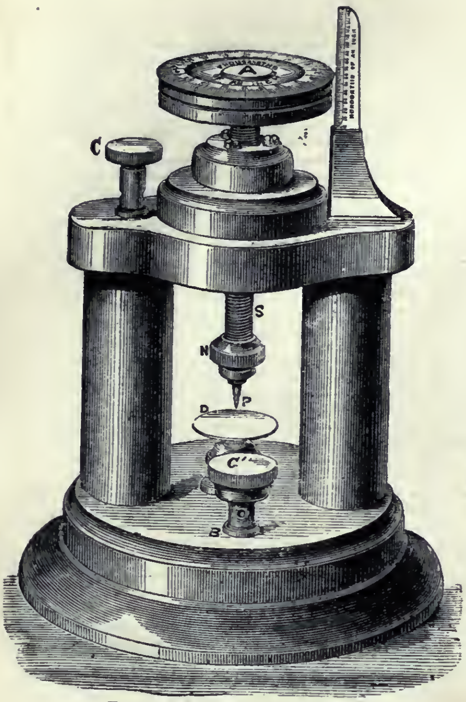


FIG. 4.—Micrometer Discharger.

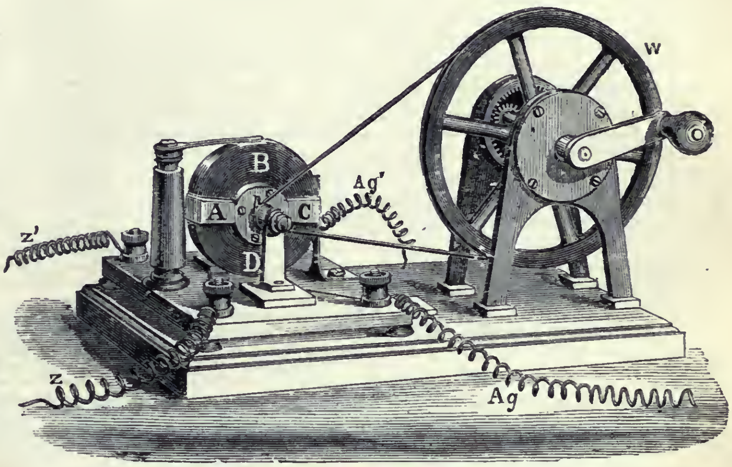


FIG. 5.—Rotating Commutator.

These resistances gradually diminish by the absorption of ammoniacal salts from the atmosphere, and this necessitates occasionally the entire renewal of the fluid.

As has been already stated, the paper deals principally with the discharge in gases at atmospheric pressure. Very elaborate series of measurements were made of the

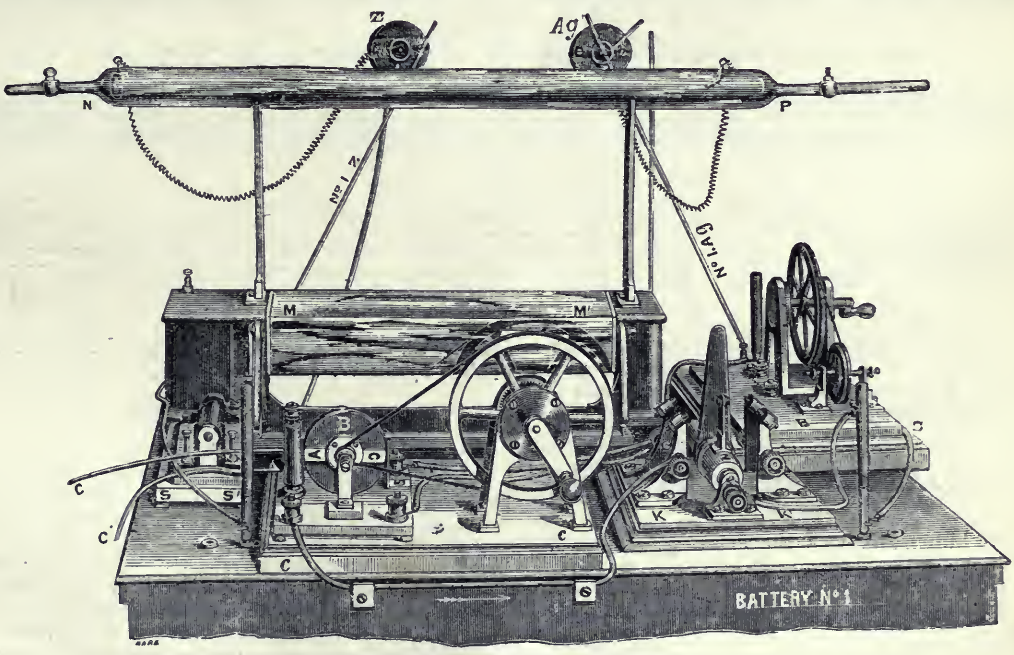


FIG. 6.—Table with Apparatus *in situ.*

potentials (*i.e.*, differences of potential) requisite to produce discharge at various distances between (1) plane surfaces, (2) spherical surfaces of 3-inch radius of curvature, (3) concentric cylinders, the diameter of the outer being constantly 0·4895 inch, that of the inner being varied from 0·4733 inch to 0·2865 inch, (4) a paraboloidal point and a

disc, (5) paraboloidal points. The results of each series are clearly set forth in the tables and plates which accompany the original memoir; here we have only space for a diagram (Fig. 7) giving a comparative view of the mean curves of all the measurements.

An inspection of the diagram will show that with plane

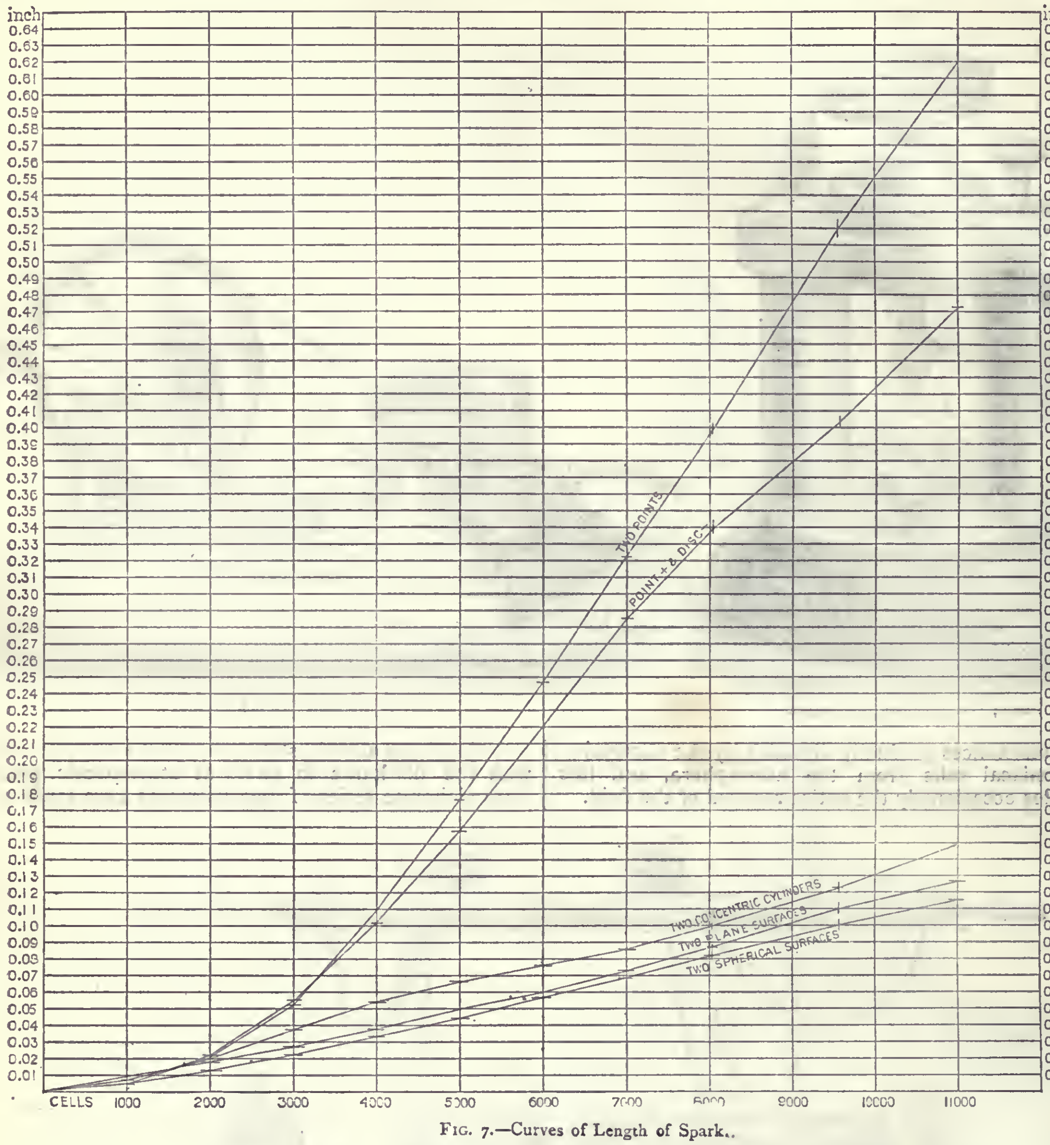


Fig. 7.—Curves of Length of Spark.

or slightly convex surfaces the potentials necessary to produce discharge at various distances are not proportional to the distances when these are small (less than 0·025 inch), but tend to become so as the distances are increased. These results confirm and extend those published by Sir W. Thomson.[1]

(*To be continued.*)

[1] *Proc.* Roy. Soc., vol. x. pp. 326-338

PHYSICS IN PHOTOGRAPHY[1]

II.

We have hitherto treated the question of a sensitive compound from what may be called a chemist's point of view, but it has also its physical aspect, and to enable us to understand what has recently been done in photography this latter must be briefly touched upon. To commence with, we are met with a difficulty in nomenclature which ought not to exist. Unhappily chemists and physicists employ the term molecule in a different sense. The physicist's molecule, for instance, in one place is defined[2] as "a small mass of matter the parts of which do not part company during the excursions which it makes when the body to which it belongs is hot." To avoid misapprehension the expression molecular group will be used for the physicist's molecule for want of a better, the word particle being rather too indefinite, and being usually applied to a group of molecules of visible size, a state of aggregation which is by no means necessary. The question as to the possible variation of the number of molecules composing a molecular group has not been entered into, as it would be trenching on ground

[1] Continued from p. 491.

[2] "Theory of Heat," Maxwell.

which has been explored by others in relation to a different subject; but this may be stated as a matter of observation that some compounds of silver which are sensitive to light are capable of forming two molecular groups, one of which absorbs the blue rays, and the other the red rays.

The iodide and bromide are the salts of silver which, either separately or together, are chiefly employed for securing a developable photographic image; and it is these with which we shall principally deal, though the chloride and one or two other combinations will come under review when considering certain new phases in photography. The point to which attention must now be directed are the radiations to which these compounds are sensitive; and these are evidently dependent upon the absorption that takes place in them individually. If we take precipitated silver bromide and fuse it into a crystalline mass, and examine it spectroscopically, we find that it energetically absorbs all rays from the extreme violet to the green, and also less markedly in the yellow of the spectrum; whilst if we place a slab of it before the slit of the spectroscope, and photograph the spectrum of white light passing through it, we find that it completely cuts off the ultra-violet rays; so that we may say that the red and perhaps the ultra-red rays are the parts of the spectrum in which but slight absorption takes place. Now, since absorption means work done in the absorbing body, it is evident that we may expect some action to take place in the silver bromide when exposed to these rays; and the action may be a chemical change, or a rise in temperature, it being remembered that the latter may co-exist with the former, since it may be produced by the result of chemical action as well as by the absorption of the radiation. The question then arises, On what does the possibility of a chemical change in a compound depend? This is a question which is very easily asked but not so easily answered. It must evidently depend amongst other things on the capability of the molecules of the compound to throw off some atom or atoms ; or on their capability of acquiring some vibrating atom or atoms of a body with which they may be brought in contact; in other words, that the molecule shall be in a state verging on indifferent equilibrium, and seeking rearrangement of the atoms when the impulses of the waves forming the radiations impinge against it. Taking for granted that the chemical theory of the formation of the photographic image holds good, we know that the atom of the halogen is thrown off from the molecule of the silver compound, leaving behind two atoms of the metal combined with one atom of the halogen. Thus silver bromide (Ag_2Br_2) is split up into argentous bromide (Ag_2Br) and bromine. If a film of the yellow sensitive salt be exposed to the spectrum an image is developed on all the parts which it absorbs, in other words, absorption in this case means chemical action. Similarly with the iodide where the absorption of radiation is most energetic there we have a chemical change. With the chloride we have a different phenomenon ; crystalline chloride is nearly colourless, and the absorption of every part (though not necessarily of the invisible regions) of the spectrum must be nearly equal, but we find that this salt is sensitive only to about the same radiations, under ordinary conditions, as the iodide. An explanation of this will be offered at a more advanced stage of these articles, but it may be as well to note the fact now. In the accompanying diagram the ordinary results of the exposure of the three chief silver compounds to the spectrum are shown (Fig. 2).

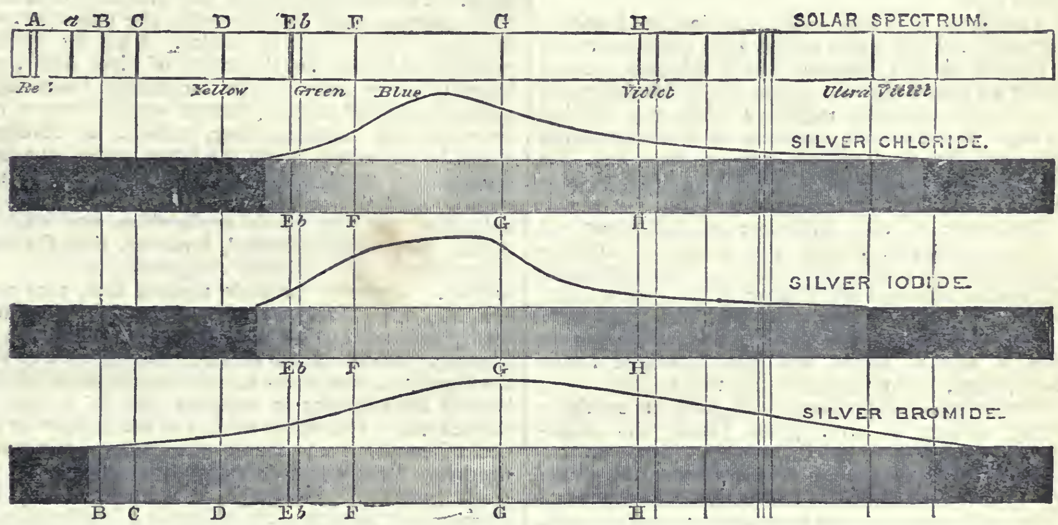


FIG. 2.

When we obtain a molecular grouping in which the red is absorbed, we may expect that the same chemical change may take place as when in the former state. This is not a necessity, since, instead of a reduction to a less complex form of molecule, the new compound, formed by the aid of light, might be rendered more complex by annexing other foreign atoms. An example of this we have in Hunt's experiment, already quoted; where it is evident that the violet subchloride would be sensitive to light if it were proved that light alone caused the absorption of oxygen. Again, in the combination of chlorine and hydrogen, when exposed to light, we probably have a greater complexity of molecular structure introduced.

In the ordinary state in which silver bromide is formed in a collodion film (we will take this compound as it is this one which has been principally examined), we have it in the state in which the blue is absorbed and the red transmitted; but there are means by which it may be made to absorb the red. We may obtain the bromide in either state by what is known as the "*emulsion process.*" Briefly, this process may be described as one in which an emulsion of silver bromide is formed in a viscous liquid, such as collodion or a solution of gelatine, by first dissolving a soluble bromide in it and then adding to it a solution of silver nitrate. The particles of solid bromide thus formed remain suspended in the fluid. In the emulsion with collodion we can get the silver bromide in various states according to the rapidity with which we add the silver nitrate. Thus if we add it rapidly, we get a mass of silver bromide which

is coarse and has a tendency to subside, and which, when poured upon a glass plate and dried, gives a film transmitting white light; whilst if we add it drop by drop and shake it up between each addition, we find that the bromide remains suspended for days, and that a film of it transmits orange light. If we take the emulsion, however, as first described, and place it in a still, and bring it to a state of ebullition, distilling over the ether and alcohol solvents of the collodion, thus breaking up the heavy particles of the suspended bromide, and then wash the contents of the flask in water to get rid of all soluble salts (which must necessarily exist owing to the double decomposition of the soluble bromide and the silver nitrate), we shall find, on redissolving the mass, after washing with alcohol, that the film will transmit lavender or a sky-blue light. On the other hand if the solvents from the emulsion which transmits the orange light be allowed to evaporate spontaneously and the solid residue be then washed and redissolved, the film will still transmit red light. It will thus be seen that silver bromide may be secured in two states *in one of which it principally absorbs the blue and in the other the red.* The chances are that the former is sensitive to the less refrangible rays of the spectrum, whilst the other is only sensitive to the more refrangible rays.

The emulsions, if made exactly as described above, might or might not give a film which, after exposure to the image formed in the camera, would develop without blackening all over under the action of the developer, or, in photographic parlance, might or might not develop without fog. If the soluble bromide be in excess in the emulsion, and be then washed out as described, the sensitive films would be free from this evil, whereas if the silver nitrate were in excess, this would not be the case. The cause of this difference in behaviour has lately been explained, though the means of correcting the emulsion has long been known. The reasoning will perhaps be rendered more clear if the experimental proofs be recounted in the order in which they were made. We must suppose that we have at hand a perfect emulsion, a film of which will give a bright image on development, after exposure in the camera. Let half-a-dozen plates be prepared with such an emulsion by simply flowing it over glass plates and allowing the films to dry; and then let these be exposed in the camera for the time necessary to give a strong image on development. Let the plates be immersed respectively after exposure in a weak solution of nitric acid, of potassium permanganate, potassium bichromate, nitrous acid, hydroxyl, iodine or bromine vapour, or be exposed to the action of ozone, it will be found that the image impressed by light will steadily refuse to develop, however much it may be coaxed; or again, if another half-dozen plates be prepared and be exposed to light external to the camera, we know if exposed to the lenticular image after such treatment, that we might obtain an image on development, but that it would be obliterated by the veil induced by the preliminary exposure. If between the preliminary exposure and exposure in the camera the plates be treated with any of the above solutions or vapours, and be then washed, we should find the impressions of light in the camera would yield images perfectly free from the veil. In other words the treatment of the film with any of these solutions will destroy the effect of the action of light. Now as we have already shown, the image is formed of silver sub-bromide; hence we may say that the treatment has changed the sub-bromide to an undevelopable compound. When exposed to iodine or bromine the sub-salt will naturally become converted into the primitive salt, but when we look at the nature of the other destructives, we cannot but be struck with the fact that they are either solvents of metallic silver or oxidising agents. In the first case we may presume that the loose atom of the silver of the sub-bromide (Ag_2Br) is dissolved away and converted into some other form of silver, leaving behind the half molecule of bromide, and in the other we may presume that the sub-bromide is oxidised to form an oxy-bromide of silver.

With this fact as a starting-point, it appeared probable that the elimination of a veil due to an emulsion ought to be effected by the same agents as if the veil occurred through the action of light. It was well known that in order to get an emulsion perfectly free from this enemy that chlorine, bromine, iodine, or some diad chloride or bromide were necessary to be added to the washed emulsion if the silver nitrate were in excess at first, and that nitric acid had the same effect if added to the emulsion with the silver nitrate. Here, then, seemed to be the proof of what was wanted, but another link was still required to make the reasoning complete. In making an emulsion if the soluble bromide was in excess none of these agencies were required. The question then arose as to why this was the case. To clear this up a fair hypothesis was taken, viz., that no soluble bromide was absolutely free from contamination. If the bromide were of the alkalis or some of the metals such as zinc, it was probably contaminated with the oxide, whilst with other diad metals it was probably of the lower form of bromide. Thus cupric bromide was probably contaminated with cuprous, and cobaltic with the cobaltous, though in infinitesimally small quantities. Now the former impurity would cause the formation of silver oxide, and the latter of silver sub-bromide (argentous bromide). Experiment showed that the former would act as a nucleus on which the metallic silver, reduced by development, would be deposited, whilst the latter would have the same composition as the latent image and thus induce the objectionable veil. The same reasoning applied to the chlorides, but the whole explanation was still incomplete. Experiment showed, however, that the order of formation of the different compounds of silver was as follows. Argentic bromide formed first, next argentic chloride, then argentous bromide and chloride, and finally the oxide. If, then, there was but little of the impurity present in the soluble bromide used in forming the emulsion, pure silver bromide would alone be formed, leaving the impurity in solution and in a state to be washed out. The whole subject of the fog-giving properties of emulsion was thus cleared up, and the correction necessary for it was apparent.

All practical photographers are aware that in the ordinary dry plate processes there is a deterioration of the image if plates be kept a long time before exposure and development, and if sufficiently long time elapse that the image will almost refuse to develop at all. The question arises why this obliteration of a developable photographic image takes place? We have seen how an image can be destroyed artificially by the use of oxidising agents, and we might naturally infer that the same destructive agency might obliterate the image even when the oxidising agent is merely ordinary air, and after considerable experimental proof we are compelled to come to the conclusion that this is the case, more especially when it is found that any readily oxidisible matter, such as gallic acid, if applied in solution and dried in contact with the sensitive film, preserves the image for a longer period than if this precaution be omitted. The oxidisable matter has to be oxidised before the image itself is attacked, for we may assume that the image itself is not as readily oxidisable as such bodies as that mentioned, and many others. The fact that the image can be oxidised and thus destroyed, seems to disprove the once held opinion that the undeveloped image was formed of metallic silver, a body which will tarnish but not oxidise, the pure oxides being unstable.

As a sequence to the destruction of the photographic image by oxidation, the hitherto unexplained results which Draper obtained when photographing the spectrum

were examined. Briefly it may be stated he found that if a spectrum was allowed to fall on a sensitised daguerrotype plate, which had received a preliminary exposure to white light, a remarkable phenomenon took place—a phenomenon which was also observable if weak white light were allowed to fall on the plate during its exposure to the solar spectrum. In developing such an image with mercury vapour, the blue, or most refrangible end of the spectrum was impressed in the usual way, that is to say, the Fraunhofer lines showed as dark lines on a lighter back-ground; at the red or least refrangible end of the spectrum, however, the Fraunhofer lines were seen as light on a darker back-ground; in other words, the photographic action was reversed, the neutral point of no action lying somewhere in the yellow. On studying a picture taken by this means, it was observed that in all cases the darkest Fraunhofer lines had the same tint, and that effect of light lines upon dark background, or dark lines upon the light background, were caused by alteration in tint of the background itself. Could this effect have anything to do with the oxidation? if it had it would indicate that the rays in the least refrangible end of the spectrum must *accelerate oxidation;* for it must be remembered that the plates had received an exposure to white light, either before, or during, exposure to the spectrum, and that the red rays prevented the development of the effect of the white light.

Now Draper had failed to get the same result on films of collodion containing the sensitive silver compounds, though he had obtained the reversal of the least refrangible end of the spectrum on such plates. If the theory

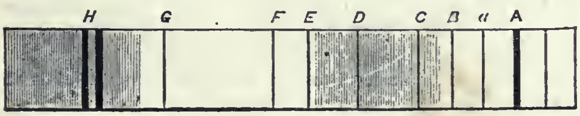


FIG. 3.

FIG. 4.

of oxidation held good for daguerrotype plates it ought also to hold good for the collodion films, and experiment decided once again in favour of the theory. Collodion films, which held *in situ* the blue form of silver bromide already alluded to, and which had been proved to be sensitive to the red end of the spectrum in the ordinary sense of the word (*i.e.*, that a proper negative picture of it could be obtained as it could of the blue end), were the subjects of experiment. It is evident, if the red rays were accelerators of oxidation, that in order to get a positive picture of the red end (*i.e.*, one answering to the reversal of the Fraunhofer lines in Draper's daguerrotype plates) the films should be exposed to the spectrum whilst in some oxidising medium, weak enough by itself, not totally to obliterate during the time of exposure to the spectrum any preliminary exposure which should be given to them, and yet strong enough to do so and to destroy the reducing action of the red rays, if these latter possessed a power of accelerating oxidation. Ozone, hydroxyl, nitric acid, and other oxidising agents, completely corroborated the idea that all the red rays had the power of accelerating oxidation, as the positive pictures of the red end here obtained, and in some case of the blue end, with negative pictures of the yellow and ultra-violet. The strength of the oxidising solutions was very small; thus, when nitric acid was used, four drops to a couple of ounces of water was found sufficient to cause this remarkable action to take place, whilst if the nitric acid were reduced in quantity, or omitted altogether, the effect of the ordinary negative picture was obtained in that part of the spectrum (Figs. 3 and 4). On the other hand, when the strength was increased, the image disappeared altogether. Figs. 3 and 4 show the results indicated above; the shaded portions show where the spectrum was photographed in the usual way, the white portions indicate where the reversed action took place.

We are not sure but we believe that Draper used silver iodide as his sensitive salt in the experiments with collodion, in which he failed to obtain these phenomena. The iodide is insensitive to the red end of the spectrum under ordinary conditions of exposure, being usually exposed in the presence of a solution of silver nitrate which clings to it after taking it out of the bath. It was found, however, if this free silver nitrate were washed away and if the exposure to the spectrum took place in the oxidising medium, after a preliminary exposure to white light, that this reversing action, as it is called, of the red end of the spectrum was obtained; and under certain conditions if the silver nitrate were removed, that the same results could be obtained even when the plate was not exposed in this oxidising medium.

Now regarding the reversal in the blue indicated above how can it be accounted for? By the very same theory, only an abandonment of the hypothesis that the least refrangible end of the spectrum *alone* is an accelerator of oxidation becomes necessary. In all comparative experiments made with the daguerreotype plate and the collodion film the difference of these conditions must be remembered. In the former the halogen liberated by the action of the light on the iodide combines immediately with the metallic plate forming fresh sensitive compound; in the latter the thickness of the sensitive compound has a limit, and much of it is altogether inoperative, the outside of the particles alone being available for the reducing action of light, and the halogen has to escape or be absorbed as best it may. In a collodion film it is manifest that the reduction of all the sensitive compound available must take place after a time, and when this is the case, if the same rays which effect reduction likewise accelerate oxidation, that the latter effect of the rays will have unimpeded action. So much for the theory, does experiment prove or disprove it? It is evident if the hypothesis be correct, that a film which is exposed to the action of light in a medium free from oxygen, or in one which is an absorbent of oxygen, should be incapable, on development, of showing this reversal of the ordinary action of light.

The results again showed that the theory was borne out, for it was found impossible to obtain a reversal of the image when so exposed. Here, then, we have a probable explanation of the phenomenon known as solarisation, to which allusion has already been made; it seems to be an oxidation of the undeveloped image.

W. DE WIVELESLIE ABNEY

(*To be continued.*)

NOTES

DR. O. FINSCH has resigned his appointment as Custos of the Museum of Bremen, and, as soon as the publication of his work on the results of the German Siberian Expedition of 1877 is completed, will leave Europe on a scientific mission to the Pacific and Australia. Dr. Ludwig, late Assistant in the Zoological Museum of Göttingen, has been appointed Dr. Finsch's successor.

WE notice the death at Cuenca, in Ecuador, on June 20, of Gustav Wallis, the botanist. He was born at Luneberg, in Detmold, May 1, 1830. In 1860 he was commissioned by the Lindens, the great horticulturists of Brussels, to gather new varieties of plants in South America, and during eight years, almost without cessation, he traversed Brazil, Peru, Ecuador,

Bolivia, Columbia, Panama, and Costa Rica, making everywhere enormous botanical collections. A similar journey to the Philippine Islands under the auspices of Veitch and Co., of London, followed in 1868, but in 1871 he was back again in his favourite field, the north-western portion of South America. Here he met his death in a hospital, reduced to poverty, and fairly worn out in the cause of science. Wallis stood fairly first among the travelling botanists of our day, possessing a rare combination of courage, energy, and scientific training. Not only were his contributions to botany of great value, but he actually introduced into European horticulture no less than 1,000 new varieties from across the ocean, and no small portion of the brilliant treasures of our modern conservatories are due to his unwearied zeal.

THE American Association met, under the presidency of Prof. O. C. Marsh, at St. Louis on August 21 and following days, and, judging from the reports that have reached us, the attendance was not quite up to the average, St. Louis having a bad reputation on account of its heat, this year aggravated by the dread lest the yellow fever might extend up the Mississippi Valley to the meeting-place of the Association. Vice-president Thurston gave the introductory address "On the Philosophic Method of the Advancement of Science," in which he traced the history of scientific research from early times, and advocated the establishment of a systematic method for discovering competent scientific writers, and endowing them adequately for the pursuit of research. Mr. A. R. Grote gave an address entitled "Education, a Succession of Experiences." Mr. Edison was present at the meeting for a short time and met with an enthusiastic reception; he read a paper on some of his inventions. A committee was appointed to arrange for a eulogy on the late Prof. Henry. We see from the neatly got-up *Daily Programme* of the Association that upwards of 100 papers were down to be read. We hope to give an account of the principal papers in an early number. The Association meets next year at Saratoga, on the last Wednesday of August, Mr. G. F. Barker of Philadelphia, president.

AT the recent International Congress of Anthropology several interesting reports were read by specially appointed reporters. M. Thulié gave a report on anthropological societies and education in anthropology. In tracing the history of anthropological societies in France and England, he showed the gradual progress which had been made in the objects and method of the science. M. Topinard's report was on astronomical, biographical, and pathological anatomy. He divided anthropology into general and special—the former embracing the human group as a whole and in its relation with the lower animals, the latter department being entitled "Zoological Anthropology;" special anthropology or ethnology investigates natural divisions, primitive or secondary, called *races*. Another division was into anatomical, biological, and pathological anthropology. Ethnographical reports were given by M. Girard de Rialle on Europe, Central and Western Asia, and America, and by M. Bordier on Africa, Eastern Asia, and Oceania. M. de Rialle called attention to the magnificent museums in the northern European countries, Sweden, Finland, and Russia, as contrasted with France, and still more markedly, we might say, with England. These reports were illustrated by reference to the fine collections shown at the Paris Exhibition. There were two reports in the department of prehistoric archæology, one by M. de Mortillet on geological times, and the other by M. Cartailhac on the neolithic period. In the report on demography (a sort of statistical anthropology), by M. Chervin, he gave a beautiful example of a statistical study by Dr. Berg, of the Swedish Statistical Bureau, who traces the influences of the Swedish wars between 1795 and 1810, generation after generation down almost to the present day. Dr. Lebon read an instructive paper on the results of his experimental researches on the variation of volume of the cranium in their relations to intelligence. He showed that intelligence was proportionate to the volume of the cranium, and that among the higher races the difference between the size of individual crania is less than among inferior races. A curious result is that among the women of the inferior races the cranium is generally larger than among those of the superior races; this result he ascribes to the insignificant part allotted to females in the active work of civilised society.

AMONG the resolutions passed by the International Congress on weights, measures, and coins, at Paris, was the following:—"The Congress learns with pleasure the progress of the metric system; it deplores that England, Russia, and the United States have not yet entered into the same path; and it is of opinion that the Governments of those countries should be solicited to give effect as early as possible to an act of progress so eminently useful to science, commerce, and international relations." The British and American members had a separate meeting, and resolved to petition their respective Governments to appoint a mixed Commission to consider the adoption of the metric system by both countries, and to make all necessary recommendations for the proper legislation to secure the desired end.

THE *Gazetta d'Italia* recounts a somewhat remarkable change in the surface of the earth at the village of Ortagli, a short distance south-east of Florence. In the course of a few days the tract on which the village stands has gradually sunk, until the depression amounts to about fifteen feet. It has not been sufficiently regular to prevent the houses from making threatening divergences from the perpendicular, and the population has taken refuge in the field. Strange to say another tract of land about two hundred yards from the village is, on the contrary, gradually rising, at times nearly rapid enough to be noticed with the eye. Several Italian *savants* are on the ground studying the strange phenomenon.

AT the next meeting of Russian Naturalists at St. Petersburg the question of chronology is again to be ventilated, with a view of proposing to the Government the change of date from the old style, now in use in Russia, to the new style used everywhere else in Europe and abroad.

ADMIRAL MOUCHEZ appointed two days every month when the Paris Observatory might be visited by the public, but the number of requests increase so much that he has been obliged to establish supplementary visits. On Saturday, September 7, the number of visitors exceeded a thousand. Admiral Mouchez intends giving a great *soirée* at the Observatory, on which occasion all the celestial objects drawn from nature by a clever artist will be shown with a magic lantern. He has asked from the Ministry of Public Instruction the loan of a portrait of Louis XIV., the founder of the Observatory. This portrait will be placed in the large saloon and surrounded by the portraits of Lalande, Laplace, Arago, Leverrier, and other great astronomers whose names have been associated with the establishment.

A COMMISSION was some time since appointed to report on the great reflector which Leverrier discovered to be imperfectly polished. The Commission has examined the instrument carefully, but unfortunately drawn up an ambiguous report, so that Admiral Mouchez is said to be left in a most perplexing position. The report does not say clearly that the instrument is good, but at the same time it gives no authority to reject it and to have it polished again. The perplexity is enlarged by the ineertitude on the results of works in course of execution. The polishing of the glasses of the large refractor has been placed in the same hands in pursuance of a contract signed by Leverrier long ago. It is expected that at the next meeting the Council of the

Observatory will give to the director the means of protecting efficiently the interests of Government and science.

MOUNT VESUVIUS is showing visible signs of agitation. An overflow of lava is considered probable on the side towards the Observatory.

AT a meeting of the Sunday Society a report was read from Mr. W. E. A. Axon, hon. secretary of the Manchester and Salford branch, announcing that, after a debate which had extended over three meetings, the City Council had, by a majority of 28 against 20, acceded to the memorial from the Sunday Society, and decided to open the several free libraries of the City on Sunday afternoons. Much satisfaction was expressed with Mr. Axon's report, and on the motion of Mr. Mark H. Judge, seconded by Mr. Frederick Long, a resolution was unanimously passed thanking the branch for the energy they have exhibited, and congratulating them upon the success they have achieved. The libraries were opened last Sunday for the first time.

ONE of the large monkeys at the Alexandra Palace had been for some time suffering from the decay of the right lower canine, and an abscess, forming a large protuberance on the jaw, had resulted. The pain seemed so great it was decided to consult a dentist as to what should be done, and, as the poor creature was at times very savage, it was thought that, if the tooth had to be extracted, the gas should be used, for the safety of the operator. Preparations were made accordingly, but the behaviour of the monkey was quite a surprise to all who were concerned. He showed great fight on being taken out of his cage, and not only struggled against being put into a sack prepared, with a hole cut for his head, but forced one of his hands out, and snapped and screamed, and gave promise of being very troublesome. Directly, however, Mr. Lewin Mosely, who had undertaken the operation, managed to get his hand on the abscess and gave relief, the monkey's demeanour changed entirely. He laid his head down quietly for examination, and, without the use of the gas, submitted to the removal of a stump and a tooth as quietly as possible.

THE *Daily News* Roman correspondent writes that from his villegiatura at Rocca di Papa, the archæologist Prof. Michael Stephen de Rossi, sends to the *Voce della Verità* an account of the earthquake that occurred there on the 3rd inst. At 11.13 P.M. of that day the inhabitants of the village were aroused by the very distinct shock of an earthquake, which was at first jerking, then undulatory in its movement, in a north-west south-east direction. The jerking lasted three seconds, but the undulatory stage occupied a considerable interval of time. In the seismographs the jerking stage of the phenomena was very strongly registered. What merits attention is that this shock coincided with a rise of the barometer,—at the close, that is to say, of an atmospheric storm. It happened when the temperature had barely reached its maximum for the season, thereby coinciding with the phenomena that took place on August 24 of last year. This earthquake was not unforeseen, although it was the first very perceptible one of which they have had experience this season. Already from August 24 there happened slight shocks, and sometimes they were felt even in Rocca di Papa. The instruments, particularly the tronometer, were continually agitated. On September 3 between 11.30 A.M. and 12.30 P.M., Prof. de Rossi counted eleven slight shocks, the most perceptible of them being that which occurred at 12.10 P.M. It was jerking on the 4th, the instruments being agitated in the most extraordinary manner, principally at 11.40 A.M. and between 2 and 5 P.M., at which time he also noticed two very tiny shocks. In the interval between August 24 and September 6 there were also felt some subterranean murmurs, for the study of which Prof. de Rossi did not, he regrets to say, put the microphone in operation. This wonderful instrument was scarcely dreamt of when he published his opinion that it could be applied to the observation of even the microphonic sounds which may accompany not only earthquakes but also microseismic movements. In fact, Count G. Mocenico, of Vicenza, tried for the first time to apply it for this purpose, with the result of hearing the most mysterious sounds which are produced under our feet in the depths of the earth. Prof. de Rossi ventures on no speculation as to the continuance and close of the actual seismic period. It is certain, however, he states, that it is found in strict relation with the extraordinary drought, of which, perhaps, as in the past year, it is the result.

WE have received a neat and well-illustrated guide-book to the new aquarium which has been opened in Princes Street, Edinburgh, under the direction of Dr. Andrew Wilson. The institution is intended to be utilised for instruction as well as for amusement, and in winter it is Dr. Wilson's intention to deliver occasional lectures of a popular kind adapted especially for school-children, who will thus, it is hoped, be incited to study natural history practically, or at least to take some interest in their living surroundings. When the arrangements are thoroughly completed it is hoped that a naturalist's table and small laboratory may be instituted. The directors, we are informed, appear to encourage as far as possible the educational features of the institution. We trust the institution will prove a success, and turn out an important addition to the educational resources of Edinburgh.

WE understand that Messrs. Chatto and Windus have in the press a volume of Essays and Lectures on Biological Subjects, by Dr. Andrew Wilson, of the Edinburgh Medical School. The work, under the title of "Leisure Time Studies," will be fully illustrated; some of the more prominent essays dealing with the relations of science teaching to ordinary education.

MR. W. S. SONNENSCHEIN will publish this year an English translation of Naegeli and Schwendener's well-known work, "Das Mikroskop. Theorie und Anwendung desselben," made by Mr. Frank Crisp, LL.B., B.A., Hon. Sec. to the Microscopic Society, and the publisher himself. The translation will be made from the last German edition, and will be supervised in part by Prof. Schwendener himself. The last chapter of the original (on Morphology) will be omitted, as having no reference to the microscope.

A GENEVA correspondent sends us a photograph showing the effects of lightning on an aspen (*Populus tremula*), situated in a wood near the Château of Crans on the shore of the Lake of Geneva. It was struck on August 9 last, in circumstances confirmatory of the views of Prof. Colladon (NATURE, vol. xvi. p. 568). The lightning chooses by preference the poplar as a conductor to reach the ground, and the case is striking here, where the tree is surrounded by other kinds, particularly firs, taller than it. Two great branches, of forty-five and fifty centimetres in diameter, which surmounted it, were struck by the lightning, and led it to the ground without having received the least apparent injury, while the trunk below them is absolutely shattered. This is a fresh proof that the upper part of trees, especially of poplars, is an excellent conductor of electricity, which only rends or shatters the wood when it finds a passage in the trunk. Other recent observations prove the preference of lightning for trees situated near streams or reservoirs of water, so that the best conductor for a house is a lofty tree, a poplar especially, situated between the house and a well, a pond, or a neighbouring stream.

AN interesting establishment was opened in the Champ de Mars, Paris, close to the École Militaire (within the precincts

of the Exhibition), a few weeks ago, and deserves notice. It is a technical library for French authors which now numbers more than 3,000 volumes. This library will be made permanent, and established somewhere in Paris when the Exhibition is over. It was originated by M. Tresca, the sub-director of the Conservatoire des Arts et Métiers.

THE weather being magnificent in Paris, the Giffard captive balloon takes up daily 500 passengers, paying twenty francs each, exclusive of a large number of *invités*, two aeronauts, and meteorological observers. M. Giffard has received propositions from the New York *Daily Graphic* for the purchase of his balloon, but he has declined; it will probably become a permanent institution in Paris.

THE Russian Technical Society at St. Petersburg has commenced the publication of polyglot technical dictionaries. The French-Russian-German-English part has just appeared.

THE rage for exhibitions has now spread even to Central Asia. The latest news from Tashkent states than an agricultural and industrial exhibition is about to be held there. Great preparations are being made for it at Samarcand, and the Government has promised gold and silver medals to the exhibitors as well as —honorary Kaftans!

WE recommend to all who have anything to do with the management of schools, two lectures by Dr. Liebreich, which have been published by Messrs. Churchill under the title of "School Life in its Influence on Sight and Figure."

THE botanist Fournier finds in Mexico 638 varieties of grasses, of which 376 occur in no other land. Of the remainder 82 are found in the United States, 30 on this side of the Atlantic, and the rest in the West Indies, South and Central America.

DR. AHLBERG describes, in Neubert's *Deutsches Magazin*, the Japanese flora as being at the height of its beauty in May. None of the great families is without its representatives, although as usual the Cruciferæ, Compositæ, Papilionaceæ, Ranunculaceæ, and Umbelliferæ predominate. The forests are marked by a variety of maples, numerous representatives of the oak, and a large number of varieties of ilex.

UNDER the title of "African Poisons" the *Journal of Applied Science* for the current month has some notes on the poisonous properties of a species of *Strychnos* and on the Inée poison (*Strophanthus hispidus*). Regarding the former it seems to be employed as an ordeal by the natives in Gaboon under the names of "Cusa" or "Icaja," and at Cape Lopez by that of "M'boundou." The plant producing it is described as growing in swampy or inundated situations, and attaining a height of only from four to six feet. The root is long and tapering, and is covered with a red bark, and it is from this bark that the active principle is obtained. The root-bark is scraped off and steeped in about a quart of water. When the water has acquired a reddish colour the poison is ready. According to the recent investigations of Messrs. G. Picholier and C. Saint Pierre the toxical principle of M'boundou is soluble in water and alcohol, and has a mode of action analogous to nux vomica, that is to say, acting on the sensitive nervous system. It only in a secondary degree affects the active nervous system. It is suggested in the article from whence the above information is obtained that it would be interesting to compare the M'boundou with the hoang-nan (*Strychnos gautheriana*) of Cochin China, which is employed in cases of leprosy and hydrophobia.

IN the British Section of the Paris Exhibition, amongst scientific instrument makers, the gold medal has been awarded to Messrs. A. Légé and Co., for their exhibit of instruments of precision and for the improvement of navigation.

WE learn that Parts I. and II. of Mr. Buchanan's work on "The Grasses of New Zealand," which is being published in the colony is on the eve of publication if it has not already appeared. These two parts contain twenty-one plates. The volume is large quarto and when completed will consist of five parts, and will contain fifty-five plates all native printed. It is said that Dr. Hector intends to bring out a reduced edition in octavo with the plates reduced by photo-lithography, and tinted. In this form it will be more handy and convenient than the original bulky volume.

THE Paris mint has published statistics on the value of pieces struck in the establishment from 1795, the date of its foundation, to 1878:—In gold, about 8,500,000,000 francs; in silver, about 5,510,000,000 francs; in copper, about 62,702,785 francs. Total value, 14,072,702,785 francs.

PROGRESS OF THE "CHALLENGER" REPORT[1]

AS a period of more than two years has now elapsed since the return of the *Challenger* expedition, I may very properly be expected to give some account to my scientific brethren of the progress which has been made up to the present time in the reduction and classification of the multitude of observations which were made in different departments, in the description and illustration of the natural history collections, and in the preparation of the official record of the voyage, which has been called for by government.

Before doing so, however, it may be well for me to sketch briefly the circumstances which led to the adoption of the plan now in operation for working up the results of the expedition, and putting them in a permanent form.

The voyage of the *Challenger* was undertaken for a very definite purpose, the determination of the physical and biological conditions of the ocean, and as the period of three years and a half occupied by our cruise round the world was quite too short even to draw the first outline sketch of general deep-sea conditions, our time was entirely devoted while the ship was at sea to registering observations, and cataloguing, and labelling, and storing specimens. Owing to the great liberality of the government in supplying abundantly all the necessary materials and appliances, an enormous collection of marine animals was sent home from time to time in wonderfully good condition. It was a matter of distinct understanding when I undertook the scientific direction of the expedition, that the responsibility not only of the conduct of the scientific work during its progress, but of the working out of its results at its close rested with me, and before the end of this cruise I was called upon by the Lords Commissioners of the Admiralty for a statement for their consideration of the course which I proposed to pursue both with regard to the publication of the results, and the ultimate destination of the specimens and other materials. Of course I had given this matter much careful thought, and I was in a position to submit to their lordships a general plan which commended itself to their approval, and which is now in process of being carried out unaltered. I proposed that for the time the collection should be placed in rooms which were given to us for the purpose in the University of Edinburgh, and that for the first year our attention should be chiefly directed to the preparation of an outline of the general report, and to the examination of the collection and its rough classification in zoological sequence; and that during this period the services of the gentlemen who had been associated with me on the scientific staff on board should be retained. I proposed that as soon as possible arrangements should be made to invite gentlemen who were recognised as authorities in different departments, and who had sufficient leisure at their disposal, to undertake the description of the zoological series, group by group, and that a sufficient sum should be granted to defray the expense of complete illustration and to compensate them to a certain extent for their expenditure of time.

With regard to the destination of the collection, I proposed that in the first place each specialist who undertook the description of a group should be requested to set aside all unique specimens

[1] "On the Progress which has been made in the Preparation of the Official Report of the *Challenger* Expedition." Paper read at the Dublin meeting of the British Association by Prof. Sir C. Wyville Thomson. Revised by the Author.

and the most complete series possible of all species of which there were duplicates to be sent at once to the British Museum, and that afterwards duplicates should be arranged in sets, and distributed to museums at home and abroad, according to a scheme to be sanctioned by their lordships.

Many considerations entered into the selection of the experts, into whose hands this vast collection of new material was to be placed. As I was solely responsible to Government for the general result, I was of course obliged to undertake this duty; but I rarely trusted my own judgment, acting in most cases with the advice and sanction of one or other, the weight of whose opinion on the special question at issue would be universally recognised. My sole object was to carry out the task intrusted to me to the best of my ability, and to prepare and to submit to Government the most complete report possible, and I asked the co-operation of men who, in my judgment, were the most likely to insure this result. I have to acknowledge, with profound gratitude, the frank readiness with which almost every appeal for assistance was responded to, and it is a matter of great satisfaction to me that the plan of work, which is the result of these arrangements, has elicited an expression of general approval from nearly all of those to whose opinion on such a question I attach the highest value.

From what I see at present the official account of the voyage of H.M.S. *Challenger* may be expected to extend to from fourteen to sixteen quarto volumes of 500 or 600 pages, the whole illustrated by about 1,200 lithographed plates, and many charts, woodcuts, and photographs. The MSS. of the first volume, which will contain a general account of the voyage, the hydrographic details contributed by Staff-Commander Tizard, the head of the Naval Surveying Staff on board, is nearly completed, and the charts of the ship's course and the sections showing the vertical distribution of ocean temperature which illustrate this volume, are in course of preparation. The second volume will consist chiefly of tables, and will include a report on the magnetic observations made during the voyage, drawn up under the superintendence of the Hydrographer to the Navy; and a detailed report on the meteorology, prepared by Capt. Tizard. The greater part of this volume is already in print, and I place before you a copy of the Magnetic Report, which will give an idea of the general appearance of the book.

Another volume will contain the discussion of the nature and composition and source of the deposits forming at the bottom of the sea, the composition and specific gravity of the sea-water, and the proportions of its contained gases, and sundry other questions, chemical and physical; and the remainder of the work will be occupied by a series of memoirs by different authors on the various groups of animals which constitute the deep-sea fauna. A large number of these monographs are in progress, and I hold in my hand a series of about 150 plates of natural objects which are now on the stone.

Only one department is finished; and I have here to record my special obligation to my friend Mr. Thomas Davidson for a most complete and thorough memoir on the *Brachiopoda* of the expedition. Mr. Davidson has not only figured all the species himself with the utmost care, but he has added to his descriptions of the *Challenger* forms a discussion of the relations which they bear, in structure and distribution, to all other known living forms, which greatly enhances the value of his monograph.

The illustration of the *Foraminifera* has been undertaken by Mr. Henry Brady, and a rapidly-thickening pile of plates testifies to the diligence of his artist, Mr. Hollick.

This plate represents several forms of a remarkable little group of Rhizopods in some ways intermediate between the Foraminifera and the Radiolaria, to which we have given the name *Challengerida*, as the twenty or thirty species of which the group is composed seem hitherto to have escaped observation.

A splendid memoir by Prof. Haeckel, on the *Radiolaria*, is in progress, and will be illustrated by upwards of a hundred plates. Everyone acquainted with Haeckel's classical work, "Die Radiolarien," will appreciate our good fortune in having secured his co-operation.

The next series of plates prepared under the direction of Mr. Moseley represent the deep-sea corals, and the next series, also by Mr. Moseley, illustrate a most remarkable series of coralloid forms of *Hydrozoa*, on whose structure and relations Mr. Moseley's careful work during the voyage and since our return has thrown quite unexpected light. The normal *Hydrozoa* are in the hands of Prof. Allman, but owing to our having already secured for other departments the services of nearly all the available British artists experienced in natural-history drawing, he has not been able as yet to make much progress. Prof. Haeckel will describe the deep-water *Medusæ*, few in number, but of the highest interest.

Echinodermata are very abundant in the abyssal region. I have undertaken to describe a portion of the first class of this type, the Stalked Crinoids, and upwards of twenty plates are on the stone, illustrating their structure. The drawings are by Mr. William Black, one of the most successful students in the Edinburgh School of Art under the Science and Art Department, and now an accomplished natural-history draughtsman. And Mr. Herbert Carpenter takes up the *Comatulidæ*, a group rich in undescribed species, which he is studying, along with the valuable collection procured by Prof. Semper in the Philippines.

Prof. Alexander Agassiz is progressing rapidly with the *Echinidea*, and his monograph, exquisitely illustrated as all these American memoirs are, will probably be among those first finished. Mr. Lyman is working at the *Ophiuridea*, which he says are very numerous and almost all new, and I expect Mr. Théel, of Upsala, to come over shortly to examine the *Holothuridea*, which he is going to describe under the general superintendence of Prof. Lovén. The Swedish Arctic Expeditions have already yielded some of the most characteristic abyssal forms of this group, and these we know through the excellent memoirs of Mr. Théel.

This bundle of beautiful plates drawn under the eye of Mr. Busk, illustrate the *Polyzoa*, a group which descends to the greatest depths, represented by many undescribed and characteristic genera.

Dr. McIntosh is working out the *Annelida*, and will require a volume to himself, while several hands are occupied with the *Crustacea*, Mr. Spence Bate taking the heaviest part of the work in the *Macroura*, Prof. George Brady describing the *Copepoda* and *Ostracoda* with many beautiful plates, some drawn by himself and others with the assistance of artists; and Prof. Huxley dealing with the *Anomoura*, which some of his recent investigations have given for him a special interest. Mr. R. Boog Watson undertakes the lower *Mollusca*, a heavy task involving the steady work of a year or two, and a ponderous volume, in which he receives the friendly assistance of our colleague, Dr. Gwyn Jeffreys, Mr. Edgar Smith, and other conchologists here and elsewhere; and Prof. Huxley closes the Invertebrates with a monograph of the *Cephalopoda*, culminating in a gem of a memoir on the structure of *Spirula*, the drawings done by his own hand from the careful dissection of the single specimen which we procured.

Brief descriptions of the deep-sea fishes have already been published by Dr. Günther, and the final memoir is in preparation; the birds in which group rhere are a comparatively small number, are in the charge of Dr. Sclater; and some anatomical details involving important additions to our knowledge of the structure of the *Cetacea*, the *Marsupialia*, and the Penguins have been worked out by Prof. Turner, Prof. Morrison Watson, and Dr. Cunningham.

It will be seen that in the foregoing notes many important groups of marine invertebrates are still unaccounted for. These are cases in which it has not yet been found possible to commence work from want of artists or some other cause, or in which the naturalists engaged have not yet reported progress. I have every hope that all will be under way in the course of another year.

Perhaps I have said enough to show that a report is in progress of preparation, which may be expected to register in a suitable form the results of a great scientific expedition. Some causes of delay have occurred, and there has been the regulation amount of friction inseparable from the working of a complicated piece of machinery, but my feeling is that on the whole things are going on wonderfully well, and the utmost I anticipate is the necessity for a little extension of time for the appearance of the later volumes.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

DR. MANSFIELD MERRIMAN, author of the "Elements of the Method of Least Squares," and for several years past an instructor in the Sheffield Scientific School of Yale College, has accepted the Professorship of Civil and Mechanical Engineering

in Lehigh University, Bethlehem, Penn., and enters upon his duties there at the beginning of the September term.

WE noticed recently the death of Prof. Behn, president of the Kaiserliche Leopoldina-Carolina Deutsche Akademie der Naturforscher. The Academy has chosen as his successor for a term of ten years, Prof. H. Knoblauch, of Halle, well known by his researches on light. With this new election, the head-quarters of the Academy are changed from Dresden to Halle.

SCIENTIFIC SERIALS

American Journal of Science and Arts, July.—In his ninth paper of "Contributions to Meteorology" (based chiefly on observations of the U.S. signal service), Prof. Loomis shows, *inter alia*, that barometric waves often travel from the Pacific Coast across the Rocky Mountains (10,000 feet) and reach the Mississippi valley with but little modification. Areas of unusually high pressure in their course eastwards are generally accompanied by areas of low pressure both on the east and west sides, and at an average distance of about 1,200 miles. When a centre of low pressure has passed the wind sets in with force from the north, and being deflected westwards by the earth produces condensation there. Again, it is shown that an area of low barometer in Ireland is usually accompanied by one of high barometer in Southern or South-eastern Europe, and that during the colder months Vienna is generally near the centre of this high area, which is replenished by air rising from the area of low pressure.—Mr. Goldmark finds, from experiment, that a rise in temperature produces a slight but constant increase in the electric potential of the air.—A new and remarkable mineral locality (for manganesian phosphates) in Fairfield County, Connecticut, is described by Messrs. Brush and Ed. Dana. No less than six new and well-defined species have been got from it, besides many other known but rare species. All the minerals are obtained from veins of albitic granite.—Prof. Mayer claims independent invention of the sound-mill (lately described by M. Drorak); Mr. Chester describes some artificial crystals of gold and gold amalgam, and Prof. Rood suggests a form of telephonic relay.

August.—This opens with the first portion of an interesting lecture by Prof. Asa Gray on "Forest Geography and Archæology." The forest production of America is sketched and considered in its relation to climatic conditions.—Prof. Leconte gives an account of the structure and origin of mountains, and answers some recent objections to the "contractional theory."—The presence of a solid hydrocarbon in the trap-rocks of New Jersey Mr. Russell accounts for by infiltration of petroleum into cavities and subsequent evaporation and (to some extent) oxidation.—Prof. Trouvelot gives an account of his observations of the recent transit of Mercury at Cambridge, Mass. Among other points noted are the luminous ring round Mercury observed distinctly for more than two hours, and the rounding off of the angles formed by the sun's limb with Mercury after internal contact, a phenomenon of the same nature as the black drop (which was not seen).—Prof. Peters indicates the position of the new planet he discovered lately.

SOCIETIES AND ACADEMIES

GENEVA

Physical and Natural History Society, June 6.—Prof. F. A. Forel, wishing to explain the action of the seiches of the Lake of Geneva, seeks to establish the effects of storms, which often coincide with marked oscillation of the fluid mass. He is still continuing his investigations.—Prof. Schift described his researches on the alterations which the blood undergoes in consequence of a momentary arrest of the circulation.—M. Phil. Plantamour has studied periodical displacements in the air-bulb of improved levels manufactured at Geneva and placed in the cellar of his house. At certain times there is shown a slow change towards the east, without sensible return towards the west; at other times the immobility is complete. The maximum of eastern elevation takes place sometimes towards 5 P.M., at other times sooner, towards mid-day, with oscillations the amplitude of which does not exceed seventeen seconds.

July 4.—M. Eug. Demole presented phenomena of oxidation produced by the free oxygen of the air. Thus the case was presented of dibromic ethylene.—Prof. Forel, by increasing the rate of the unrolling of the paper of his limnograph, has attained interesting traces corresponding to the perpetual oscillations of the level of the Lake of Geneva. These oscillations vary from half a minute to two or three minutes in maximum, corresponding neither with the duration of the waves nor with those of the seiches. They vary notably in intensity with the agitation of the lake by the wind or by a steamer, but their cause is yet totally unknown.

VIENNA

Imperial Academy of Sciences, July 11.—The following among other papers were read:—On the nature of galvanic polarisation, by Dr. Exner.—Optical investigation of spark-waves, by Prof. Mach and Dr. Gruss.—On the optical properties of soot, by Dr. Rosicky.—Researches on the origination of chlorophyll-granules, by Dr. Mikosch.—On two new isomeric cyanide acids, by Herr Herzig.—On the elastic reaction in glass, by Herr Klemencie.—On the composition of cinchonin, and on its oxidation products, by Dr. Strauss.—Action of oxidising agents on some hydrocarbons, by Herrn Othman and Zeidler.—On camphor chlorides, by Dr. Spitzer.—On a camphene derived from camphor, and the synthesis of its homologues, by the same.

PARIS

Academy of Sciences, September 2.—M. Fizeau, president, in the chair.—The following papers were read:—Formation of a cylinder out of a circular disc, by M. Tresca.—Litter manure, by M. Ch. Brame.—On the influence of the quantity of blood contained in the muscles on their irritability, by M. J. Schmoulewitsch. The author has proved that the muscles, on becoming anæmic, do not lose their irritability at once; on the contrary, it increases for some time, and having attained a certain degree, begins to decrease.—On the existence of an intra-Mercurial planet observed during the solar eclipse of July 29, by Prof. Watson. M. Gaillot is investigating the results of Prof. Watson's observations.—On the diffusion of cerium, lanthanum, and didymium, by M. Cossa.—On the causes of the humming of insects, by M. J. Pérez. The author finds that among hymenoptera and diptera, humming is due to two distinct causes; one, the vibrations of which the articulation of the wing is the seat, and which constitute the true hum; the other, the friction of the wings against the air, an effect which more or less modifies the former. Among the powerful-winged lepidoptera, such as the Sphinxes, the sweet and mellow hum of these insects is due only to the rustling of the wings by the air. This sound, always grave, is the only one produced; it is not accompanied by basilary beatings, on account of a peculiar organisation, and especially on account of the presence of scales. Among the Libellula, the base of whose wings is provided with soft and fleshy parts, there does not exist true humming, but a simple noise due to the rustling of the organs of flight.—Application of borax to researches in vegetable physiology, by M. Schnetzler.

CONTENTS

THURSDAY, SEPTEMBER 19, 1878

THE LINKS OF THE ANIMAL WORLD

Les Enchaînements du Monde Animal dans les Temps Géologiques, Mammifères, Tertiares. Par Albert Gaudry. 8vo. (Paris: F. Savy, 1878.)

UNDER this poetic title Prof. Gaudry has added one more to his valuable works on the fossil mammalia of the Tertiary Period. Some sixteen years ago he carried on the explorations on the classic ground of Pikermi, which proved that the plains of Marathon were haunted by antelopes, gazelles, giraffes, and hipparions, in the upper miocene age, while the forests then overshadowing rocky Attica sheltered the mastodon and rhinoceros, and yielded food to innumerable monkeys (Mesopithecus) allied to the Barbary ape. During the last five years he has been painting a like picture of the upper miocene mammalia of Mont Léberon, in which, as in Pikermi, the remains of the animals are preserved in an abundance and a perfection somewhat like those discovered in the Western States, by Prof. Marsh. The two elaborate quartos on these discoveries are now followed by an elegant octavo full of woodcuts, treating of the links that bind together some of the more important groups of tertiary and living mammalia. In it the author deals with the European mammalia without treating exhaustively the accumulation of facts brought to light by Marsh, Hayden, Leidy, and others, in the American eocenes and miocenes. Nor are these as yet published in sufficient detail to allow of their being worked into the book before us. The impression made upon my mind by the vast stores of remains in the museum at Yale is one of profound melancholy; for so numerous and varied are the mammalia that many years must elapse before they can be brought into complete relation with those of Europe. Up to the present time the fragmentary notices which have appeared bear the same relation to the systematic treatment to be expected from Prof. Marsh, that isolated *tesseræ* bear to the whole design of a mosaic. Prof. Gaudry, therefore, has acted well in confining his work mainly to the Tertiary mammalia of Europe.

Our author approaches his subject from the point of view offered by evolution, by taking some of the more salient characters of a group, and tracing their "descent with modification," in passing from older to newer stages of the geological record. The fishes passed through the principal phases of their evolution in the Primary, the reptiles manifested their most extraordinary modifications in the Secondary age, and at its close had settled down into that equilibrium which they now present, while the mammalia were being unfolded, form after form—"en pleine évolution"—in the Tertiary Period. In the opening chapter the relation of the marsupials to the placental mammals is discussed, and our author concludes, from the results of embryology, that the former must have preceded the latter, and that the imperfect and helpless condition of the young, nourished in a marsupium, or taking refuge on the back of the mother, would be a serious obstacle to rapid increase, since the young would be likely to be drowned in the passage of rivers or arms of the sea in migration in search of food. This may have been one of the causes of their being supplanted by the placental mammals at the beginning of the Tertiary Period, as they would be heavily weighted in the struggle for life by this condition. The elimination of the marsupial herbivora from the Tertiary fauna of Europe and America might have been produced by the migration rendered necessary by the great geographical and climatal changes at the close of the cretaceous age. It is a significant fact that the only living mammalian genus found in the eocene age is the tree-haunting Didelphys, which took refuge probably in the forests, and held its place in Europe as late as the lower miocene age, and which still holds its own in the warmer parts of America as the sole representative of a Secondary marsupial fauna once in possession of the European and American continents. There are, however, traces of a marsupial fauna other than those offered by Didelphys, to be seen in certain *intermediate forms*. The Hyænodon of the eocene and lower and middle miocene, and the eocene Pterodon, were carnivores combining the dental formula of four true molars (three of which are carnassials), and three premolars of the *Thylacinus*, with the characters of an ordinary placental carnivore. The dentition of the lower eocene Palæonictis is remarkably like that of the Tasmanian Dasyure, and the lower miocene Proviverra unites a marsupial dentition and brain with the ordinary viverrine attributes. These marsupial characteristics can only be accounted for on the hypothesis that they have been handed down from a marsupial ancestry, and they render the conclusion highly probable, that all the placental are descended from the implacental carnivores.

Prof. Gaudry points out that the rhinoceros and the palæothere are probably descended from common ancestors, and shows the gradual modification of skull, and evolution of horns in the former animal. In the middle miocene a hornless rhinoceros (Aceratherium) appears, and is accompanied in the upper miocenes of Eppelsheim by a small horned species, of which it is probably the female. As we pass upwards to the pliocene the horn becomes more highly developed, and the nasal bones more strongly built, until we arrive at a maximum in the pleistocene *Rhinoceros tichorhinus.* We believe that the horn was originally a male character transferred ultimately to the female in the way pointed out by Mr. Darwin.

In like manner, also, our author traces the evolution of horns and antlers in the ruminants. The earliest representative of the order, if it be a representative, is the hornless Xiphodon of the upper eocenes. Horns come in in the middle miocene, and in the upper attain a considerable development, as in the antelopes. We may add that, in Europe, the bovine development of horns arrived at its maximum in the gigantic bisons and uri of the pleistocene, while in the pliocene of Italy there was a bos without any horns—a fact which renders it probable that the polled cattle produced by domestication is merely a case of a reversion to an ancestral type fostered by the care of man. None of the lower miocene deer possessed antlers. In the middle miocene they are bifurcated, as in the Muntjac; in the upper they are still small, and with three or more types; in the pliocene they pass through the stages presented by the

Axis and Rusa, ultimately culminating in the wonderfully complex antlers of *Cervus Sedgwickii* of the forest bed, and *C. dicranios* of the Val d'Arno, and in the gigantic antlers of the pleistocene, and pre-historic Irish elk. It must, however, be pointed out that the *Cervus Matheronis* of the upper miocene, identified by Prof. Gaudry with the Axis, has no relation with that animal, as may be seen by the examination of the most perfect antlers yet discovered of the former animal, in the British Museum. A more rudimentary form of antler (*Procervulus*, Gaudry) even than the Muntjac has been discovered in the middle miocene strata of Thenay, without a burr, which was, like ordinary horns, persistent through the life of the animal.

We have merely touched upon some of the questions raised in this work, which occupies most important ground in the evolution controversy, and may be looked upon as one of the first fruits of the principles laid down by Mr. Darwin in the "Origin of Species."

W. BOYD DAWKINS

AMERICAN GEOLOGICAL SURVEYS

United States Geological Exploration of the Fortieth Parallel. By Clarence King, Geologist in Charge. Vol. II. *Descriptive Geology*, 1877. Vol. IV. *Ornithology and Palæontology*, 1877. (Washington: Government Printing Office, 1877.)

THE important survey of a portion of the north-western states of America, which was commenced by Clarence King and his able assistants in 1867, has now, after ten years of arduous labour, been brought to a close. In the original scheme drawn up for the publication of the results of this survey it was proposed that the observations of the surveyors should be published in five volumes, devoted to the following subjects:—

I. Systematic Geology.
II. Descriptive Geology
III. Mining Industry.
IV. Zoology and Palæontology.
V. Botany.

The third of these volumes was prepared and issued soon after the commencement of the survey. It abounds with valuable details concerning the rich ore deposits of the north-west and the methods by which they can best be worked. It is difficult to know which to admire most—the accuracy and beauty of illustration of this volume or the characteristic energy and promptitude with which it was produced in order to meet a pressing want.

In 1876 a supplementary volume numbered VI., not contemplated in the original scheme, was published; it deals with the subject of Microscopic Petrography, and is from the pen of Prof. Zirkel, of Leipzig. As this work has already been noticed in the pages of NATURE, we need do no more on the present occasion than refer to the circumstances under which it was published.

Two other volumes, those numbered II. and IV. are now before us, and amply sustain the high reputation which Mr. Clarence King and his indefatigable fellow-workers have acquired for energy and zeal in the prosecution of their important task, no less than for great geological knowledge and literary ability.

The volume on Descriptive Geology consists of a series of chapters giving full and accurate accounts of the geological features of the Rocky Mountains, the Green River Basin, the Utah Basin, the Nevada Plateau, and the Nevada Basin respectively, the descriptions being from the pens of Arnold Hague and S. F. Emmons. The rocks exposed in this vast area include representatives of the whole series of geological formations from the Archæan to the Post-pliocene, together with many plutonic and volcanic masses belonging to various geological periods. The descriptions are of the most minute and careful character, and are interspersed with valuable analyses of the rocks described.

One of the most useful features of this volume is the series of twenty-six lithographic plates illustrating the grand and peculiar scenery of the district. These are admirable copies of photographs taken upon the spot, and they are probably, without exception, the most successful attempts to illustrate scenery in this manner that have ever been made. We would especially instance Plates V. and VI., illustrating the Tertiary bluffs near Green River City, Wyoming, and Plate XIV., showing the characters of the Wahsatch Limestone Cliffs, Provo Cañon, Wahsatch Range, as presenting the characteristic features of rock-masses in a manner which cannot fail to be appreciated by every geologist who has had opportunities for extensive observation in the field. Other plates, such as X., representing the Agassiz Ampitheatre in the Uinta Mountains, and XIX., in which a ridge of Archæan quartzite in the Humboldt Range is depicted, are wonderfully striking reproductions of the remarkable scenery of the district. In a series of plates illustrating the saline springs, we have the peculiar features of the great plains also presented to us in a very vivid manner.

Volume IV. consists of three parts. In the first of these a series of fossils from all the formations, from the Silurian to the Tertiary inclusive, are described by the late F. B. Meek. This part is illustrated by seventeen lithographic plates of great excellence. The second part is by James Hall and R. P. Whitfield, and describes certain new forms, from the Primordial to the Jurassic; it is illustrated by seven plates.

The third part of the volume is devoted to the description of the habits of the various species of birds met with during the several expeditions. It is from the pen of Mr. Robert Ridgway, the zoologist attached to the staff.

The United States Government is to be congratulated on having been able to secure such valuable illustrations of the natural history of their extensive and interesting territories as are contained in the splendid volumes before us, and the value of these contributions to science is greatly enhanced by the liberality with which they have been distributed among scientific workers and public libraries in every part of Europe. We shall look forward with much interest for the appearance of the other volumes of the series.

J. W. JUDD

OUR BOOK SHELF

Tent Work in Palestine. A Record of Discovery and Adventure. By Claude Regnier Conder, R.E., Officer in Command of the Survey Expedition. Two vols. (London: Bentley and Son, 1878.)

THIS is the first substantial result of the survey of Palestine, which has been going on for the last few years. It

is published by the Committee of the Palestine Exploration Fund, but is merely preliminary to the publication of the various detailed memoirs and the unprecedentedly minute map which are in preparation. It is mainly the narrative of Lieut. Conder's personal work and adventure, but besides its strong interest as a record of adventure in one of the most interesting countries in the world, it contains a vast amount of information and discussion concerning the many places so full of sacred associations to all Christian peoples. The work of the Survey was often pursued under considerable hardships, and occasionally at some risk, and more than one of the staff had to succumb during the progress of the work. It is evident that this most interesting of surveys has been executed with a minuteness and a care that leave little to be desired. The survey was actually commenced at the end of the year 1871. Capt. Stewart, the first officer in charge, had to come home on account of his health, and in July, 1872, Lieut. Conder took up the command, and completed four-fifths of the survey, the remaining fifth being carried out in 1877 by Lieut. Kitchener. The great map now extends over 6,000 square miles, from Dan to Beersheba, and from the Jordan to the Mediterranean Sea. This map is being prepared in twenty-six sheets; and an idea of its minuteness may be obtained from the fact that it will show tombs, caves, cisterns, wells, springs, rock-cut wine-presses, remarkable trees, and even the Roman milestones. Accompanying the map will be a memoir prepared by Lieut. Conder under the direction of Major Wilson and Mr. George Grove. It is hoped that all will be ready for publication in the course of a few months. This memoir will contain a vast collection of varied information gathered from many sources, and with the map will undoubtedly be of infinite service to students of the Biblical narratives. Lieut. Conder's work will amply repay careful study, and the many illustrations of places whose names are "familiar as household words," add greatly to its interest and value.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Discovery of Vulcan

A LETTER from the Astronomer-Royal, in NATURE, vol. xviii. p. 380, giving the exact position of θ Cancri on the day of the total solar eclipse, intimates that, as the position given by Prof. Watson of the intra-Mercurial planet discovered on that day agrees so closely with that star, it may have been the object discovered, I have thought it advisable to give the facts concerning it, in order to correct such an impression if it still exists. That he had a view of the planet as stated there is no doubt, for I myself saw it some four or five minutes later, using θ as a comparison star, and am able not only to corroborate the discovery, but to substantiate the position given by him. Its proximity to θ enabled me to estimate its position with great exactness, especially in declination. It may be well here to state that I was prevented from searching to the east of the sun, in consequence of forgetting to untie a string with which I had tied, to the eye end of the telescope, a long pole to prevent the wind from shaking it, the end resting on the ground not allowing the instrument to be moved to the eastward. It is undoubtedly to this circumstance, which at the time seemed untoward, that I owe the discovery of Vulcan. In my eagerness to discover this hypothetical planet I had decided to ignore nearly all of the phenomena attending the eclipse, and as, at the commencement of total phase, there was visible neither the chromosphere nor any protuberances—nothing, in fact, but the corona, I almost immediately began the sweeps for it; but my hampered telescope behaved badly, and no regularity in the sweeps could be maintained. Almost at once my eye caught two red stars about 3° south-west of the sun, with large, round, and equally bright discs, which I estimated as of the fifth magnitude, appearing (this was my thought at the time) about as bright in the telescope as the pole star does to the naked eye. I then carefully noted their distances from the sun and from each other, and the direction in which they pointed, &c., and recorded them in my memory, where, to my mind's eye, they are still distinctly visible. I then swept southward, not daring to venture far to the west for fear I should be unable to get back again, and soon came upon two stars resembling in every particular the former two I had found, and, sighting along the outside of the tube, was surprised to find I was viewing the same objects. Again I observed them with the utmost care, and then recommenced my sweeps in another direction, but I soon had them again, and for the third time in the field. This was also the last, as a small cloud hindered a final leave-taking just before the end of totality, as I had intended. I saw no other stars besides these two, not even δ, so close to the eastern limb of the sun. The distance between them was about 7′ or 8′.

By three careful estimates the two stars pointed exactly to the sun's centre. When it is considered that a deviation of not over 15″, in two objects so close, will cause them to point considerably to one side of the centre of the sun—three degrees away—it may be assumed that its declination was quite correctly estimated. Thus far all seems clear and free from doubt, but it is just here where the trouble begins, for, unfortunately, I could not tell which was the star and which the planet. Happily Prof. Watson comes to the rescue, and with his means of measuring, says "the planet was nearest the sun."

The Astronomer-Royal gives the place of θ, on that day, as in R.A. 8h. 24m. 40s., Decl. + 18° 30′ 20″. From this I deduce the position of the planet at 5h. 22m. Washington M.T. to have been in

R.A. 8h. 26m. 40s.
Decl. + 18° 30′ 25″.

This is a close approximation to that given by Prof. Watson. It is to be hoped that a comparison will determine the position in its orbit, whether it was approaching superior conjunction, as Watson thinks, or, as appears most reasonable to me, had just passed its inferior conjunction.

LEWIS SWIFT

Rochester, N. Y. September 4

The Respiration of Plants

I DESIRE, with your permission, to give publicity in the columns of NATURE to the results of some observations on the above subject, communicated by me to the Royal Society of Victoria on June 13. As the facts to be mentioned are not referred to in Sachs' "Text-book of Botany," in the dictionaries of chemistry of Watts and Wurtz, or in recent volumes of the *Journal of the Chemical Society* or the *Chemisches Centralblatt*, I presume that they are little, if at all, known to botanists. I have found, first about nine years ago, and have more systematically observed lately, that fresh sections of many fruits, such as the apple and pear, and other vegetable structures as the potato, turnip, &c., give the reactions considered to be characteristic of ozone, viz., causing separation of iodine from iodide of potassium, and turning tincture of guaiacum blue, the intensity of these reactions varying in different samples of the vegetable substances, but depending mainly on their comparative freshness. I have further found that the same structures contain a substance which acts as an *Ozonträger*, to use Schönbein's expression, a substance which transfers ozone from peroxide of hydrogen and similar articles. This is shown by the fact that if the guaiacum is not blued at all, or only to a slight extent, the blue colour becomes very marked when a drop of ethereal solution of peroxide of hydrogen is added. I infer from these observations (1) that the oxygen inhaled by living plants, and even by pulled fruits for a time, is ozonised or rendered active, probably by entering into loose combination, as is the case with the oxygen in the blood of animals; and (2) that it is probable, though not proved, that the ozone-transferring substance existing in almost every fresh vegetable structure is that with which it is loosely combined, as the oxygen in the blood is with the hæmoglobin of the red corpuscles, which is a very active *Ozonträger*. This element in plants is gradually destroyed as decay comes on, and ceases to perform its

ozone transferring function when the fruit, &c., containing it is cooked. It is not chlorophyll, as is shown by its situation, and it seems to be intimately associated with the vascular tissue. From analogy with the animal substances hæmoglobin, fibrin, myosin, &c., which have a similar action, it may be presumed to be proteinaceous, though I am unable to indicate its chemical and other characters more exactly. The interesting analogy between the respiratory functions of animals and plants indicated by these observations will, I hope, be considered a sufficient excuse if I ask you to insert this short summary of my paper which you will receive at the same time. JAMES JAMIESON

Melbourne, July 6

The Electro-Magnet a Receiving Telephone

THE experiment of Mr. F. G. Lloyd, described in NATURE, vol. xviii. p. 488, is simply a repetition of Page's original experiment, the basis of all telephony. The electro-magnets of ordinary relays and Morse apparatus make capital telephone receivers when their armatures are screwed up, and it is a common thing for operators at intermediate stations in America to enjoy the music that is being transmitted between the terminal stations during some telephonic display. I remember the station-master at Menlo Park telling me that the music sent from New York and received at Philadelphia was heard, much to his surprise and delight, all over his little wayside station. The effect is dependent upon the strength of the currents flowing. With a Riess' transmitter sending musical notes and voltaic currents it can be made very loud. With an Edison or a Hughes transmitter the effect is much less, and with a Bell transmitter it is almost, if not quite, inaudible.

Electro-magnets also can be used as transmitters, because their iron cores invariably contain some residual magnetism.

September 16 W. H. PREECE

Rayons de Crépuscule

IN your issue of July 25, Mr. Abbay, in writing of Ceylon, says that, as far as he is aware, the *rayons de crépuscule* are never seen in the low country.

Allow me to add the weight of my testimony to that of Mr. Pringle, given in NATURE of August 1.

During a residence of about five weeks here, the rays have been noticed from twelve to fifteen times, and I have been told by an old resident that their appearance is very common.

This country is the perfectly flat alluvial land and marsh bordering the Gulf of Mexico, whose shores are distant about eight miles from our camp.

On fully half of the occasions have the bands been traced from the sun, without break, to the point in the east, 180° from it. The rays are seen best when the sun is very near or below the horizon. On the evening of August 16, the display was exceptionally beautiful. The sun had set behind a bank of dense cumulus clouds, while the remainder of the heavens was covered with very faint cirro-cumulus clouds. A band of dark blue spread from the sun, and after widening to about 85° near the zenith, converged to a point either in, or slightly below the horizon. This was the cloud's shadow. In the south a line about 15° wide, and in the north, one about 80° wide, were lighted up, and shone with brilliant tints, varying from rose to orange. The lines between the deep blue of the shadow, and the lighter blue, mottled with the shining, closely packed cloud flecks, were sharply marked, as sharply, indeed, as the arch in the Aurora, which it called to mind. I have noticed the phenomenon several times in Maryland, in a gently rolling country, but nowhere have I seen it as often and as distinctly as here.

JULIUS KRUTTSCHNITT

Morgan's La. and Texas R. R. Camp, St. Mary Parish, Louisiana, August 24

"LES rayons de crépuscule" seem not to be of uncommon occurrence. They presented a most superb spectacle at this place last Sunday evening. The weather had been extremely warm all day and the mountains were seen through a thick haze. At sunset masses of dark clouds, fringed with gold, lay along the horizon to the west, while beyond them the sky was of a beautiful pink. As the sun sank lower many bands of pink appeared, stretching from the west entirely across to the east, appearing broader and paler, of course, near the zenith. They changed gradually in width, position, and number for perhaps half-an-hour, and then disappeared. Their changeableness indicated that they were due to clouds near the horizon.

Sing Sing-on-the-Hudson, September 3 H. S. CARHART

The Microphone

THE form of microphone described by Mr. Gerald B. Francis (NATURE, vol. xviii. p. 383) is easily made and very efficient. It not only did for me all its inventor promised, but with a common tumbler inverted over it upon the sounding-board so as to prevent direct impact of sound waves upon the ball, it became a powerful transmitter of the human voice. I conversed easily and satisfactorily with a friend a half-mile from my end of the wire. The exact contact of the lower wire with the ball was effected by a screw with a very fine thread passing through without touching the lower block or cup. The voice must be kept low to prevent bounding of the ball so as to break contact. Bell telephones were used as receiving instruments, the batteries being Hill and Calland gravity batteries used extensively in this country upon telegraph lines. These batteries agree exactly in every respect with the one used by Prof. Hughes in his interesting experiments excepting the clay, which is not necessary, and must be a great inconvenience in a permanent arrangement. S. T. BARRETT

Port Jervis, New York, August 30

A White Swallow.—Albinism in Birds

BY the side of a steep sand-cliff overhanging a stream—the Cambeck, in Cumberland—I lately saw, on a glorious summer afternoon, a white swallow flying about with many other birds of the same species. A most beautiful bird it was; perfectly snow white, with perhaps a slight tinge of blueish grey near the roots of the tail-feathers. In size it seemed to be rather smaller than the swallows around it; but in its flight and pursuit of insects there was no noticeable difference. From my position at the top of the cliff I could often see the bird within a very few yards of me.

Like the grouse of which Sir Joseph Fayrer writes in NATURE (vol. xviii. p. 518), this white swallow is, I believe, of considerable rarity. I have been able to hear of only one, seen many years ago near Repton, in Derbyshire; and in numerous works on British and other birds which I have consulted, I cannot find any very precise mention of a white variety. Magillivray remarks of the *Hirundo riparia*, the species to which the bird I saw belonged, that "individuals of a whitish colour are said to occur, but I have never met with any remarkable deviations from the ordinary appearance." Yarrel speaks of a white variety of the common swallow as not uncommon; while of the same swallow, or *Hirundo domestica*, several varieties are recorded by Buffon, and among them the white, there being "no country in Europe where these have not been seen, from the Archipelago to Prussia."

Able to catch flies on a cloudless summer day, this white swallow can, I hardly think, have been an albino, although I had no opportunity of such close inspection as Sir Joseph Fayrer had of the grouse he shot near Dunrobin. Albinism in birds must, I should imagine, be altogether unknown or unobserved, for I can nowhere meet with any account of it. Undoubted albinos are sometimes spoken of as "white varieties"—an albino monkey is, or lately was, so labelled at the Zoological Gardens; and it is possible that this very general term may include some cases of albinism, even among birds.

HERBERT W. PAGE

New Cavendish Street, W., September 16

The Hearing of Insects

I AM not aware if it is generally known that there is a wasp in South America which seems to present undoubted evidence of a faculty to hear, or it may be to feel, and distinguish certain vibrations of sound.

The wasp is a common one on the Guayaquil River; a large slender black species, much feared on account of the virulence of his sting, which not unfrequently produces fever. I, myself, though little susceptible to the bites of mosquitoes or flies, and the stings of scorpions, &c., when once stung on the finger by

a "cubo," as this wasp is called in Ecuador, had my whole hand and forearm considerably swollen for a couple of days.

A common spot chosen by the cubo for his nest is high up on a palm stem at the river-side, and natives are well aware of the danger of uttering any loud cry when in its proximity. I have frequently experimented by giving a shrill whistle—his particular abhorrence—from a safe distance, with the invariable result of all the wasps flying in confusion from the nest in manifest anger.

It is said that there is a wasp in New Granada in whose proximity one dare not speak, but for this I cannot vouch, and very possibly this may be an exaggerated account of the cubo. It would certainly be a dangerous experiment to speak loud when very close to a cubo's nest, even on the Guayas, and a shrill voice would be sure to irritate the wasp. ALFRED SIMSON

4, Fairlie Place, Calcutta, August 20

Circulating Decimals

IN NATURE, vol. xviii. p. 291, is an extract from a letter by Mr. R. Chartres, in which is given a remarkable property of certain circulating decimals. Mr. Chartres only refers to fractions of the form $\frac{1}{nr - 1}$ (where $r = 10$); but I have since found that a somewhat similar property belongs to other fractions when expressed as circulating decimals.

For instance, $\frac{1}{7} = \dot{1}4285\dot{7}$; here we observe that the last figure of the circulator is the same as the denominator of the vulgar fraction; moreover, by multiplying the 7 by 5, we get the next figure, and this by 5, taking in the remainder, gives the third from the end, and so on till we get the whole recurring decimal.

So far this is somewhat similar to Mr. Chartres' discovery, but now observe the curious property in the following fractions:—

$$\frac{1}{7} = \cdot\dot{1}4285\dot{7}$$
$$\frac{1}{17} = \cdot\dot{0}58823529411764\dot{7}$$
$$\frac{1}{27} = \cdot\dot{0}3\dot{7}$$
$$\frac{1}{37} = \cdot\dot{0}2\dot{7}$$

In each case the last figure of the circulator is the same as the unit figure of the denominator of the vulgar fraction. Now the multipliers which give the remaining figures are, for the first fraction, 5; for the second, (5 + 7); for the third, (5 + 14); for the fourth, (5 + 21); and generally to convert a vulgar fraction of the form $\frac{1}{nr + 7}$ ($r = 10$) to a circulator, we put down the last figure 7 and multiply successively by $7n + 5$.

For fractions of the form $\frac{1}{nr + 3}$ the multiplier is $3n + 1$, and the last figure 3.

For fractions of the form $\frac{1}{nr + 1}$ the last figure in the circulator is 9, and the multiplier is $9n + 1$.

Of course the last figure must be that one which, multiplied into the unit of the denominator, and the unit of the result being subtracted from 10, leaves a remainder of 1.

These rules added to that of Mr. Chartres include every case of fractions which, when reduced to decimals, circulate.

Littlehampton, Sussex EDMUND P. TOY

Autophyllogeny

WITH reference to the note in NATURE on autophyllogeny in a leaf of *Papaya vulgaris*, I wish to place on record another freak which I have more than once noticed in the Papeea or Papeeta, as we call *Carica papaya* out here. The plant is diœcious, the female being stumpy and her flowers and fruit sessile; the male plant, on the other hand, is tall and graceful, and the flowers depend from long stalks. The freak I have above alluded to *consists in the presence of distinct and well-formed fruit on the male plant*, and I regret I was unable, on both occasions, to secure the anomalous production for examination. R. F. HUTCHINSON, M.D.,

Mussooree, August 19 Surgeon-Major, Bengal Army

The Sea-Serpent Explained

THE letters of Dr. Drew and others remind me of what I witnessed at Sandgate twenty-four years ago. I was staying at a cottage on an elevation which commanded an extensive sea-view. One morning my attention was called to a large, dark, undulating body, which moved rapidly through the sea. As it was some way out from shore, I naturally concluded it to be of enormous length. I lost no time in making inquiries as to the nature of this phenomenon, and was so fortunate as to discover a fisherman who had witnessed it. He told me it was a flight of petrels. But for this I should certainly have believed that I had seen the Great Unknown. I have often seen a similar phenomenon, but nothing nearly so striking as this.

Valentines, Ilford, September 16 C. M. INGLEBY

RECENT PROGRESS OF SELENOGRAPHY

THE most active period in the study of selenography during late years is comprised between two epochs, that of the announcement of a change in the crater *Linné* in the year 1866 by Dr. Schmidt, Director of the Observatory at Athens, and that of the announcement of a new crater north-west of Hyginus, by Dr. Klein, of Cologne, in the year 1877. The years elapsed between the two events above-mentioned have been characterised more or less by the manifestation of considerable interest in lunar studies, of which the projection of a map of the moon 200 inches in diameter, to have been constructed under the auspices of the British Association for the Advancement of Science was the first indication. Of this map, four sections embracing an area of 100 square degrees of lunar latitude and longitude have been published, containing all the formations known in 1866–1868 to exist on this area, each of which is separately catalogued. Three of these sections, with catalogues, were published in the *Reports of the British Association* for 1866 and 1868; the fourth was published by the aid of a private subscription, in 1870. We are not aware that much use has been made of these areas and catalogues in endeavouring to ascertain if the 433 objects chronicled in them retain the characteristics they possessed in the above-mentioned years. It was a part of the duty of the Committee appointed by the Association to receive the reports of volunteer observers who undertook to examine the objects in certain subzones at stated intervals, which resulted in the addition of several new objects to those originally published, but nothing has been effected in this direction since the Committee was not reappointed in 1868.

In February, 1869, a map of the Grey Plain, the *Mare Serenitatis*, was published in the *Astronomical Register* for that month, by Messrs. Joynson and Williams. It contained several new objects not on former maps, and was followed in the course of a few months by a map and monogram of the same region. The map was divided into the British Association areas, and it contained 277 objects, each being distinguished by a British Association symbol; they were briefly described in a table of the areas in which any part of the Mare was found.

The four areas of the British Association map on a scale of 200 inches to the moon's diameter accompanied by a monogram of the formation *Hipparchus* on a scale of 100 inches, with that of the *Mare Serenitatis* form a collection of maps, which, with the descriptions of 710 separate objects embody the conditions of those portions of the moon's surface which were telescopically or photographically examined between 1866 and 1870. As placing in the hands of the student a body of facts especially suitable for future reference, these maps and monograms will furnish most important information on the condition of objects recorded on or in them during the four years above mentioned. It is in the future the real progress of the past is more truly measured.

In the years 1871 and 1872, Reports of a Committee appointed for discussing observations of lunar objects suspected of change, was read before the British Association for the Advancement of Science, the principal results being the discovery of about thirty-six spots and

crater cones upon the floor of the walled plain *Plato*, which were very variable in visibility; also of several streaks which were not only variable in visibility but also in form; the most usual shape that was observed was that of a trident on the south-west part of the floor. There appeared to be a close connection between the spots and streaks, but its nature was not mentioned in the reports further than a surmise that the appearance of certain streaks appeared to be coincident with an increased visibility of certain spots. Another result was a brightening of the north-west floor, for a time only, as well as a brightening of several of the streaks, but perhaps the most important was that of a *darkening* of the darker parts of the floor as the sun rose higher above its horizon. This result has been most strenuously controverted by a popular writer, who alleges that the effect observed was due only to contrast. Be this as it may, the Report for 1872 is explicit as to a gradual darkening with increased solar altitude; at any rate the same writer bears testimony to the exceptional care with which the floor was scrutinised.

In the year 1876 (May) Neison's "Moon and the Condition and Configuration of its Surface," appeared. It marked an era in selenographical research, the English student having within his reach, for the first time, a description of the principal formations on the moon's surface in his own language. This work, of 576 pages, includes a map drawn in twenty-two sections, the principal formations on each being fully described in the body of the work. Besides the purely topographical part, the author has given five introductory chapters in which he has treated of the physical condition of the surface, the general characteristics of the lunar formations and the variations that have from time to time been detected on the surface. He has also given a most lucid chapter on the history both of the progress of mathematical investigation and selenographical research as regards the moon.

The most important recent event in the progress of selenography was the discovery, on May 19, 1877, by Dr. Klein, of a dark spot which he described as "a great black crater full of shadow without a wall north-west of Hyginus," (NATURE, vol. xviii. p. 197). It was first announced by him in his *Wochenschrift für Astronomie* for March, 27, 1878, and has since been seen by several observers who have generally confirmed the appearances mentioned by him, and also agree in their testimony that no such spot existed formerly in the region in which it was seen. In the celebrated case of Linné the information in 1866-67, as regards the *former* condition of the surface of the moon was so imperfect that it was considered by astronomers that "no change in Linné could have taken place, but that Lohrmann, Beer and Mädler, and Schmidt must have been mistaken." [1] A considerable controversy on this subject occurred during 1867-68, but nearly six months have elapsed since Dr. Klein's announcement, and we have not heard of his discovery having been seriously questioned. It is true that a certain white spot, alleged to have been found on photograms taken within the last fourteen years, has been regarded as indicative of the existence of Klein's crater during those years, and from the probability of its locality being that of Klein's crater, the conclusion has been drawn that the latter is not new. Admitting that a somewhat similar spot existed thirteen years ago, the real nature of which we are unacquainted with, and which may have disappeared in the interim, we are quite as much in the dark as to the real nature of the change ascertained to have taken place by Klein, as to the former existence of the spot discovered by him; both await further elucidation.

The recent publication of Schmidt's large map of the moon in twenty-five sections indicates the latest era of progress. If selenographers apply themselves energetically to examine it, the study of the moon's surface will make great progress; but for this purpose much self-denying labour is indispensable, as the map contains as many as 32,856 craters, exceeding those recorded by Mädler by 25,121. It is important for the true progress of selenography that all of these newly-recorded craters should be observed in such a manner as to leave no doubt of their existence. To effect this each observer should keep his record of observations in the form of a catalogue. In the earlier part of this article we have alluded to the plan adopted by the British Association, viz., of cataloguing every object inserted or to be inserted on its map, and we have also alluded to those publications which contain the descriptions of 710 objects. We believe that a comparison of those publications with Schmidt's map is in progress, and that some of the objects in them are not in Schmidt's map. It is, therefore, certain that, indefatigable as Schmidt has been, if we regard his map as perfect we shall make a great mistake. May we rather endeavour to add to the already large number of objects which he has chronicled.

[1] Neison. "The Moon, and the Condition and Configuration of its Surface," p. 125.

HYDRO-INCUBATION [1]

WE wish that Mr. Christy had used a less barbarous term for his useful apparatus: that, however, is of little consequence to practical men.

At a time when our native farm-yard and dairy produce only supplies about two-thirds of what London and our secondary and third-rate towns need, anything that will assist the unready Saxon in so unsatisfactory a state of things ought to be received with gratitude.

Fowls, in a dietetic point of view, are, we are satisfied, certainly of much more importance than is ordinarily supposed. Let any thoughtful medical man in general practice think what a comfort it would be to him if well-fed young fowls were available for the poorer among his patients, and he will agree with us that such an apparatus as Mr. Christy's may become a great boon. That the masses of the people should be able, in time of illness, to purchase useful wine, the more delicate kinds of fish (such as whiting and soles), fresh eggs, and succulent and tender poultry—these are things desirable to a degree only known to those who are familiar with the treatment of diseases in the homes of the common people. Many a kindly Family Doctor passes from house to house heavy-hearted as well as (too often) overworked; the sad answer to his advice as to regimen being, again and again—"It is easy, Sir, for you to prescribe, but how can we afford these luxuries."

The old remark as to the value of an improvement in the grazing department of the farm, that should "make two blades of grass grow where only one grew before," is now more than ever pertinent. Let our readers look at the Registrar-General's returns for London only, especially in a very healthy season, such as last year, and he will see that the number of the births weekly, as compared to the deaths, is such as to add about *two thousand* hungry mouths to the population in three weeks. Then from all the provinces men and women are pouring into London and the large towns, where they need in the closer air more and better food than would have sufficed them in the country. Therefore we are glad to see energetic merchants like Mr. Christy, bestirring themselves to see what new cattle-food can be found from other climates, and how our rural people can be stimulated and helped to grow and develop for their own benefit and for the benefit of others, food of many kinds that shall be as "manna" to thousands and myriads of hungry people.

But there is another, and much more limited, sphere in which such an apparatus as the "hydro-incubator," will

[1] "Hydro-Incubation in Theory and Practice." By Thomas Christy, F.L.S. Second Edition. (London: 1878.

be of the greatest value. The readers of NATURE will appreciate anything that helps the scientific worker. Now, at last, we are going to the root of things in biology, and only the embryologist knows fully what a boon an egg-hatcher, convenient and easy to work, would be. The eggs of the hen will be wanted in their various stages of incubation, as long as there are workers in these departments, but many other sorts of oviparous animals have to be worked out in all their stages besides the common fowl. Snakes, lizards, tortoises, crocodiles, all these are rivals of the bird in their embryology, and of many kinds the eggs could be procured and their embryos developed if the worker had some such apparatus as Mr. Christy is bringing out. We want, not merely the general embryology of these ovipara, such as is so excellently illustrated and described in Messrs. Foster and Balfour's work, but the special development of any important organ ought to be traced in all its stages through not one, but many types of the vertebrata: through *all* the principal kinds indeed.

Some of us are trying to do this in the skeletal structures; the nervous system, still more important, wants an army of workers, then there are the respiratory, digestive, excretory, and generative organs, all these want a complete history in all their stages, not in one type merely, but in scores of types. We therefore wish well to all energetic and enterprising men who put it into our power to work on a wider scale; such means and appliances as can be brought out by men too restless for close and patient study, may be of infinite service to the close and patient student, who is too dreamy and abstracted to invent for himself.

W. K. P.

NOTES ON SOME NATAL PLANTS

GROWING plentifully among the grass on the coast hills of Natal is a small blue flower belonging to the Rubiaceæ. In this plant, generally speaking, there are two forms only, in one of which the five stamens are exserted considerably beyond the tube of the rotate corolla, and the stigma is included in the tube; in this form the tube is almost devoid of hairs. In the other common form the position of these essential organs is reversed, the stigma protruding to about the same extent that the stamens do in the first mentioned, and the stamens being included; here, however, the upper part of the corolla tube is *thickly* covered with downy hairs—of course this is an ordinary dimorphic plant. But I find lately a third form of the same species (only, however, rarely) in which both stamens and stigma are exserted and are of the same length, so that here self-fertilisation must take place, as the stamens and stigma touch at the time the former dehisce. I do not think this can be termed a cleistogamic form, as, although rather smaller and lighter in colour than the others, the difference is only trifling. The hairs which cover the corolla-tube in the form with included stamens serve to keep the pollen collected near the upper part of the tube, as, if it fell to the base it would not be so easily transferred by the proboscis of an insect as when lightly held by the hairs through which the insect must make way. As these hairs would be for this purpose useless when the stamens are exserted they do not occur in the other form.

I notice the same arrangement of hairs in another dimorphic plant belonging, I think, to the same order, which grows on the marshy flats near the sea. I have found on the coast lands here four other plants, in which cross-fertilisation is secured by dimorphism, one of them being a monocotyledonous plant.

There is a species of *Polygonum* which climbs in the bush which well illustrates another plan ensuring cross-fertilisation; while the flower is young and the perianth still closed, enveloping the immature stamens, the three branching stigmas protrude from between the segments in a fit state to receive the pollen. If (as is usual) the ripe stigmas were only exposed when the flower opens, although the evils of self-fertilisation would of course be avoided by the plant being proterogynous, still, as it is wind-fertilised, the perianth and stamens would be in the way of any stray pollen-grains reaching the stigmas; while as it is, nothing interposes between pollen and stigmas.

Lately I have found a curious aberration of form in *Tecoma capense* growing here. It is very common in the bush, forming great beds of bright colour, and normally has a scarlet trumpet-shaped corolla, with one rudimentary and four perfect stamens. I found, however, three or four plants growing within a short distance of each other, in which there were eight perfect stamens; they seemed, however, to have been formed at the expense of the corolla, for there was only one segment coloured at all, the remainder being colourless and small. The ovary seemed in several cases to have been fertilised. The ordinary form of this plant, although individually so brightly coloured, growing in large numbers and secreting much nectar, is seldom or never visited by Lepidoptera. It is, however, frequented by honeysuckers and small bees in numbers. All through the day you can hear the shrill chirp of the small bright honeysucker among the blossoms. The immediate reason why butterflies and moths do not visit it I cannot give; but the stamens and stigma (which are beneath the large upper segment of the corolla) are long, and so high above the opening of the corolla-tube that those insects, in visiting the flower for its nectar, would not be at all certain to touch either, and so in comparison to the honeysucker and small bees would be of little benefit to the plant; for when the former of these visits the flower the feathers of his head are just of the height to brush off the pollen, and the latter in collecting the pollen is equally certain to distribute it, as the bifid stigma is about the same length or only slightly longer than the stamens. Can the nectar have been modified to suit the taste of the useful honeysucker without reference to the useless butterfly?

Natal, June 27 M. S. EVANS

PHYSICS IN PHOTOGRAPHY [1]

III.

THESE last experiments were remarkable in another point of view, as they opened out the question as to whether the salts of silver might not prove sensitive to rays to which they had been supposed hitherto to be insensitive. Silver iodide, for instance, when exposed to the spectrum in a solution of potassium sulphite proved sensitive as far as "*a*" of the spectrum instead of stopping short at the point indicated in Fig. 2 (p. 529); and silver bromide in the molecular grouping which absorbed the red proved sensitive to a wave-length of somewhere near 11,000, whereas in its normal state 9,600 was its limit.

Similarly silver chloride proved sensitive to an extent which presumably may be increased till it is equal to that of the bromide. In both these instances we have a proof that the compound was sensitive to these abnormal rays, and that the image formed by those rays was destroyed as soon as formed by their oxidising action giving an undevelopable form of salt. It may be remarked that by exposing films in reducing solutions such as ferrous sulphate, and pyrogallic acid rendered very slightly alkaline, that an image can be developed as fast as it is formed.

The natural outcome of the experiments on the oxidation of the photographic image just narrated is that it should lead to the solution of the problem of photography in natural colours, such as that of Becquerel, Niepce de St. Victor, and others. In the fourth edition

[1] Continued from p. 531.

of Hunt's "Handbook of Photography," we read, at p. 161, "Niepce de St. Victor has made many experiments to produce the colours upon salts of silver and copper spread upon paper, but without success; the metallic plate appears absolutely necessary, and the purer the silver the more perfect and intense is the impression." The following is recommended as the most effectual mode of manipulating:—"The plate is highly polished with tripoli powder and ammonia; being perfectly cleaned, it is connected with the battery and plunged into the bath prepared in any of the ways stated. [The baths were made from ferric chloride, cupric chloride, hydrochloric acid, &c.] It is allowed to remain in the bath for some minutes, taken from it, washed in a large quantity of water, and dried over a spirit-lamp. The surface thus produced is of a dull neutral tint, often almost black; the sensibility of the plate appears to be increased *by the action of heat*, and, when brought by the spirit lamp to the cerise red, it is in its most sensitive state."

"The sensibility, however, of the plates is low—*two or three hours* being required to produce a decided effect in the camera obscura. . . . These, when I first saw them, were perfectly coloured in correspondence with the drawings of which they were copies, but the colours soon faded, and it does not appear as yet that any successful mode of fixing the colours has been discovered." The coloured spectra which Becquerel photographed were produced in a somewhat similar way, the variation from which need scarcely be repeated.

In Hunt's work we also find that natural colouration of photographs was found to be possible by one or two other processes, but that the above gave the most satisfactory results. Mr. Simpson also noticed when using an emulsion of silver chloride and after exposing the film to white light so as to tint the surface with a lavender colour, that he was able to reproduce on the film the tint of different coloured glasses to which such a surface might be exposed.

It will be noticed that the coloured spectra were produced on a dark compound of silver which gradually responded the colour falling on it. We have first a case of total or nearly total absorption of all the rays, and a subsequent production of compounds of varying tints. In order to produce any variations of colour it is only necessary that we should have at the most three molecular groupings, one of which should absorb the blue and green, another the green and red, and the last the red and blue. Whether the number of groupings may be reduced to two is a question for future consideration. In Lockyer's note read before the Royal Society on June 11, 1874, "On the Evidence of Variation in Molecular Structure," we find statements which might have been conceived to be almost too bold at the time when they were made, but which subsequent investigations seem to prove to be exact. In this note he refers to definite molecular groupings of compounds and the absorption caused by them, and indicates that we may have a group which will absorb at the blue end and another which will absorb the red end of the visible spectrum. It has already been shown that the silver bromide can be reduced to two groupings, one absorbing the blue and the other the red, and it is somewhat remarkable that, by applying pressure to the latter molecular grouping, it is gradually resolved into the former grouping, and passes through all tints of spectrum between the blue and the red. It must be remembered that these colours are not the colours of thin plates, but are totally independent of the thickness of the film so long as light can penetrate through it. It is not too much to assume that if silver bromide can be made to group itself into these two states, that the sub-bromide when oxidized should also assume a similar molecular condition. With this compound in a state which practically absorbs all rays, it is easy to imagine that particular sets of vibrations may cause it to resolve itself into groupings which answer to them. We have, in fact, the inverse of the reduction of the silver bromide by different portions of the spectrum. It is found that one molecular grouping can be reduced by a whole series of vibrations; thus the blue absorbing molecular group is altered by all the radiations from the ultra violet to the yellow, and the red absorbing molecular group by the radiations from the ultra-red to the green. If there were a green absorbing molecular group, of which there is a strong suspicion of the existence, it would probably be altered by radiations from the blue to the orange. If, then, one silver compound can exist in two or three states of molecular grouping, it is quite within the range of reason that the oxidised compound should exist in the same three groupings. The black compound to which we have already referred, in fact, does arrange itself thus, probably by a re-arrangement of molecules, as formed when it absorbs oxygen. If a plate be prepared in a similar manner to that described above, and if it be exposed in an oxidising medium, these groupings are attained rapidly, a few minutes sufficing where previously hours were required. The images thus formed, however, appear not to be unchangeable, as exposure to white light, or to any colour except that in which the re-arrangement takes place causes the colours to fade. The feat of producing *permanent* photographs in natural colours is as yet unsolved, but it may not be so far distant as may be imagined. In order to obtain them it is necessary that a method should be found by which the molecular groupings of metallic silver can be formed in either of the two (or three) states already described. As is well known, the absorption by metallic silver in a thin film takes place entirely in the red end of the spectrum, but it is a fact well known to photographers at large, that in certain processes it is perfectly feasible to obtain silver in which the transmitted light is of a pink red colour, whilst tints varying from indigo, passing through olive green to rich brown are familiar. In order to obtain permanent photographs in natural colours, the object to be sought is a method by which the sensitive silver compound may be reduced by the red rays to a molecular grouping, which on development (probably by the alkaline method) shall be grouped into the red transmitting molecular grouping, and so on. When this is discovered, the leap between monochromatic pictures, and chromatic, will have been taken, and the once apparent improbability have become more than a possibility.

We have finally to return to the subject of photography with the light of those rays which are usually inactive upon sensitive salts, and at which we have already glanced.

To Dr. H. Vogel, of Berlin, is undoubtedly due the new interest which has been taken in this branch of photography. Towards the end of the year 1873 he announced that he had discovered a method of making the non-actinic rays in certain circumstances actinic. We quote his own words[1]:—"I have found that bodies which absorb the yellow ray of the spectrum make bromide of silver sensitive to the yellow ray. In like manner I find bodies which absorb the red ray of the spectrum make bromide of silver sensitive to the red rays. For example, by the addition of *corallin*—which absorbs the yellow ray—to a bromide of silver film, it becomes as sensitive to the yellow ray as to the blue ray." In articles which he published at various times he enlarged on this idea, some of his most striking experiments being conducted with aniline dyes of various kinds. He and Waterhouse have shown that a silver bromide film becomes sensitive to the part of the spectrum which certain of those dyes absorb, whether the absorption be due to a compound formed between the dye and silver, or to aqueous or alcoholic solutions. This at once opened

[1] *Photographic News*, December 5, 1873.

out a large field for inquiry, and made research in this direction doubly interesting owing to the fact that apparently certain physical laws would have to be modified if Vogel's theory were correct. He divided the action of the substances so added into two, the dye he called an optical sensitiser for that particular part of the spectrum which it absorbed, whilst bodies which absorbed the halogen (thrown off by the reduction of the molecule) he called a chemical sensitiser, and a combination of both properties in a dye made the film sensitive to the absorbed rays. The theory of the optical sensitiser seemed to clash with the received notion of molecular motion, but before analysing the results the accompanying figure should be studied, which is taken from Vogel's work on Photography (Fig. 5).

Let us take one or two examples from the above figures and see whether they agree with Vogel's assumption. We will take VIII. as a standard of comparison, being the effect on unstained bromide, and this will be fair (though it does not take the form given, shown in Fig. 2, p. 529), as it is presumed that this sample of bromide was worked with throughout. Comparing say IV. with VIII. we see that in the blue the sensitivenesses, as shown by the ordinate of the curved line, are very similar, but that the action is got in the yellow. In examining cyanine blue, the dye used, we find that the absorption takes place just at that part of the spectrum. Similarly examining V. and VI. we arrive at the same results, and in fact the absorption of the rays invariably corresponds with the photographic action.

It will be seen then that without doubt the principle Vogel contends for might explain the phenomena. He, however, found that if the silver bromide film had been prepared with an excess of bromide, that the actions indicated did not take place. This seemed to indicate a weak spot in the theory, and it pointed at first sight to the idea that it was necessary to form a coloured compound of the dye with silver, in order to render it sensitive. In the majority of cases this still seems to be, if not a necessity, yet a cause of increase of sensitiveness to the region of the spectrum absorbed. Vogel, however, shows that, if the silver bromide film, prepared with an excess of bromide, be washed, and be then treated with a dye, and a chemical sensitiser, such as tannin, that the same action takes place. The theory of a silver compound in this case must evidently be abandoned, and would point to

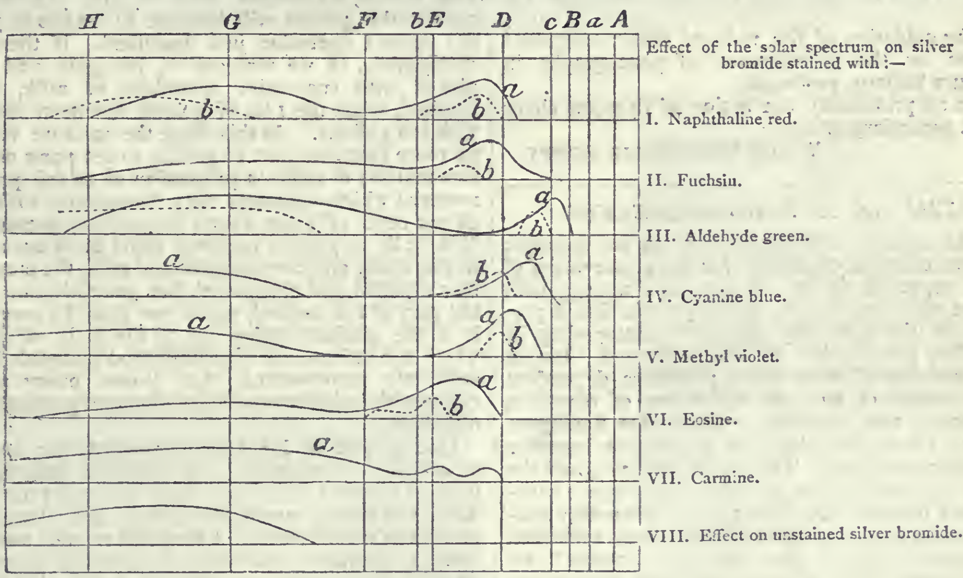


FIG. 5.

The curves marked *a* show longer exposure than those marked *b*.

the correctness of Vogel's theory, did not other experiments in a certain degree offer an explanation more accordant with our preconceived ideas. Let the dye be mixed with *plain* collodion and a film of it be exposed to the spectrum, it will soon be visibly evident that there is a marked effect produced by the rays absorbed. Thus cyanine blue will be found bleached in the region near D and below it. If over the dyed film of collodion thus exposed, a film of silver bromide in collodion be poured in the dark, and the alkaline developer be applied gradually, a silver image of the altered portion of the dyed collodion film will make its appearance, although the film of silver bromide has received no impression by light. The explanation of this remarkable phenomenon is to be found in the theory of alkaline development already given in these articles. The reduced dye acts as a nucleus on which the metallic silver will first adhere, and this first reduction of metallic silver determines the position which the further reduced silver shall occupy, and thus the image is built up. When there is an excess of soluble bromide in the film the developing action will be retarded. This series of experiments seems, then, to indicate that there is no need for optical sensitisers to alter the oscillation of the molecules or molecular groups, but that, in some instances, the same theory may be applied to the action of dyes as may be applied to the deposition of metallic silver on a glass plate which has not been freed from "dirt," a disagreeable phase which is well known to photographers at large. The explanation of the diagram is, therefore, not hard to understand when viewed in this light. The theory of rendering the silver salt sensitive to the red has already been explained, and the same explanation is naturally applicable to those dyes which, when brought in contact with silver, form a definite compound with it.

There are many interesting physical researches which spring out of these various experiments, amongst which may be mentioned a method of determining the size of atoms and their arrangement in the molecule, and last and not least, the production of permanent photographs painted in natural colours by light itself. The attempts made of late to form photographs in proper colours by taking distinct negatives through blue, through green, and

through red media and then printing positives from such, and finally obtaining red, green, and blue prints and superimposing them, is not a step in a scientific direction, since it is utterly impossible to secure monochromatic colours which are pure enough to give the truth of nature. Such efforts, though they may be commercially valuable, yet are not to be followed with too much zeal by scientific photographers.

We may now axiomise the results we have indicated:—

1. That the undeveloped photographic image on a silver compound is formed by the reduction of that compound.

2. That the compound may exist in two (or three) molecular groupings.

3. That the compound can only be sensitive to the rays which it absorbs.

4. That the reduced silver compound may be rendered incapable of development by combining with oxygen.

5. That light of every refrangibility may cause an acceleration of oxidation provided the compound acted on absorbs such light.

6. That the oxidation of the compound reduced by any particular ray may be as rapid as the reduction, and thus to give a false idea of their limit of sensitiveness to the spectrum.

7. That the oxidation of the reduced silver compound may account for the phenomenon of photographs in natural colours hitherto produced.

8. That in all probability the action of dyes on silver bromide is a secondary one.

W. DE WIVELESLIE ABNEY

GELATIN AS A FOOD-PRESERVER

REMOVAL of water and exclusion of air are amongst the most effective conditions for the preservation of animal and vegetable foods. If you coat an egg with collodion you may keep it a year, and yet will find it perfectly sound at the last. By dipping a mutton-chop in melted paraffin, putrefaction will be prevented. But in both these examples of preservative processes, dependent upon the exclusion of air, you make use of materials which are costly and uneatable. There are analogous drawbacks to all similar plans for preventing injurious changes in articles of food. The tinning method, and the method of simple desiccation in warm dry air, are satisfactory in their results; but the range of alimentary substances amenable to such treatment is not very extensive. In Dr. Campbell Morfit's new "Gelatin Process" we seem to see several points of superiority over most of the older plans for attaining the same end. It is true that chemists have not been in the habit of looking upon gelatin (or indeed any other similar complex nitrogenous body) as likely to prevent or arrest decay. On the contrary, few solutions afford a more suitable *nidus* for the development of fungoid germs than a liquid containing gelatin. But the experience of a good many months tends to show that food-preparations containing gelatin, if once dried so as not to contain more than 10 or 12 per cent. of moisture, do not become mouldy even when exposed to warm and moist air. A large number of Dr. Morfit's experimental mixtures have been so exposed for some weeks, lying on my office table: yet they have not suffered any decided deterioration. They comprise many perishable foods, such as cabbage, tomato, milk, and meat. Though not of equal merit as specimens of the gelatin process, all are edible, and some positively palatable. Further experiment will doubtless enable the inventor to improve his process by modifying it still further, so as to suit a greater variety of vegetable and animal foods.

Perhaps the best way of explaining the nature of Dr. Morfit's invention will be to take as an illustrative example the case of milk. The mere drying-up of milk has been tried with but moderate success—the resulting powder becoming quickly rancid on exposure to the air. The preserved or condensed milk now in such extensive use is in many respects a satisfactory and convenient preparation, but it is mawkishly sweet, containing more than one-fourth its weight of added cane sugar. Moreover, in consequence of this addition, the proportion of nitrogenous or flesh-forming substances in it has been seriously lowered. Now the substitution of gelatin for cane sugar in preserving milk meets both these objections to ordinary condensed milk. The milk preserves its natural and moderate degree of sweetness, while the gelatin, even if its own value as a nitrogenous nutrient be not considered, certainly does not *lower* the proportion of flesh-formers to heat-givers in the product.

In order to apply his process to the preservation of milk, Dr. Morfit directs us to dissolve 1 lb. of gelatin in 1 gallon of milk at a temperature of 130° to 140° Fahr., and then to allow the solution to set into a jelly; this is then cut into slices and dried. By employing the product of this first operation in lieu of fresh gelatin, for gelatinising a second gallon of milk, a jelly is obtained in which the milk-solids are just doubled in amount. As a gallon of milk contains about 6,400 grains of these solid nutrients, casein, milk-sugar, milk-fat, and phosphates, their ratio to the gelatin will become as 12,800 to 7,000 after the second operation just described. If then the dried *milk-jujube*, as we may call it, be again and again employed with successive quantities of milk, a limit is reached, when the 1 lb. of gelatin has been incorporated with ten gallons. At this stage the mixture will contain no more than one part of gelatin to ten parts of the nutritive matters of milk—a proportion of added preservative material which contrasts very favourably with the 25 to 28 per cent. of sugar found in ordinary preserved milk. If the 1 lb. of gelatin required could be at once dissolved in the whole eight or ten gallons of milk, the process would be simplified and cheapened, but gelatinisation, an essential part of the method, could not then be secured. For it is the gradual drying up of the slabs of jelly, with which the animal and vegetable food-materials have been uniformly incorporated, that leaves every particle of changeable substance with an adequate protective coating of gelatin.

One at least of Dr. Morfit's preparations has become an article of commerce. He dissolves gelatin in lime-juice at a gentle heat, and after removing much of the water and adding sugar, incorporates the mixture with the powder of navy-biscuit. Pressed in moulds and carefully dried, a granular acidulous and agreeable biscuit is produced, which should combine a considerable alimentary value with the anti-scorbutic properties of lime-juice. On analysing the lime-juice jujube, the basis of these biscuits, I find about 8 per cent. of water, 8 of gelatine, 5 of free citric acid, much sugar, and less than 1 (0·7) per cent. of mineral matter or ash. This proportion of gelatin is rather high when compared with the free citric acid, the characteristic ingredient of lime-juice; but the sample analysed was made in April, 1877, and may not represent the exact composition of the recent product. And it becomes a question, whether for travellers' use, it would not be advisable in this preparation to neutralise a little of the acidity of the lime-juice with potash, rather than to mask its presence by an excessive quantity of sugar. Pure lime-juice itself contains very little potash and phosphoric acid or other mineral matter; but that fact affords no argument against the introduction of small quantities of these compounds into such a preparation as that now under consideration.

It would be impossible to discuss in detail the applicability of the gelatin process to the preservation and concentration, in an uninjured, compact, and available form, of fruits, of meat, of cheese, &c., &c. But it may be safely affirmed that Dr. Morfit's invention has already been successfully applied in several directions, and that it is full of promise for the future. A. H. CHURCH

ELECTRIC DISCHARGE IN GASES[1]

II.

THE form of point most favourable to the production of the arc has been minutely investigated by the authors. By turning wires in a lathe to various outlines they arrived experimentally at the best point; this was then placed under the microscope and drawn by means of the camera lucida; from the study of the drawing it was ascertained that the longest spark was procured when the point assumed a form resembling a paraboloid; the curved out

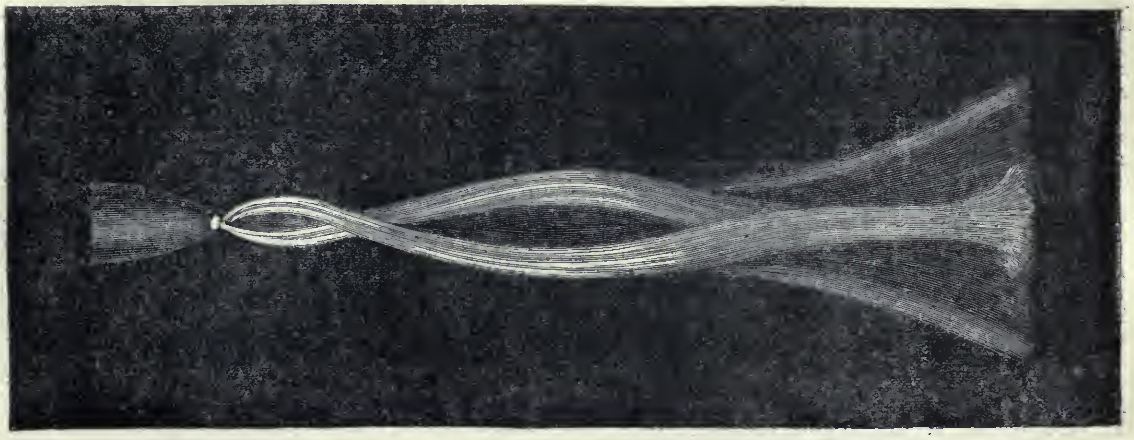


FIG. 8.—"Streamers."

line, which corresponded to that found experimentally, was one in which each succeeding ordinate was in the ratio of the square root of the odd numbers 1, 3, 5, &c., the sectional areas being consequently in the ratio of the odd numbers.

The curves in the diagram (p. 528) show the distances at which, with a given potential, the arc is formed between such a point and a disc, and between two such points respectively. The results recorded in the case of the point and disc are those obtained by electrifying the point to the sign (positive or negative) which gave the greatest length; for it was found that with low potentials the distance at

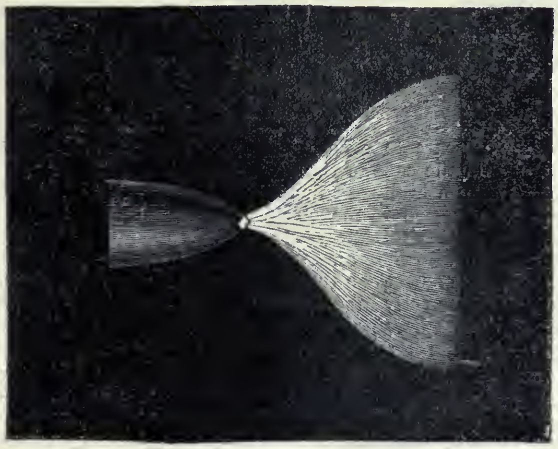


FIG. 9—"Glow."

which the arc forms is greater when the point is *negative*, that between 3,000 and 5,000 volts it is the same whether the point be positive or negative, but that with potentials higher than 5,000 volts it is greater when the point is *positive*.

The actual formation of the arc, when a point and disc or two points are employed as terminals, is preceded by a luminous discharge (streamers and glow), presenting phenomena of an interesting character; an extremely minute quantity of electricity passes as compared to that

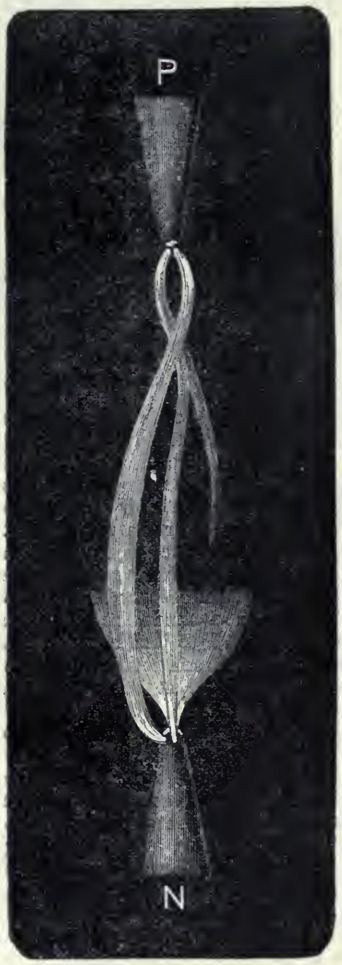


FIG. 10.—"Streamers and Glow."

when the arc is formed, but still it is sufficient when

[1] Continued from p. 528.

caused to pass through a vacuum tube to illuminate it strongly.

"The discharge from the point, when positive, has mainly to the naked eye a stream-like appearance, consisting of a waving line of light surrounded by a very faint sugar-loaf luminosity, and producing a rattling or strong hissing sound; from the point when negative it consists of a glow of light paraboloidal in form, and extending from the point to the disc, but much more brilliant at the point, the noise of the discharge being much less than when the point is positive. The disc, especially when positive, soon becomes covered with a peach-like bloom, and the deposit assumes the appearance of Newton's rings.

"Small as this current is, it is, nevertheless, very manifest from its brightness and the rattling, or loud hissing noise it produces; it frequently continues for some minutes before the spark actually jumps between terminals placed at the striking distance asunder; the streamer-discharge becomes brighter just before the jump and formation of the arc, and this suggested the possibility of particles being carried off in increasing quantity from the point to the disc, and thus contributing to the production of the spark. In order to test this hypothesis, the terminals were placed at the striking distance, and a continuous blast from a blowpipe bellows sent between them; this did not, however, have any effect on the length of the spark, but it deflected the arc when once it had formed.

"Under the microscope the discharge, from the point when positive, is seen to consist of several streams of light, which twist round each other like loosely-bound strands, as shown in Fig. 8, representing the discharge between the terminals, a point and a disc, in a horizontal discharger. Part of the discharge from the point negative is shown in Fig. 9. Fig. 10 shows on a scale of 4 to 1 the phenomenon obtained with 11,000 cells, the streamers and glow proceeding simultaneously from the opposite points of a vertical discharger and existing independently of each other."

In order to study these discharges the authors constructed a microscope (Fig. 11) with a revolving mirror placed at an angle formed by the two tubes composing the body of the microscope, which is bent for the convenience of observation. That part beyond the upright is formed of ebonite, in order to protect the eye from accidental shocks. The discharger used when this microscope was employed was horizontal, as seen in the figure, in order to permit of the employment of the mirror which was mounted on a horizontal axis; the

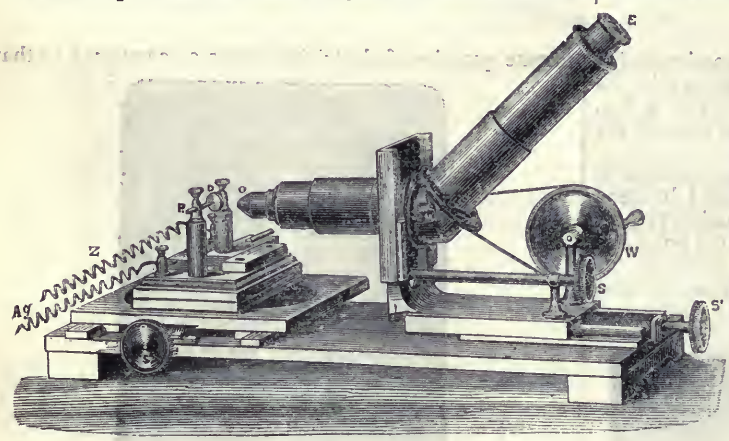


FIG. 11.—Microscope with Revolving Mirror.

FIG. 12.—Analysis of Streamers and Glow.

point P was placed in the fixed upright, and the disc D in that upright which is attached to the adjustable slide. The screw S′ is used to approach or recede the microscope in order to adjust to focus; S″ to bring the point horizontally, and S to elevate or depress the microscope by means of a pinion and rack to adjust the point vertically in the field. O is the object-glass and E the eye-piece; W a multiplying-wheel giving five revolutions of the mirror for one turn of the handle, which may be rotated with ease 200 times in a minute, so that the mirror can be caused to make 1,000 revolutions in a minute, or about seventeen revolutions in a second. The whole framework of the stand is of ebonite, to ensure insulation.

"When the revolving mirror was set in motion it was seen that the streamer-discharge was in reality, to a great extent, intermittent. At times a moderate rotation-velocity of the mirror served to show this by the production of a number of distinct images, as seen in the left-hand drawing in Fig. 12; at others it required the full speed of the mirror. The discharge appeared much more continuous with the point negative: so much so that the image was generally seen in the microscope as a sheet of light brightest near the point, and nearly uniform in a direction at right angles to the axis of the mirror. The right-hand drawing shows the appearance when the point is negative. The difference in the sound emitted when the point is positive and negative respectively appears to afford an additional proof of less continuity in the case of the positive.

"When a high resistance—four megohms, for example—was inserted in the circuit, the character of the discharge was completely changed. Instead of jumping across and forming an arc, a series of brilliant snapping sparks pass between the terminals at more or less rapid intervals, exactly like the sparks from a small Leyden jar; these pierce a piece of writing paper interposed between the terminals, producing minute holes. The spark does not jump at the full distance when the four megohms resistance is inserted; and, to produce an almost continuous succession of intermittent sparks, it was usually necessary to approach the point to 0·30 inch, when, without resistance, the spark with 8,040 cells would jump and form the arc at 0·34 inch."

When charcoal terminals were used the jump of the spark was about the same as with other terminals having a similar shape; the charcoal points could be separated when 8,040 cells were used to 1·25 inch generally, and occasionally to 1·5 inch, without breaking the arc. The arc presented the ordinary characters, as shown in Fig. 13; but that which takes place when the terminals are vertically one over the other is different, on account of its being undisturbed by upward air currents which deflect the arc.

The length of the spark differs in various gases at ordinary atmospheric pressures, but the authors found

that the difference in the length of the spark does not bear any precise ratio either to the specific gravity of the gas or its viscosity in reference to mechanical impulse; they propose to ascribe it, at all events provisionally, to a difference of *electric viscosity*. In order to make experiments on the dge ischarin different gases, they placed the

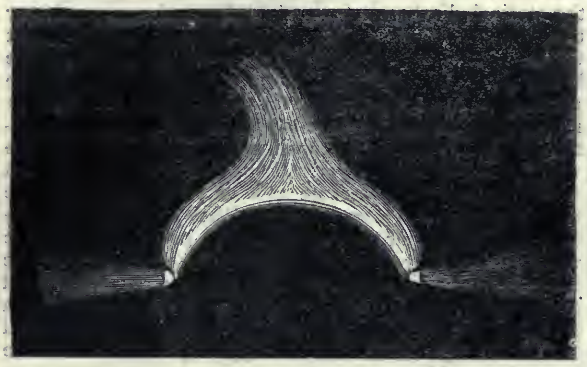


FIG. 13.—Arc between Charcoal Points.

discharger already described (Fig. 4), under a bell glass G G′ (Fig. 14) open at the top and covered with a glass plate P. The glass plate had two screw-clamps which were connected at its under surface with wires led from the screw-clamps *c* and *c′* of the discharger. In connection with these, on the outside surface, were two other screw-

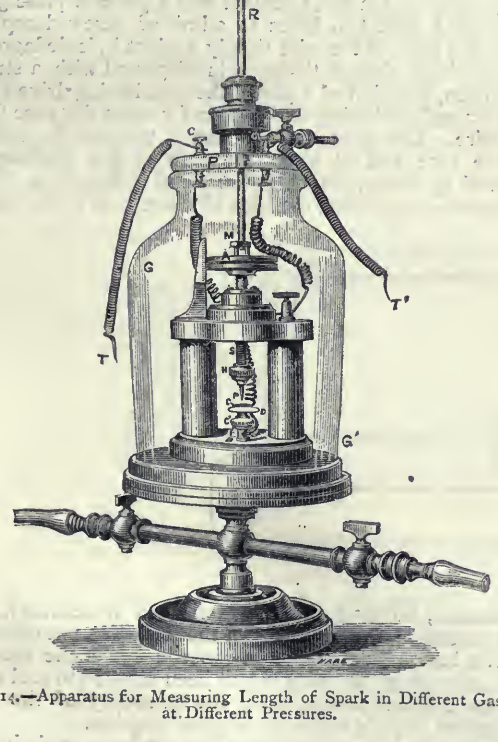


FIG. 14.—Apparatus for Measuring Length of Spark in Different Gases or at Different Pressures.

clamps *c c′* with which the terminals of the battery T T′ were connected. Through a stuffing box in the glass cover a steel rod R passed; this, below the glass, carried a crutch M, with two ebonite pins, which drop into corresponding holes made to receive them in the micrometer wheel A; the rod had on the top, outside the jar, a cross handle *h* for turning it. The distance of the terminals was easily regulated by means of this rod, as the micrometer could be read through the bell glass. Before admitting any gas into it, the bell glass was exhausted by the mercurial pump to a pressure of less than a millimetre, then filled with dry gas, and again exhausted and recharged. The following table shows the results obtained with spherical surfaces 1·5 inch in diameter and 3 inches radius.

Gas.	Length of Spark.	Ratio of Length of of Spark.		Ratio of "Electric Viscosity."	
		Referred to Air.	Referred to Hydrogen.	Referred to Air.	Referred to Hydrogen.
	Inch.				
Air	0·082	1·000	0·547	1·0000	1·828
Hydrogen	0·150	1·829	1·000	0·5467	1·000
Oxygen	0·082	1·000	0·547	1·0000	1·828
Carbonic anhydride	0·077	0·939	0·513	1·0650	1·949

The appearance of the arc was different in different gases, as will be seen in Fig. 15, where 1 represents the arc in air; this when examined with the microscope pre-

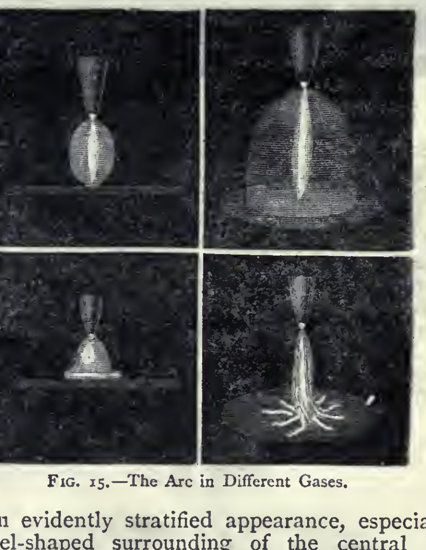


FIG. 15.—The Arc in Different Gases.

sented an evidently stratified appearance, especially in the barrel-shaped surrounding of the central bright spindle; the laminæ were extremely close and seen with very great difficulty, even with the revolving mirror of the microscope; with a moderate magnifier, a hand lens, the barrel-shaped surrounding appeared as if shaded with lines across it.

The arc in hydrogen, with the point positive, is shown in 2, Fig. 15; the central spindle was surrounded by a beautiful blue halo like a glass shade illumined by fluorescent light, and very brilliant on the disc. With four megohms resistance in the circuit, the streaming discharge from the point positive, at 0·502 inch distance, was carmine in colour. This distinctive colour is a proof that the gas in which the discharge takes place is the carrier of electrification in the streamer discharge. The appearance of the arc when the point is negative is shown in 4, Fig. 15; it moved about very rapidly and formed a star-like appearance on the positive disc; when the point was negative, before the jump of the spark, a very pale glass-shade-like halo, of a saddened olive tint extended from the point almost to the outer periphery of the disc.

The arc in nitrogen was reddish violet, and in oxygen it presented an appearance similar to that in air. The arc in carbonic anhydride is shown in 3, Fig. 15.

In the course of their experiments several curious and interesting phenomena were noticed incidentally. A slip of dry glazed note paper, when placed upon the disc of the discharger (Fig. 4), the other terminal being a point, was attracted to the disc so as to require a lateral pull amounting, in some cases, to as much as 30,000 grains, to cause it to slide over the disc: this lateral strain was reproduced when the paper was pressed against the disc by a weight of 129,690 grains. We must refer our readers to the original memoir itself for the full discussion of this phenomenon.

When the terminals were placed opposite to two gas jets emanating from the same gas pipe in metallic communication with the gas main, and consequently to earth, the flame opposed to the negative terminal is attracted, but that opposed to the positive *repelled*, as was observed to be the case with static electricity by M. Neyreneuf, at whose request the experiment was made. Fig. 16 shows the arrangement and effects observed.

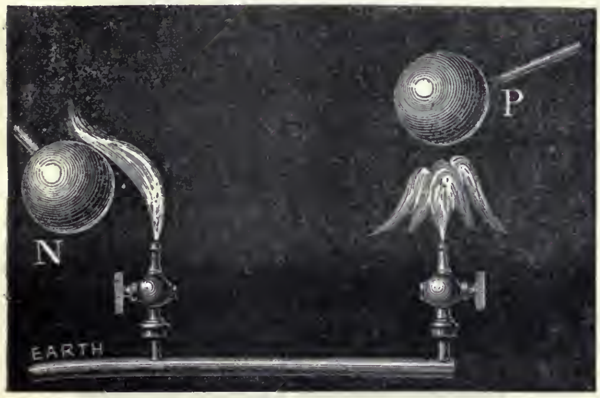


Fig. 16.—Repulsion of Gas-flame.

"When one of the terminals consisted of a very fine platinum wire 0·002 inch diameter and about 0·56 inch long, held in a holder like that used for holding needles in a mathematical instrument box, but adapted to go into the discharger, the wire took up a straight, circular, or elliptical oscillation, the glow at the point forming a continuous line of light marking its course; with the point positive the excursion was less than when negative, being with a potential of 8,040 cells, and a distance between the terminals of 0·32 inch, about 0·375 inch; with the point negative it was much more brilliant and about 0·8 inch. By interposing a resistance of 4 megohms the statical discharge took place from the extremity of the wire, frequently producing a beautiful and brilliant figure by the apparent crossing and interlacing of the bright lines of discharge from different points in the path of the oscillating wire; these occurred at such short intervals that the discharge looked like a persistent pattern of intricate engine-turning. By approaching the wire cautiously it was generally possible to cause the end of it to fuse into a minute globule, and the discharge then became much more striking. With 4 megohms resistance the static spark was longest and brightest when the wire was negative; if the wire was very straight, the oscillations took place in a cycloidal curve in a vertical plane, the discharge occurring at equal distances from the middle of the path as the minute globule at the end of the wire attained the limit of the greatest discharge from either side, so that two streaks of light were seen continuously; if the wire was slightly bent, the oscillation was conical or elliptical, and the figure produced by the discharge was then much more continuous and beautiful, because the distance from the point to the plate remained nearly constant."

Another point studied by the authors was the deflagration of wires of different metals. With their condenser of 42·8 microfarads charged with 3,240 cells, they could deflagrate 10·5 inches of platinum wire 0·0125 inch in diameter. When 2½ inches of either platinum, gold, silver, copper, iron, zinc, or aluminium wire of this size, strained across and kept in close contact with a piece of plate glass perforated with two holes to admit of screw clamps to press the ends of the wire tightly against it, is deflagrated by the above charge, the wire is dispersed with a strong explosion, like that of a pistol, the metal being driven into and strongly adhering to the glass for some distance from the wire. The greatest dispersion is towards the middle of the wire, and all along the path of the discharge is crossed by furrows in which the metal is deposited. This appearance suggested the idea of a stress at right angles to the path of the discharge which might involve a notable longitudinal contraction.

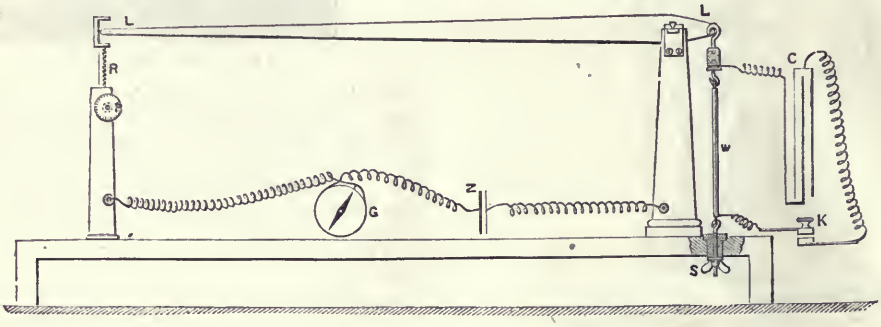


Fig. 17.

An experiment was made to test this hypothesis, but the results were entirely negative. When the key K (Fig. 17) was pressed down the accumulator C was discharged through the wire W, which was 8 inches long, and of diameters varying from 0·2 to 0·0125 inch; if now the wire had, at the moment of discharge, contracted longitudinally by $\frac{1}{13000}$ inch, the fact would have been recorded by the deflection of the galvanometer G, in consequence of the closing of the circuit of the battery Z, at the extremity of the long arm of the lever; with this apparatus, however, the contraction, if it occurred, was not detected; the extremity of the lever was not observed to rise before it descended in consequence of the elevated temperature of W; after W had been allowed to cool down to its original temperature, the extremity of the lever remained slightly below its original position.

Only a few experiments have as yet been made by the authors on the length of spark at different pressures below that of the atmosphere, but they intend to pursue this investigation with a micrometer discharger of longer range than that shown in Fig. 4.

With two spherical surfaces, each of 3 inches radius of

curvature and 1·5 inch diameter, the following results were obtained with 8,040 cells:—

Pressure.	Fraction of an atmosphere.	Length of spark.	Ratio to length at 1 atmosphere.	Ratio of length of spark to dilatation.
Millims.		Inch.		
760	$\frac{1}{1}$	0·079	1·00	$\frac{1}{1} = 1·000$
602	$\frac{1}{1·262}$	0·100	1·26	$\frac{1·26}{1·262} = 0·999$
414·7	$\frac{1}{1·833}$	0·200	2·52	$\frac{2·52}{1·833} = 1·375$
299·5	$\frac{1}{2·537}$	0·400	5·04	$\frac{5·04}{2·537} = 1·986$
141·5	$\frac{1}{5·370}$	0·800	10·08	$\frac{10·08}{5·370} = 1·876$

ERNEST QUETELET

THE death of M. Ernest Quetelet, "chef du service Astronomique" of the Observatory of Brussels, took place at Ixelles on the 6th instant, after a long and painful illness. His connection with the observatory dates from 1855, when he entered it as an assistant to his father, the late Lambert Adolphe Jacques Quetelet, who was then the director, and who died so recently as February, 1874. Ernest was born in Brussels August 7, 1825. At that time his father was busily occupied in pressing on the king and the municipality of Brussels the importance of establishing an observatory for meteorological purposes. After much discussion and many delays it was determined in 1826 to establish an astronomical observatory; Quetelet was directed to obtain instruments, to visit Paris and London, and on January 9, 1828, he received his official appointment, his title being "astronomer." The three principal astronomical instruments were set up in 1835, but the first four volumes of the *Annales* of the observatory coming down to 1845 contain only meteorological notes. The first volume (date 1834) opens with an "aperçu historique des observations de météorologie faites en Belgique jusqu'à ce jour," commencing with 1763, and shows how thoroughly the director entered into the importance of the work. The observatory in 1845 was the centre of meteorological observing stations, of which there were more than eighty.

Although Ernest, as he grew up, shared his father's interest in the various observations included under the head meteorological, and took terrestrial magnetism as a special subject for study, on joining the observatory in 1855 he was appointed to take the astronomical observations and the *Annales* record that the observations were made by him and the calculations of the reductions by M. L. Estourgies. From 1857 he has had in hand the revision of a catalogue of the variable stars, a large part of which has been published. Two years ago he issued the climatological elements of Brussels, in a series of eighteen tables, for the ten years ending 1873, and in the *Bulletin* of the Société Royale are many papers by him on magnetism.

Before entering the observatory he was in the Engineers for several years, after having passed through the École Militaire of Brussels. In 1848 he was engaged as a subaltern on the work of the fortifications of Antwerp. It was while still in the Engineers he communicated his first paper to the Academy, "Recherches sur les Médianes" (October 9, 1859), which was printed in the *Mémoirs Couronnés*. In 1856, shortly after joining the Observatory, he wrote a paper on the magnetism of the earth in North Germany and Holland, and in 1859, "On the Magnetic Declination at Brussels."

The Observatory has all through its existence had to struggle against difficulties; one of the latest recommendations of the Commission on it was to effect the following improvements:—

"To complete the magnetic system of the Observatory by the acquisition of self-registering instruments, to organise the International Meteorological Service, to obtain an equatorial of large dimensions with the accessories necessary to the spectroscopic investigation of the heavens, and to increase the number and improve the position of the observer."

During many years M. Quetelet has often been appointed referee to the printing of papers in the *Mémoires* of the Académie Royale, and has been himself a frequent contributor.

GEOGRAPHICAL NOTES

IN the Geographical Section of the French Association some papers of interest were read. Dr. Carret read a curious paper on the Distribution of Antipodes, in which the author indulged in some rather fanciful theorising. Gen. Ricci spoke on the geodetic work carried on by the Italian Government, which is energetically completing the triangulation of Italy, connecting it with the rest of Europe on the one side and Africa on the other. Gen. Ricci also spoke of the regular tidal observations carried on at various stations on the coast of Italy, mainly with a view of getting a true level for geodetic purposes. M. H. Duveyrier read an elaborate paper on the remaining problems in African geography, in which he divided unknown Africa into seven great regions: (1) The Sahara and the Libyan Desert; (2) The country between the Joliba and the Guinea Coast; (3) The upper courses of the Binue and Shari; (4) The region behind Cape Guardafui; (5) The equatorial chain of lofty mountains; (6) The completion of the basins of the Nile, Congo, and Ogove; (7) The basin of the Cunene. Altogether more than 11,000,000 square kilometres remain unexplored, more than one-third of the whole surface of Africa. At the mean rate of discovery since the beginning of the century, this might be covered in about forty-eight years, though the ratio is now so increased that it ought to be done in much less time. A paper by M. Maunoir recounted the services done to geography by France since 1800, and when all put together with the eloquence of a Frenchman, they seem formidable.

NEWS from Capt. Tyson's Arctic expedition in the *Florence* has been brought to Washington by the schooner *Helen*, which wintered in the same bay on the coast of Cumberland. Meteorological observations were taken most accurately during winter by Mr. Sherman, the physicist of the expedition. Unfortunately the naturalist inflicted on himself a wound when on duty, and was disabled for the greater part of the winter. Capt. Tyson went to Disco to recruit natives, collect goods, and purchase dogs. He fulfilled his duty with his wonted activity and success. But when everything was ready he learned by a message sent from Washington that the preparations were useless, the American Congress having taken its vacation without deliberating upon the report so carefully drawn by the Marine Committee. The brave and accomplished commander of the *Florence* is now on his way to Washington, where he is expected daily. We are informed that a demonstration will be made against this piece of Parliamentary negligence. This preliminary expedition was entirely fitted out at the expense of Capt. Howgate and his friends.

NEWS has arrived that the Bremen steamer *Neptune*, Capt. Rasmussen, which left for the Ob, in Siberia, on July 16, reached Hammerfest on the 6th instant with a full cargo of Siberian wheat. The *Neptune* was laden with all sorts of mercantile goods. She entered the Nadym on August 13, and had no ice difficulties on the voyage out. Perhaps in future, when the

navigation of the estuary of the Ob is better known, the voyage may be made in even a shorter time. Indeed if depôts were established at suitable points on the north Norwegian coast, it might be possible for a ship to make two journeys to Siberia in one summer. Probably the Ob is the most important of the Siberian rivers so far as commerce is concerned. Trade on the Ob is already considerably developed, the river being navigated by over thirty steamers. The region around the river is the most productive and most thickly inhabited in Siberia.

IN an article in the September number of Petermann's *Mittheilungen*, on the chief branches of the Russians, much interesting information is given on the characteristics and distribution of the Great, Little, and White Russians, illustrated by a carefully constructed map. In the same number Dr. Junker, in a letter to Dr. G. Schweinfurth, describes his travels in the south-west part of the Nile Region in January-October, 1877, adding considerably to our knowledge of the region, and making several corrections on existing maps. Lieut. Weyprecht describes the results of his observations in 1871-4, on the temperature and depth of the sea to the east of Spitzbergen. The sea, he finds, is comparatively shallow, seldom exceeding 400 metres.

WE have received a handsome atlas of the State of New Hampshire (U.S.), containing, besides a series of beautifully-executed maps of the state and of its counties,—meteorological, geological, agricultural, and arboricultural,—a vast amount of well-arranged information on its topography, geography, river systems, climatology, railroads, educational institutions, agricultural and botanical productions, mechanical and manufacturing interests, &c. The work is edited by Mr. H. F. Walling, C.E., and Prof. C. H. Hitchcock, and is published by Comstock and Clive, New York. The work is creditable both to the editors and publishers. The long list of "patrons" of the atlas appended—mostly people in business—speaks well for the intelligence of the inhabitants of New Hampshire.

IN a letter to Sir Samuel Baker from a gentleman in the Khedive's service, the latter describes a successful journey which had been made with some Indian elephants in the White Nile region, proving that this powerful and useful animal may be utilised advantageously in African travel and exploration.

OUR ASTRONOMICAL COLUMN

THE INTRA-MERCURIAL PLANET.—In addition to the letter addressed to the Astronomer-Royal by Prof. James Watson, after revising his first position of the object near θ Cancri, more carefully at Ann Arbor, similar communications have been made to M. Fizeau (*Comptes Rendus*, May 2), Prof. Förster (*Circular zum Berliner Astronomischen Jahrbuch*, No. 98), and to Prof. Peters (*Astronomische Nachrichten*, No. 2,217). The definite position is in R.A. 8h. 27m. 24s., Decl. + 18° 16′ for July 29, at 5h. 16m. 37s. Washington M.T., or 10h. 24m. 49s. M.T. at Greenwich, which position Prof. Watson considers to be trustworthy within five minutes of arc, with a greater probable error in the declination than in the right ascension. The other points named by the discoverer, upon which stress is to be laid, are the fact of the star θ Cancri being also observed, the appearance of a sensible disk with a power of 45 on a 4½-inch refractor, its ruddy colour and much greater brightness than that of the neighbouring star. There has never been a suspicion of a variable star in this vicinity, nor can the appearance of a disc be so explained. Prof. Watson seems to have satisfied himself that the object was not a comet; indeed, such a body would hardly appear round and well-defined with the sun totally eclipsed. In the case of the comet of March 1847, which was observed in full daylight, at a similar distance from the sun to that of Prof. Watson's object, two short tails were visible though the head was circular, and the great comet of February, 1843, also exhibited a bifid tail, which was bright and distinct to the naked eye. Mr. Hartnup, who observed the comet of Klinkerfues, 1853, in broad daylight, described it as circular, well-defined, and without tail, but the case is hardly analogous to that of a comet viewed while the sun is wholly hidden.

[Since the above was in type, the full details of Prof. Watson's observations and reductions have been received.]

THE VARIABLE NEBULA IN TAURUS (HIND, 1852).—In the diagram attached to M. Tempel's remarks upon this object in *Astron. Nach.*, No. 2,212, a distinction is made between the position given in No. 839 of the same periodical and that assigned with reference to the neighbouring variable star T Tauri. To prevent misconception on this point it may be well to remark that, on the first night the nebula was perceived with Mr. Bishop's seven-inch equatorial—October 11, 1852—it preceded the variable star 1s.·2, and was south of it 0′·7, as stated in the *Astron. Nach*, and that at no subsequent time when the nebula was observed with the same instrument was any difference of position noticed: it appeared to nearly touch the star on the S.P. side. No. 1 on M. Tempel's diagram should be therefore erased. In a note in his Supplementary Catalogue Mr. Dreyer states that he found no appearance of nebulosity near the well-known variable; nor did Dr. Copeland, observing with the large refractor at Lord Lindsay's Observatory; nor M. Tempel, with a fine Amici of 11-in. aperture, at Arcetri. On the other hand, M. Otto Struve still found traces of nebulosity with the Pulkova instrument, which he "believes is certainly the variable nebula itself, only in altered brightness and spread over a larger space;" and he adds, "some traces of nebulosity are still to be seen exactly on the spot where Hind and d'Arrest placed the variable nebula." The accurate position of T Tauri has yet to be determined by meridional observation. Argelander re-observed Bessel's star of the ninth magnitude, which precedes it 16·5s., about 4′ south.

THE LATE DR. E. VON ASTEN.—By the early death of Dr. von Asten astronomy has lost a most able worker in a branch which has numbered of late years fewer distinguished names than formerly. He was one of Argelander's pupils and intended to apply himself to observations, but, we believe, through a serious accident, he was incapacitated for active occupation, and his desire to devote his attention to astronomy could only be gratified by obtaining employment in calculation. This he was so fortunate as to effect through the director of the Imperial Observatory at Pulkova, M. Otto Struve, who engaged him as one of the staff of computers. In this position Dr. von Asten had for some time carried on a rigorous investigation on the motions of Encke's comet, one of the most interesting results of which has been to prove that while in some revolutions an acceleration similar to that attributed by Encke to the existence of a resisting medium has made itself evident; in others the motion of the comet could be precisely followed without such hypothesis, and hence a different cause might be found for the cases of acceleration. Dr. von Asten previous to his connection with Pulkova, minutely discussed the whole of the observations of the great comet of Donati (1858 vi.), arriving at the conclusion that at the time it was visible it was moving in an elliptical orbit with a period of nearly 1,900 years.

NOTES

THE Association of German Naturalists and Physicians commenced its sittings at Cassel on Wednesday last week, and judging from the numbers of the *Tageblatt* and of the Cassel papers that have been sent us, the meeting has been quite as

successful as usual. Every provision had been made by the local authorities for the reception of the Association, and while abundance of serious work has been done, a fair proportion of the time has been given to enjoyment. Among the arrangements, for example, are performances of "Faust," "Midsummer's Night's Dream," "Comedy of Errors," "Tannhäuser," and other classical pieces in the theatre, and among the excursions is one to the Paris Exhibition. The president, Dr. Stilling, in his opening address, traced the history of the scientific life of Cassel, showing that much good work had been done there, and that the names of not a few eminent men of science have been connected with the city. The title of Prof. Oscar Schmidt's lecture seems sensational enough—"Darwinism and Social Democracy,"—but his treatment of it seems to have been quiet enough; he aimed at showing that Darwinism instead of being a leveller, showed the tendency to be everywhere to heterogeneity. Prof. Hueter, in his lecture on the Physician in Relation to Research and Natural Science advocated a longer curriculum for medical students and a more thorough training in science and in the experimental method. The numerous sections were busy enough, and many good papers were read, but any further account of the proceedings we reserve till we have received a complete set of the *Tageblatt.*

IN the terrible panic which has seized the Southern States under the epidemic of yellow fever, we are glad to see that science has been pressed into service and stuck bravely to her post. Every one who can is flying for life, but it has been deemed advisable to retain the sergeants of the United States Signal Service at their posts in order to keep up for the use of the medical men regular observations of temperature, humidity, and other atmospheric phenomena which may have any influence in the spread of the disease. One of these officers died at his post and two others had been struck down, probably also fatally, as we learn from the New York *Tribune.* It is encouraging to see that the United States authorities have kept their heads clear enough to perceive that the services of science are indispensable to "the healing of the nations."

M. BISCHOFSHEIM, the well-known Parisian banker, has sent a sum of 10,000 francs to the French Bureau Central Météorologique to help in the construction of the intended Mont Ventoux Observatory. We may remind our readers that he, at the suggestion of his friend M. Leverrier, helped in the same manner the construction of the Puy-de-Dôme and Pic-du-Midi establishment. M. Bischofsheim has also agreed to pay M. Eichens 1,000*l.* to complete within a year the construction of the great refractor begun in Leverrier's time in 1870.

THE fitting up of the Lyons Observatory is progressing favourably; the inauguration will take place in a few weeks.

WE noticed with regret a few weeks ago that a tax had been imposed on the French communes to entitle them to receive the daily meteorological telegrams of the Bureau Central. A new delay has been granted for the subscription, and we are happy to state that a large number of rural parishes, fully appreciating the importance of the service rendered by the telegraphic warnings, have already agreed to pay the yearly charge, which has been reduced to forty francs.

M. COCHERY, the director of the French postal telegraph, is now in London studying the working of the English system and hopes to introduce into the French service a number of improvements which the large traffic and progressive character of the English service has brought into use.

THE meeting of the Iron and Steel Institute opened at Paris on Monday, with a presidential address by Dr. C. W. Siemens, who showed how comparatively well provided France is with institutions for scientific education, and referred briefly to the work carried on at some of the great industrial centres. We have already given a list of the papers to be read, all of them of more or less technical bearing.

THE fourth Congress of Orientalists commenced its sittings this year at Florence on the 12th inst. The chief nationalities have been well represented, and the reception by Florence and by Prince Amadeo has been hearty. One attractive feature is an extensive exhibition of objects connected with the subjects with which the Congress deals.

THE Fourth Annual Conference of the Cryptogamic Society of Scotland, will be held at Edinburgh on October 9, 10, and 11. The programme includes excursions, a dinner, and an exhibition of fungi. The meeting-place is the Botanic Gardens of Edinburgh, the president is Prof. Balfour, and the secretary Dr. Buchanan White, Perth.

AN agricultural exhibition took place at Lockwitz, near Dresden, on September 5–7. It formed part of the general meeting of the Saxon Agricultural Society which enjoys the special patronage of the King of Saxony.

THE Congress for 1878 of the German Viticultural Society was held at Würzburg on September 15–19.

GREAT activity continues to be manifest in Vesuvius, and volumes of lava are projected to a height of 100 yards above the new crater, accompanied by loud explosions. However, no flames are yet visible.

SINCE M. du Moncel presented the Edison phonograph to the Academy of Sciences electrical inventions of every description are sent to him for presentation. A large number of these deserve notice, and no sitting passes without M. du Moncel speaking on two or three different inventions. This state of things has created some anxiety amongst members unable to understand electrical matters. On Monday week one of them proposed to the president that M. du Moncel be obliged to execute all the experiments he was describing before the Academy, in order to prove whether they were sound. M. du Moncel replied that he was himself verifying them with much care, but that a number could not be executed before the learned assembly, as two different stations, situated at a great distance, were required; and he reminded them that, when he brought the phonograph before the Academy, he had taken the precaution to procure an able operator for the working of it. The point of the reply is that a certain number of the members said that the phonograph was exhibited by a ventriloquist. M. Fizeau, who was in the chair, called the assembly to a vote, and the discussion ended. It was not recorded in the *Comptes Rendus.*

AT the meeting of the Botanical and Horticultural Congress in Paris, the following were among the most interesting communications and discussions:—On the influence of the age of seeds on the plants raised from them. Prof. Baillon found that Prof. Cazzuole's view, that the newer the seeds of *Cucurbitaceæ* the larger the proportion of male flowers, and *vice versâ*, was not confirmed by his own experiments, in which he had sown melon-seeds dating from 1870, and for comparison last year's seeds.—On double flowers. Prof. Morren, in support of his well-known theory of the incompatibility of truly variegated leaves and double flowers, pointed out that in the camellia and *Kerria japonica* normal flowers are only known to occur on variegated stocks. In a Hibiscus, which unites these peculiarities, the flower-buds fall without opening; in a variegated and double wallflower, many of the branches revert and are quite green.—Descriptions were given of the chief botanical laboratories in St. Petersburg, Amsterdam, Florence, and Paris.

In the last city there are no less than four in active work, viz., those of the Sorbonne, École de Médicine, École de Pharmacie, and Muséum (Jardin des Plantes) respectively, besides one for experimental physiology at Vincennes.—On the question of gymnospermy. Prof. Arcangeli's anatomical researches had led him to conclude that the coat of the ovule in Gymnosperms was sometimes carpellary in origin, but not always. Unfortunately no discussion followed.—M. Sirodot gave an abstract of his researches on *Batrachospermum*, which he shows is the sexual form of *Chantransia*.—M. Borodin gave an account of the variations in the excretion of CO_2 in leaves of different ages. —M. Millardet found the lesions from phylloxera differ according as the part attacked is the young rootlet or an older part. In the latter case a septum of cork is often formed to preserve the parts that remain healthy. Unfortunately the question of the "Hortus europæus" was hardly discussed, but suggestions were made for the compilation of a new "Steudel." Besides the excursion to Segrez, a large number of members were conducted, on the 22nd, all over the remarkable irrigation works at Genevilliers (where a fourth of the sewage of the city of Paris is utilised), by M. Durand-Claie, Engineer to the Works, and M. H. Vilmorin, Secretary to the Commission d'Etudes. Afterwards many of the foreign members breakfasted with M. E. Cosson, and visited his splendid herbarium, and in the evening was held the banquet of the Congress. On the 23rd a large party visited the gardens of the Jardin des Plantes with Prof. Decaisne, and the herbarium with Prof. Bureau. The final session of the Congress was held at Versailles on the 25th, after which the members visited the show of the Horticultural Society of the town, at whose annual banquet the foreign members were entertained in the evening.

A VERY useful paper "On Lightning Conductors and Accidents by Lightning" was read at the British Association meeting by Richard Anderson, F.C.S. So slow has been the "march of progress" in the application of one of the greatest scientific discoveries of modern times to the uses of daily life, that even now, after the lapse of more than a century, the employment of lightning conductors, simple as they are, and as inexpensive as simple, is far from being general, still less universal. At least one-half, and perhaps two-thirds, of all the public buildings, including the churches and chapels, of Great Britain and Ireland, are without protection against lightning. As to private houses, it is safe to assert that not five out of every hundred have lightning conductors. It is well known that the amount of property destroyed annually by lightning is very great, though it is naturally impossible to form any estimate of it. The terrible losses, both of property and human lives, still occasioned by lightning, are the more lamentable, as they are in nearly all cases the result of the grossest negligence. The negligence is three-fold—namely, first, in not providing any lightning conductors at all; secondly, in not placing them in the right position, or in sufficient number to cover a given area; and, thirdly, in not having them regularly tested, so as to ascertain their constant efficiency. Even some of the first cathedrals of England, such as Peterborough, have no lightning conductors whatever, while others, supplied with them, are insufficiently protected, as is apparent to any competent observer. Mr. Anderson gives striking examples of the absence of lightning conductors, and of the disastrous effects of their being badly placed. The third cause of neglect is by no means the least. Mr. Anderson justly argues that lightning conductors ought to be at regular intervals, at least once a year, carefully inspected, and their efficiency tested by a galvanometer. The absolute neglect of this precaution which is now prevailing is no doubt the cause of a vast number of casualties by lightning, inflicted upon buildings nominally protected by conductors. Utter neglect of the conductor, when once it has been put in its place, is the commonest thing, and indeed the rule, as regards private dwellings; and, we fear, there is little difference in this respect as to most public buildings, churches, and chapels. In fact, it is the old case of a matter of however great consequence, yet being utterly disregarded as "nobody's business." Between three and four thousand pounds were spent in protecting the Houses of Parliament by lightning conductors at the time of their erection, some twenty years ago. Since that time, as far as Mr. Anderson can learn, after minute investigation, they have never been tested, and there is no guarantee whatever that a discharge of lightning may not at any time fall upon the Queen's throne, the Lord Chancellor's woolsack, or the Speaker's chair. A French writer pithily expresses the results that follow from a lightning conductor over a house not having a proper "earth connection," by saying it is lightning guided to the owner's iron bedstead. Mr. Anderson then gives several useful practical instructions as to what ought to be done to amend the present unsatisfactory state of matters, which well deserve attention. As the clock in churches and other public buildings is methodically inspected by the clockmaker, so ought every lightning conductor to be as systematically examined by an electrician or other competent person. Already such a system of inspection and testing of conductors exists in Paris and several other French towns. Shall we say, once again, "They manage these things better in France?"

THE long-expected report of the United States Entomological Commission, appointed to investigate the ravages of the locust, has been published as one of the series of Dr. Hayden's survey, and constitutes a very important addition to the scientific and practical literature on this subject. Although it has been several years since there has been any serious damage caused by the Rocky Mountain locust, their enormous destructiveness, when they do occur in abundance, is such as seriously to threaten the prosperity of the States in which their ravages are prosecuted. The present report professes to be for 1877, and posts the subject up to that date, being a stout volume of nearly 800 pages, and is accompanied by excellent wood-cuts and engravings, representing the insect and its winged and other enemies in all stages of development and condition. In addition to the descriptions of the species and its general natural history, various remedies and devices for its destruction are communicated; also notes on the influence of prairie fires on the increase of the locust, the influence of the weather on the species, the effects which generally follow severe locust injury, and the uses to which locusts may be put. There are also chapters on the ravages of other species of locusts in the United States and on the ravages of locusts in other countries. Congress at the last session provided for the continuation of the inquiry for the present year under the same commission.

MR. W. H. SHRUBSOLE informs us that an imperfect tooth recently found in the London clay at Warden, in the Isle of Sheppey, has been submitted by him to Prof. Owen, who says of the specimen that "it suffices to determine both the mammalian and ungulate nature of the animal it belonged to; that it comes nearest to the kind of *Palæotherium* figured in 'British Fossil Mammals,' p. 322, Fig. 116, but is too incomplete to show the genus or species." Mr. Shrubsole adds that there is no record of any mammalian remains having been found in Sheppey before.

MESSRS. CHURCHILL will publish, early in November, a work on the poisonous snakes of India, by Dr. Ewart, illustrated with coloured plates reduced from Sir Joseph Fayrer's large folio work.

MESSRS. TEGG AND CO., Pancras Lane, will shortly publish "Berkeley's Principles of Human Knowledge," with Introduc-

tion and Copious Explanations, by Collyns Simon, LL.D., author of "The Nature and Elements of the External World," and Proposer of the Berkeleian Prizes in 1848 and 1850.

A NEW book on Ferns has just made its appearance at Salem, Massachusetts, under the title of "Ferns in their Homes and Ours." Its author is Mr. J. Robinson, Professor of Botany, Massachusetts Horticultural Society, and the book forms one of a series called the "American Natural History Series." It has been put together especially for the use of persons residing in the United States, but the author has nevertheless made himself thoroughly acquainted with the works of European pteridologists and pays a high tribute to those of our own country, notably the more recent works of the veteran John Smith. Though the book commences with a consideration of the life-history of a fern, classification, distribution, and nomenclature, it is for its practical part, dealing with the selection and cultivation of these favourite plants in living, that the book will be most valued.

NEWS from Denmark states that the last pillar of the first fixed bridge across the Lim Fjord has now been finished; the new bridge will connect Aalborg on the south side of the fjord, with Norresundby on the north, and it is hoped that it will be opened for traffic during the autumn. Our readers will remember that the Lim Fjord is an arm of the sea stretching right across the Danish continent from east to west.

SOME interesting excavations have been recently made at the "Limburg," a large ruin near Dürkheim in the Bavarian Palatinate, at the instigation of the German Anthropological Society. During 1877 prehistoric remains had been found at this spot, and the work being continued this year, numbers of urns, human and animal bones were discovered, all undoubtedly of prehistoric origin. The most interesting part of the discovery is the laying bare of a cremation ground.

A PAPER on "The Salt Lakes, Deserts, and Salt Districts of Asia," by Mr. Thomas Ward, read before the Liverpool Literary and Philosophical Society has been published separately, with a map. The author endeavours to illustrate from what is known to be going on in the formation of salt at the present time, the way in which salt was formed in past ages.

THE Rev. Thomas Powell, of Upolu, Samoa, writes us that in vol. xv. of NATURE, p. 503, in our report of the Linnean Society, his paper on "Poisoned Spears and Arrows" is represented as having reference to *Samoa*. Mr. Powell sends us a corrected copy of the paper from which we see that the paper has reference to the New Hebrides. The Samoans, Mr. Powell states, have no such custom as the use of poisoned weapons of any kind. They formerly made use of the bow and arrow, not, however, for purposes of war, but of sport only. The introduction of fowling-pieces has abolished the use of the bow. Another error, Mr. Powell writes, into which we have been led is the statement that *Callophyllum inophyllum* was among the trees whose milky juice was used as a poison. This is not Mr. Powell's statement. His informant said that the *Toto* resembled that tree in general appearance. The *C. inophyllum* is a valuable timber tree in common use. Its flowers and fruit are used in Samoa as a perfume. From its fruit an oil is extracted in Fiji, which is useful as a liniment in rheumatism.

THE additions to the Zoological Society's Gardens during the past week include a Banded Ichneumon (*Herpestes fasciatus*) from West Africa, presented by Mr. F. T. Blackley; two Vinaceous Turtledoves (*Turtur vinaceus*) from West Africa, a Greek Land Tortoise (*Testudo græca*), European, presented by Miss Harris; a Common Adder (*Vipera berus*), European, presented by the Viscount Mandeville; a Spotted Turtledove (*Turtur auritus*), bred in the Gardens.

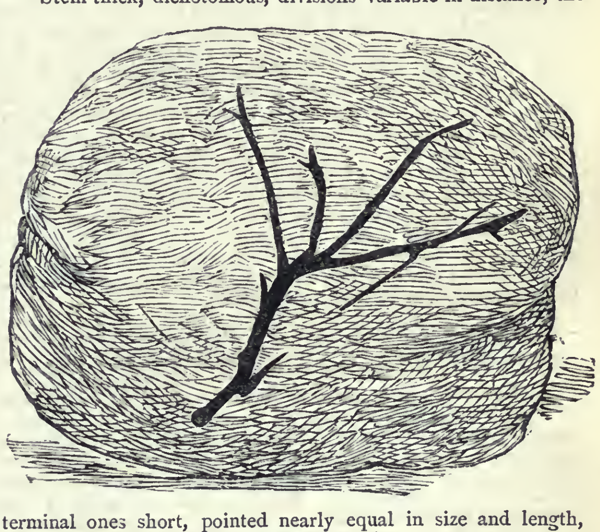
A FOSSIL PLANT[1]

MANY years ago the late Sir William Logan drew attention to the occurrence of fossil plants in the Devonian strata of Canada, and Prof. J. W. Dawson, F.R.S., in the *Quarterly Journal* of the Geological Society, in vols. xv. and xviii., described and figured some of these specimens. Amongst them was a plant which he designated *Psilophytum*. Dr. S. S. Scoville has since discovered the remains of plants in the lower silurians at Longstreet Creek, near Lebanon, Ohio, which Prof. Newberry considered as the casts of some large fucoids or marine plants. Count Saporta has found the branch of a fern in the silurian schists or slates of Angers, France. Prof. Leo Lesquereux, to whom we owe so much for his labours in investigating the fossil plants of the United States, in a paper read before the American Philosophical Society, October 10, 1877, has described and figured a plant from the lower Heldeberg sandstone, Michigan, under the name of *Psilophytum cornutum*.

In a paper read by myself before this society on December 26, 1876, I stated that after some years' search I had not been able to find the *Palæochorda major* mentioned by Professors Harkness and Nicholson as occurring in the Manx schists in such a state of preservation as to be certain of its true nature, but I had a fucoid in my possession found by Mr. Grindlay in the drift near Laxey.

As Mr. Lesquereux's specimen so much resembles the one found at Laxey I shall give his description at length.

"Stem thick, dichotomous, divisions variable in distance, the



terminal ones short, pointed nearly equal in size and length, surface slightly rugose and irregularly striate.

"The branches in the lower part are thick comparatively to their length, three or four millimetres, irregularly striate when decorticated, or merely punctuate upon the thin bark with small projecting dots resembling the basilar remains of scales or small decayed leaves; lateral branches short, narrowed to a sharp point; the upper or terminal ones about equal in length, appearing like a pair of pointed horns."

The species is only comparable to some of the fragments not specified but figured by Prof. J. W. Dawson (Geol. Survey of Canada, Fossil Plants of the Devonian and upper silurian formations, Figs. 243, 244). The author remarks "that these fragments are probably originating in the upper silurian of Gaspé; that as they are found in the lower part of the limestone which underlies the Devonian Gaspé sandstone and become more abundant in the upper beds, this suffices to indicate the existence of the neighbouring land, probably composed of silurian rocks and supporting vegetation."

From the preservation of its branches even to the smallest subdivisions, the specimens here represent part of a plant embedded in the place of its growth. The matrice is a piece of very hard calcareous shale seven to eight millimetres thick, bearing on one side irregular undulations like ripple marks, without any trace of organic remains, and on the other the fragments of plants as figured here. The branch in (*a*) represents a different species,

[1] "Notice of a Fossil Plant found at Laxey, in the Isle of Man," by E. W. Binney, F.R.S., F.G.S., President, paper read at the Literary and Philosophical Society, Manchester.

and indeed a marine or rather a brackish plant, closely related to the species of the present genus *Chorda*, Stack. This fragment seems to have been mixed in the tide pools with fresh water or land plants growing there. For another thick specimen of the same locality and compound bears a profusion of marine mollusks, and has only branches of this as yet undescribed marine species, *Calamophycus septus*.

Habitat lower heldeberg sandstone, Michigan, discovered and communicated by Dr. Carl Rominger (State Geologist).

On comparing my Manx specimen, which was found on the surface in a field at Laxey with that figured and described by Prof. Lesquereux, it agrees with the latter in every respect, except that striæ and scales are not observable on the stem. The stem is thick, dichotomous; divisions variable in distance, the terminal ones short, pointed nearly equal in size and length, surface nearly smooth. The branches in the lower part are thick comparatively to their length. The surface of the stem appears to be smooth and affords no evidence of striæ or scales.

The woodcut on the preceding page represents the specimen a little over the natural size.

The stone in which the plant is embedded is a fine-grained grit of a grey colour, and the specimen itself is of a yellow tint as if coloured by oxide of iron; it runs nearly at right angles to the bedding of the stone, and appears as if standing in the same position as it had grown. The stone is a rolled one but it is evidently from the Manx schists found in the vicinity. These, according to Profs. Harkness and Nicholson, are of the age of the Skiddaw slates, but the rock in which the fossil occurs may be of older date, as some of the lower portions of the series have not yet been clearly determined; so here we have evidence of a plant in the lowest part of the silurian formation, or even lower. By diligent search the rock in which the specimen occurs may probably be found *in situ* in the upper part of the Laxey valley. The great resemblance, if not the identity, of the Manx with the American specimen is very remarkable, and shows the similarity of conditions then prevailing in distant parts of the globe. The specimen might have been called *Psilophytum cornutum*, if any marking on the surface of the stem had been observed, but as these appear to be absent it is proposed to call it *Psilophytum monense*. As to the nature of the water in which it grew there is no evidence from organic remains, but its characters resemble those of a fucoid more than a land plant.

THE FIGURE AND SIZE OF THE EARTH[1]

THE portion of the earth's surface bounded by the horizon which one is able to take in at one view, is but seldom a regular plane; more generally heights and depressions, mountains and valleys, alternate with each other so irregularly, that at first nothing seems farther from reality than the idea of a regular form of the earth's surface. But the more our point of view overtops the mountains which lie within the horizon, the further obviously will our range of view extend, and all the mountains and valleys which give so irregular a form to the horizon of the plain will, under this condition, become imperceptible and unimportant. Indeed, one can easily conceive that if the eye were able to comprehend at one time a much greater portion of the surface, the irregularities of the plain caused by the mountains and valleys would appear exceedingly small in comparison with the extent of surface. But such considerations must also have occurred to the ancients; for the earliest conception among the Greeks of the form of the earth's surface was that of a flat disc surrounded by the river Okeanos, into which the sun plunged nightly. The first advance was made by Thales, who said the earth must have a point of support, and imagined it was borne by the water. Anaximenes supposed that a strong dense atmosphere supported the earth. Quite another idea prevailed in India, where the earth was believed to be borne on the back of an elephant. More correct views of the figure of the earth prevailed at an earlier period in other parts of the East, in Egypt and a part of Asia. The Egyptians and Chaldeans taught at the earliest period the spherical form of the earth, and Pythagoras appears to have adopted this idea from them.

This difference of conception need not, however, be wondered at when we remember that the Greeks seldom undertook long journeys, and knew of the lands outside Greece only from fabulous narratives. It was otherwise with the people of the East, who, through their frequent and extensive travels, learned at an early period to know the positions of the stars as guides, and attained to a more correct conception of the size and form of the earth. The Chaldæans already knew the circumference of the earth so nearly that they said a good walker would take three years to walk round it.

[1] From a series of papers in *Die Natur*, by Karl Maria Friederici.

Eudoxus was the first in Greece to recognise a symmetrical curvature of the earth's surface. He had noticed on long journeys that stars which at their greatest height (culmination) stood near the horizon gradually diminished in altitude, and finally disappeared; but on his return to those regions they again gradually became visible and assumed their previous altitudes. The circumstance that these altitudes of the stars changed regularly in proportion to the length of way travelled, led him to the conclusion of a regular curvature of the earth's surface. This conclusion being accepted, a simple method was indicated for measuring the circumference of the terrestrial sphere. For suppose a star reaches at a place, A, at its maximum a height of seven degrees above the horizon, if the observer move to another place, B, lying to the north, but in the same geographical longitude as A, and measure again the highest altitude of the same star, say six degrees; then the distance of the place A from B is equal to the 360th part of the whole circumference of the earth. Let the distance between A and B be now measured, and it will be found to be sixty-nine English miles; thus the entire circumference of the earth would be $69 \times 360 =$ about 25,000 miles.

Aristotle inferred, from physical and especially hydrostatic considerations, that the earth was spherical, since, he said, the water, which formed the larger part of the upper stratum of the earth, sought, by virtue of its weight and the mobility of its molecules, to approach as near as possible to the centre of the earth, it sought to assume the lowest position, and could never be in equilibrium until all parts of its surface were equidistant from the centre of the earth, *i.e.*, formed a globular surface. This inference, near as it comes to the truth, was yet in Aristotle's time only an unproved hypothesis; the existence of a centre exerting attraction in all directions was first recognised as probable at a much later period, Newton being the first to publish the conception.

The theory according to which the earth is a spherical body, was more and more generally accepted, and was put beyond doubt when the first circumnavigation by the Portuguese Magellan (1519) became known, an example followed, at short intervals, by almost all European nations. Thus the idea so generally accepted at a very early period that the figure of the earth must be spherical, was again revived about the end of the seventeenth century. The desire to ascertain, according to the above-described methods the circumference of this circle was also cherished by the ancients, and we have accounts of measurements taken for this purpose in the earliest times, of the most important of which we give some account.

The first determination known to us of the size of the earth was made by Eratosthenes in Alexandria in the third century before Christ. He observed at the solstice (the time of its greatest northern declination) in Alexandria, the greatest altitude of the sun above the horizon, and it was known that at that time the sun stood when at its greatest altitude, in the zenith at Syene (from which we may conclude that it could be seen in a deep well). Now since the altitude of the sun above the horizon is always equal to 90° minus its distance from the zenith, he thus required only to subtract the measured height from 90°, and thus found the distance from the zenith to be the fiftieth part of the whole circumference, or 7° 12′. According to this process the distance of the two places was regarded as a fiftieth part of the earth's circumference; and as that distance, according to the accounts of travellers, was 5,000 stadia, the whole circumference of the earth was equal to 250,000 stadia. Eratosthenes altered the result to 252,000 stadia, taking for the length of a degree, 700 stadia. Without considering the great inaccuracy of his altitude measurements, there are yet too many other formidable sources of error in this estimate of the earth's circumference, to allow it any claim to much accuracy. First there was the taking for granted that both places lay on the same meridian, which was not the case, since Syene lay three degrees east from Alexandria; and second, the distance of the two places reckoned at 5,000 stadia was too great.

A second investigation was made by Posidonius in the first century before Christ, but his result was still more erroneous than that of Eratosthenes. He observed the height of one of the brightest stars (Canopus in Argo) above the horizon. It reaches, at the time of its culmination at Alexandria, an altitude equal to the forty-eighth part of the circumference, while in Rhodes it was

visible just on the horizon. Hence it followed from a calculation similar to the above that Rhodes lay about $7\frac{1}{2}°$ farther north than Alexandria, and taking the distance of the two places to be 5,000 stadia, he reckoned the earth's circumference at 240,000 stadia. Here also we find the assumption that the two places lay on the same meridian, nearly $1\frac{1}{2}°$ wrong. But the chief source of error in this observation lay in ignoring the refraction of the atmosphere, which is subject to very great differences near the horizon, and makes the stars not only appear at greater altitudes than they actually have, but disturbs the places of the lower stars much more considerably than those of the upper. But we are not now in a position to be able to discover satisfactorily the extent of these sources of error in the results of Eratosthenes and Posidonius, since the stadium was of uncertain length, and we do not know in what relation it stood to our modern measures.

These are the only results worthy of notice that have reached us from these times, for then commenced the decay of science in the east, and it was only at a much later period that it flourished for a short time among the Arabs. The Kalif Al Maimon had obtained from the Greeks the writings of their philosophers, and turning his attention chiefly to mathematics and astronomy, he was incited to undertake an investigation into the mathematical figure of the earth. He formed the resolution of undertaking the measurement of a new degree, and collected for this purpose a great number of mathematicians. These selected an extensive and level tract of land, the Sinjar Desert, and made their measurements from one point, some going north, others south. The result was that the one party found a degree of the meridian to measure 56 Arabic miles, and the other $56\frac{2}{3}$. Al Maimon had the operation repeated in order to obtain a better result, but the figures obtained were the same. We have more certainty as to the unit of this measurement, the Arabic mile, than in the case of the stadium, but yet not sufficient for perfect accuracy, as appears from the following definition:—According to Alfraganus the Arabic mile contained 4,000 ells of twenty-four inches, the inch being the space covered by six barleycorns laid side by side. P. Snellius compared this measure of length with one of his own units of measure, and after numerous observations found that on an average eighty-nine barley-corns are equal to a Rhenish foot. By this proportion it is found that an Arabic mile is equal to 6472 Rhenish feet. It is usual to reckon the Rhenish foot as ·16103 of a toise, and thus the mean length of the measured degree would be 58710 toises, which is too great by 1700 toises according to recent measurements. The toise is equivalent to 6·3946 feet, or 1·949040 metre.

We have mentioned already that from the decline of science we had no other than this Arabic measurement to produce, and we may further add that the most boundless ignorance, particularly with reference to natural science, reigned supreme, especially among the European nations. But it was not enough that this inaccurate determination of the size of the earth should stand as the only one for centuries; very soon it, and with it the knowledge of the spherical form of the earth was forgotten. It was not until the sixteenth century that a French physician, Fernel, again undertook the measurement of a degree. He made use for this purpose of a peculiar apparatus, which would certainly not lead us to hope for an accurate result, but, nevertheless, through fortunate circumstances, he came very near to the truth. He had a waggon constructed which, by means of a piece of mechanism, registered the number of turns made by its wheel. With this he set out from Paris in the direction of Amiens until he had gone a degree of latitude northwards, calculated from the number of turns of the wheel the linear measure, and obtained for this distance, which, according to his observation, was equal to a degree, 57070 toises. This result, as we shall see further on, agrees very closely with later observations, which is all the more wonderful from his finding the geographical latitude of Paris too little by 12′. But since this resulted from a constant error of his instrument, he must also have observed the latitude of the other end of the arc as too little by the same amount, and thus since in the calculation only the difference of the two observations is used, these errors are without any influence in the result. The other sources of error, which arose from the unevenness of the measured distance, and evidently must have given too great a result, he eliminated by subtracting a certain quantity from his calculation, and he did this so successfully that, as we have said, his result very closely agrees with modern measurements.

Another investigation at this period into the circumference of the earth, without the help of the stars, but simply by terrestrial measurements, deserves mention. Starting from a point as high as practicable (a mountain top or high tower, whose height was known), the observer went as nearly as possible in a straight line until he reached a distance at which the top of the mountain or tower disappeared in the horizon. The distance of this point from the mountain or tower was then measured, and from simple trigonometrical considerations it will be seen that the square of this distance divided by the height of the mountain or tower would be equal to the earth's diameter. But in this method the irregular action of terrestrial refraction is so disturbing, that the point at which the mountain-top would seem to vanish must be very uncertain, and the result as to the diameter of the earth consequently very erroneous.

All the methods hitherto referred to as in use in ancient times and in the middle ages, for obtaining a knowledge of the size and figure of the earth, are deficient in trustworthiness, partly from their defective theory, but still more from the impossibility of then carrying out those practical geodetic operations which are

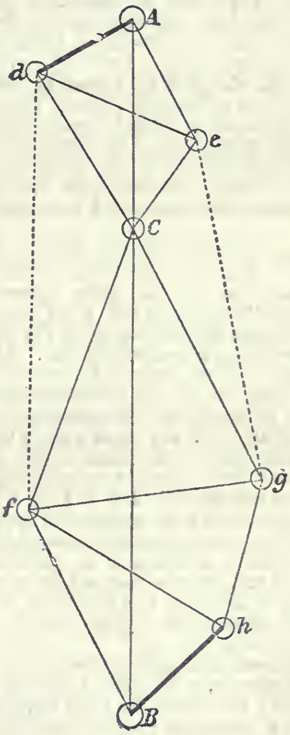


FIG. 1.

necessary for the solution of the problem with anything like accuracy. We shall see in the sequel with what wonderful accuracy it became possible to solve this important question.

The method of measuring degrees underwent, in the beginning of the seventeenth century, a fundamental reformation. Hitherto, in all such measurements, only the simplest points in the geometry of the circle had been applied, but Snellius of Leyden, making use of the properties of triangles, founded a new method for the measurement of a meridian arc, and applied it first in the year 1615—viz. the method of triangulation. His method, which has been followed ever since, possessed the invaluable practical advantage over the earlier methods, that it considerably reduced the most difficult operation in the measurement of degrees, namely, the measurement of a base line on the earth's surface. How it is possible, even in regions of very uneven surface to measure a large extent of a meridian arc with great accuracy, will be seen from the following short explanation. Suppose two places, A and B, one or more degrees of latitude distant from each other, but in the same meridian; if the unevenness of the intervening surface, from mountains and valleys, allowed of no direct measurement, one would proceed

in the following manner by the method of triangulation. First setting out from A (see Fig. 1) in whatever direction the character of the ground permits, a base-line A *d* is measured with the greatest possible accuracy. At the point A, the angle *d* A *e*, and at the point *d* the angle A *de* are observed with a circular instrument. Thus in the triangle A *de*, the adjacent side A *d* and the two other parts of the angles being known, the triangle can be computed. Place now in the straight line connecting A and B (in the same meridian) a point C, which can be seen from the points *d* and *e*; then we may, by means of the theodolite, measure at *d* and *e* the angles A *d* C and A *e* C.[1] Subtract now from these angles the previously observed angles A *de* and A *ed*, and we have now found in the second triangle, C *de*, the angles *d* and *e*. But then, also, the side *de*, as belonging to the first triangle, is known, and thus also the second triangle, and consequently its sides C *d* and C *e* are known. But if the triangles A *cd* and A *ed* are known, so are also the triangles A *d* C and A *e* C; consequently, also, the common side A C; and thus a part of the distance is measured. To obtain the length of the other part B C, a base B *h* will be measured from B, and by operations similar to the above B C will easily be found. As a test of the accuracy of the measurements, we may connect the first operation, starting from C *ed* towards *f* and *d*, and going on to B, and obtain by means of the agreement of the measured length B *h* with the calculated length of B *h* as a side of the triangle B *hf*, a proof of the accuracy of the measurements of base and angles. Should the length A B be very great and the intervening ground mountainous, a very great number of small triangles may be required: in which case, though the principle is exactly the same, yet in practice, on account of the numerous measurements necessary under such circumstances, unavoidable errors and inaccuracies will certainly accumulate.

As we have already said, Snellius, in the year 1615, was the first to measure a degree by the method of triangulation. He measured a base line on the plain between Leyden and Sonterwonde (316 Rhenish rods and 4 feet long), and by means of connected triangles obtained an arc of the meridian (between Alkmaar and Bergen-op-zoom) of 1° 11′ 30″. Although Snellius was in possession of an improved instrument (Galileo had already taught the use of the recently-discovered telescope[2] for astronomical purposes), yet his measurements were so inaccurate that he obtained far too small a result (55011 toises for a degree). He soon became convinced of the erroneous nature of his result, and seven years after repeated the operation, measuring in the neighbourhood of Leyden a base-line in the ice. Probably deterred by the multifarious and difficult numerical operations which were at that time connected with the working out of the calculation of this new measurement by means of arithmetic, he did not carry this out, but his successor, Muschenbroek, devoting himself to the execution of this work after revising the triangulation, found 57033 toises as the length of a degree in the Netherlands.

Although the method of triangulation used by Snellius was a great step in advance, yet it was a long time before it became generally adopted; for even in the years 1633 to 1635 a degree-measurement was carried out by Norwood between London and York after the old method. He used an improved instrument (a five-foot sector) and obtained as the difference in latitude of the two places 2° 28′, and the length of a degree 57424 toises. Newton, who shortly after began the elaboration of his theory of universal gravitation, did not, at all events, know this result, since he took as the basis of his researches the earlier very inaccurate results as to the dimensions of the earth, and since he found his calculations did not correspond with them he abandoned for a time his theory.

Soon after, Picard, at the instance of the Paris Academy of Sciences, undertook anew a meridian measurement, and that not only after the improved method of Snellius (since he measured all three angles of each triangle, and computed the length of the arc by pieces), but he also gave to the measuring instruments a hitherto unattained accuracy by the addition of a micrometer apparatus.[3] He measured on the meridian of Paris an arc of 1° 22′ 55″, and finding for the latitude of that place 49° 13′, with the, as we now know, wonderfully accurate result of 57060 toises for the length of a degree. When Newton, in 1682, learned the result of Picard's measurement, he resumed his calculations in gravitation, and had the satisfaction, after thoroughly revising his work, of seeing his theory of gravitation established. A few years afterwards he gave to the world his immortal work on the mechanics of the universe. For a short time Picard's dimensions of the earth were accepted as correct and were universally made use of. But while hitherto the measurements had reference alone to the discovery of the size of the earth —for its spherical form was taken as proved—there now began a new epoch in the solution of the second part of the problem—the true figure of the earth. Influenced by the fact that the length of a degree measured at different places on the earth always gave a different result—which could not in all cases be ascribed to inaccurate measurement—Picard had already broached the idea that the earth could not be a true sphere. Soon after, Newton, in his great work, showed, on the supposition that the earth existed originally in a fluid state, that on account of the rotation round the polar axis, the supposed spherical form must be more truly that of an elliptical spheroid, the polar diameter being diminished and the equatorial diameter increased. Shortly after Huyghens was led to the same result; and while Newton by his calculations found the polar diameter to be to the equatorial as 229 to 230, Huyghens, on the basis of less general theories, found the proportion to be 577 to 578. Indeed, although differing somewhat in magnitude (Newton's proportion was then accepted as the more correct), yet, in principle, they both led to the same result, viz., that the earth is flattened at the poles, so that the length of a degree near the poles must be greater than in the neighbourhood of the equator. Moreover, Newton had shown experimentally the flattening at the pole, by rotating a soft clay sphere quickly round its axis, by which it became flattened at its poles.

To this was now added another valuable proof. The French astronomer Richer, in the prosecution of his observations at Cayenne, found to his astonishment that his pendulum, which beat seconds in Paris, vibrated too slowly in Cayenne; he had to shorten it by a line in order to make it again beat seconds accurately. On his return to Paris he had to lengthen the pendulum again by the same amount, since it now went too fast. Newton perceived that this apparently insignificant fact was really of the highest importance, for he recognised that these different rates of oscillation were due in Paris to the less, and in Cayenne to the greater, distance from the centre of the earth. Cassini's discovery of the notable flattening of the planet Jupiter was an additional proof of the truth of Newton's theory. Yet it was not until the middle of last century that Newton's theory was generally accepted as an irrefragable truth.

(*To be continued.*)

[1] There is no necessity for the point C being taken in a line between A and B, nor any advantage even if it could be done. The angles need not be measured in the way here laid down.

[2] This remark seems to imply that Snellius used a telescope in measuring angles. The application of the telescope to circular instruments was a step taken by Picard.

[3] Picard adapted to his measuring instrument a telescope with cross-wires in its focus; this appears to be the only "micrometer apparatus."

THE VARIOUS METHODS OF DETERMINING THE VELOCITY OF SOUND

THE propagation of sound is a question with many bearings in the province of physics, and the researches of physicists in relation to it, though numerous, have left some points still under discussion. It is useful in the view of further inquiry to be furnished with a historical survey of what has been already done, and this is the object of a recent memoir by Dr. H. Benno-Mecklenburg, published in Berlin (a *résumé* of which to the following effect appears in the May number of the *Journal de Physique*).

The author has adopted the following classification of the methods that have been employed for measuring the velocity of sound:—

I. Methods requiring the measurement of a time and a course traversed.

1. Direct measurement of the velocity; the most ancient measurements of this kind were executed by P. Mersenne in 1657, by the Academicians of Florence in 1660,[1] by Walker[2] (in England), in 1698; by Cassini and Huyghens (in France), &c.

2. Method of coincidences, indicated by Bosscha,[3] and employed by Koenig.[4]

[1] Newton, "Philosophia Naturalis Principia Mathematicæ," II., Prop. XLVIII.-L.

[2] Laplace, "Mécanique Céleste," t. v. livre xii. p. 115.

[3] Tentamina, "Exper. Academ. del Cimento," 1738, xi. p. 116.

[4] *Philosophical Transactions*, 1698.

3. Apparatus of Neumann[1] and Le Roux.[2]

II. Estimation of the velocity of sound by the number of vibrations and the wave-length of musical sounds.

A. Direct methods :—

1. Method of Bernouilli, with sonorous tubes.
2. Method of Chladni,[3] with rods.
3. Method of Kundt.
4. Methods of Stefan[4] and Warburg.[5]

B. Methods based on the interference of sonorous waves :—

1. Method of Savart.[6]
2. Method of measurement of the wave-length with Quincke's interference tubes.[7]
3. Method of Zach.[8]
4. Method of beats.

The way in which the velocity of sound is affected by certain circumstances, especially intensity and pitch, requires further elucidation. Up till recent times it was believed, in accordance with the earlier observations and the theoretical formulæ of Newton and Laplace, that sound is propagated with a uniform velocity in the same medium, the temperature remaining constant; that the velocity of sound in air at zero, *e.g.*, is an invariable quantity. After an observation by Parry related by Sir James Ross, that the sound of a cannon was always heard sooner than the word of command to fire, Schröder van der Kolck was led by theory to a formula giving the velocity of sound in a gas as a function of the relation of the two specific heats and the degree of compression of the medium. This velocity would be greater the more intense and grave the sound, and would diminish with the distance traversed.

Regnault set himself to determine rigorously the ratio of the two specific heats of gases, with a view to deducing the mechanical equivalent of heat. He remarked that Newton and Laplace had assumed, in their formulæ, that the gases were perfect, *i.e.* (1), that they followed Mariotte's law exactly; (2) that their elasticity was not altered by surrounding bodies; and (3) that gas opposes no inertia to the transmission of sound-waves. Accordingly the propagation of sound was supposed the same whatever the intensity. Regnault's more complete formula indicated that the velocity is greater the greater the intensity of the wave.

Experiment proved that the intensity of the wave diminishes in a tube more rapidly the smaller the section. The wave is weakened by the reaction of the elastic walls of the tube, causing a considerable loss of *vis viva;* and the diminution of intensity, according to Regnault's formula, should result in diminution of velocity, which diminution must be more rapid the narrower the tube. This was confirmed by experiment.

As regards experiments with the human voice and wind-instruments, the following are the principal observations of Regnault; acute sounds are propagated with much less facility than grave sounds; in very wide pipes, it is necessary to sing with a baritone voice in order to be well heard; the fundamental sound is heard before its harmonics, which succeed in order of pitch, and the timbre is thus altered. The velocity was found independent of pressure, as indicated by the formulæ. Lastly, with different gases, the velocities are inversely proportional to the square roots of the densities.

In connection with the foregoing, it is interesting to compare the results that have been obtained by Kundt.[9] The idea of his method was suggested by Chladni's figures. A tube of glass is used about 2 m. long, containing a certain quantity of lycopodium powder (distributed as regularly as possible), and closed at the two ends. You rub the tube longitudinally, so as to produce a sound. The powder is then seen to accumulate at the nodes of vibration, so that the sonorous waves of the gas are, in a way, rendered visible. The distance from one node to the next being half a wave length, suppose that we have twelve in the tube; the length of the tube vibrating transversely, will be the half of a wave-length in the *glass.* Under such conditions, then, the length of half a wave in the glass is sixteen times the length of half a wave in the air. It will follow that the velocity of sound in the glass is sixteen times that in air. Other gases may be used in the tube, and the velocity of sound similarly found in them.

By a simple modification of the apparatus, this method gives the velocity of sound in a large number of solid bodies, and the results agree pretty closely with those found by different methods.

But Kundt's method does not give sufficient precision in respect to the delicate questions investigated by Regnault. The wave-lengths measured never go beyond about 45 mm., making the 0·0001386 part of the course traversed by sound in a second; hence an error of 0·1 mm. made in the measurement of a wave-length would lead to an error of $\frac{11}{16}$ m. in the result sought. With this reserve, Kundt's results may be here noted.

1. The length of sonorous waves, and consequently the velocity of sound, diminishes proportionally to the diameter of the tube, when this is less than a quarter of the length of undulation.
2. In narrow tubes a high sound is transmitted more rapidly than a grave one, and the diminution of the velocity of sound is in inverse ratio to the wave-length.
3. The velocity of sound is independent of the pressure in a wide tube, but increases with it in a narrow one.

It will be seen that these latter results are in contradiction with those found by Regnault.

It may be generally affirmed that every influence which tends to increase the *vis viva* of the molecules of the sonorous medium has an accelerating action on the velocity of sound, and every influence tending to diminish the *vis viva* diminishes also the velocity.

The causes affecting the velocity of sound are (it is shown) various. *In an indefinite medium* they are :—1. The temperature of the medium; 2. The quantity of foreign substances found in it, *e.g.*, water-vapour; 3. The pitch of the sound; 4. The direction and force of the wind; 5. In solid bodies, the direction of the sound in relation to the molecular structure.

In sonorous tubes :—

6. The diameter; 7. The curvature; 8. The rugosity of the interior surface; 9. The thickness of the walls.

The action of the following additional causes is still disputed :—

1. The intensity of the sound; 2. The length of course traversed; 3. The substance forming the tubes.

There is complete disagreement between Regnault, Schröder, Kundt, and Seebeck, as regards the influence of the pitch of the sound. Regnault affirmed merely that an acute sound is transmitted more easily, but not more rapidly, than a grave sound; Schröder finds that the velocity diminishes as the acuteness increases; Kundt and Seebeck reach the contrary result. Fresh experiments are required to settle this important question.

[1] *Pogg. Ann.*, t. xcii. p. 485.
[2] *Comptes Rendus*, t. lv. p. 609.
[3] *Pogg. Ann.*, t. cxxviii. p. 307.
[4] *Comptes Rendus*, t. lv. p. 662.
[5] Chladni's "Acoustics."
[6] *Sitzungsberichte der Wiener Akademie*, t. lvii. pp. 197 and 708.
[7] *Pogg. Ann.*, t. cxxxvi. p. 285.
[8] *Comptes Rendus*, t. vii. p. 1068.
[9] *Pogg. Ann.* t. v. p. 496.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

A NEW supplemental charter having been granted to the University of London a few months since, on the joint application of the Senate and of Convocation, empowering the Senate to admit women to graduate in its several faculties (Arts, Science, Law, Medicine, and Music), on such conditions as the Senate, with the concurrence of the Home Secretary, should deem expedient, the Senate lost no time in passing a resolution which made all the existing regulations, relating not only to graduation, but also to the various honours and rewards granted at the several examinations open to female as well as to male candidates. This resolution having been now approved by the Home Secretary, female candidates will be admitted forthwith to the matriculation examination; and all such as have already passed the general examination for women will be considered as having matriculated, and will be admissible (after the required interval) to the first degree examination in either of the faculties. Further, with a view to the special encouragement of female candidates desiring to go through a regular academical course, the trustees of the Gilchrist Educational Trust have instituted two exhibitions, one of 30*l.*, the other of 20*l.*, per annum, tenable for two years, to the female candidates who pass highest in the honours division at the matriculation examination; and two exhibitions, one of 40*l.*, the other of 30*l.*, per annum, tenable for two years, to the female candidates who pass highest at the first B.A. examination (provided that they obtain in the first case two-thirds, and in the second three-fifths, of the total number of marks), to assist them in pursuing their studies at some collegiate institution approved by the trustees; with the further reward of a gold medal of the value of 20*l.* (or of a book-prize of the same value) to the female candidate who passes highest at the second B.A. examination, if she obtains not less than two-thirds of the total number of marks. These rewards are quite independent of those granted

by the University, and may be held in conjunction with them. Further particulars may be obtained by application to the Registrar of the University, London, W.

SCIENTIFIC SERIALS

Annalen der Physik und Chemie, No. 7, 1878.—Theory and experiment having given different results for the heat-conduction of certain (polyatomic) gases, Prof. Wüllner supposed the reason to be that the values were not comparable, because they related to different temperatures. He shows from experiment that the ratio of the two specific heats varies with the temperature. For those gases whose specific heat at constant pressure does not vary with the temperature, the variation is of about the same order as the divergence of gases from Mariotte's law. For gases, whose specific heat varies with the temperature, the ratio of the specific heats varies in a greater degree, and approximately so that the difference of the specific heats at 0° and 100° is constant. Herr Wüllner finds here an explanation of the discrepancy.—In a lengthy paper Herr Hittorf vindicates the affirmation that "electrolytes are salts," in reply to Dr. Bleekrode's criticism. We note, also, papers on the energy of reciprocal action, by Herr W. Weber, and on the law of storms, by Herr Schröder.

No. 8.—This number opens with a paper by Herr Herwig, on the amount of electricity required for full charge of a condenser platina water-cell, and on the distance of the molecules in liquid water. The upper limit he deduces for the latter is 0·186 millionth mm.; which agrees well with other determinations, Lorenz 0·1 millionth mm., Thomson (for lower limit) 0·05. He finds in the result a confirmation of the hypothesis of rotatable electrolytic molecules.—In a paper on the wandering of ions, M. Kirmis shows that the amount of transference of copper from the solution of its sulphate salt increases with decreasing concentration.—Investigating the history of the invention of the pendulum clock Herr Gerland considers that Bürgi and Treffler have not the least claim to this merit. It belongs to Galileo and Huyghens, who made the discovery independently. As the former, however, came on it fifteen years earlier, the pendulum clock is properly his work.—Herr Auerbach endeavours to show that Grossmann's vowel theory applies not to actual vowels, but to typical ideal clangs, and when the changes thus rendered necessary are introduced into it, it affords, if not incorrect results, nothing new as against Helmholtz's theory.—Herr Bauer has a paper on summation tones as difference and beat-tones from the over-tones of the primary tones; and Herren Nilson and Pettersson write on the production and valence of beryllium.

Journal de Physique, August, 1878.—In this number M. Bouty explains the construction and use of electric diagrams, for representing as completely as possible all the peculiarities of an electric field.—Some curious experiments with the electric tourniquet are described by M. Bichat.

SOCIETIES AND ACADEMIES

Paris

Academy of Sciences, September 9.—M. Fizeau, president, in the chair.—The following papers were read:—On some phenomena of a vaso-motor action observed in the course of researches on the physiology of the excito-secretive nerves, by M. A. Vulpian.—On some new effects produced by the telephone, by M. du Moncel.—On the new palæozoic group of Dolerophylleæ, by M. G. de Saporta.—On a new gyroscopic apparatus, by M. Gruey.—On the accumulation of magnetism on the summit of hemispheric poles, by M. L. Romain.—On the waves of the high sea, by M. Ch. Antoine.—Some notes, by MM. Vasseur, Lassalle, and Cameron, regarding aerial navigation.—An additional paper by M. Giraud respecting the treatment of cholera.—A note by M. F. Bettelhauser respecting the various means employed for the destruction of phylloxera.—Prof. Asa Gray, who has been nominated a correspondent of the Botanical Section, presented the first part of his "Synoptic Flora of North America."—Rectification of the position previously assigned to the new planet discovered during the eclipse of the sun on July 29, by Prof. J. Watson, and note on the observation of a second star seen under the same circumstances.—On a new method to decompose numbers into square binary sums and its application to indetermined analysis, by M. E. de Jonquières.—On the depression which, at the surface of a horizontal elastic and isotropous ground, is produced by a weight deposited thereon, and on the distribution of this weight between its various points of support, by M. J. Boussinesq.—On the variations of intensity which take place in a current if the pressure between the two contacts, which complete the circuit is modified, by M. Treve.—On the application of the telephone to the determination of the magnetic meridian, by M. H. de Parville.—On the constitution of the inactive glucose of crude cane sugar and molasses, by M. U. Gayon.—Comparison between the *Balaena* (*Macleayius*) *australiensis*, of the Paris Museum, and the *Balaena biscayensis*, of Naples University, by M. Fr. Gasco.—On the reproduction of the *Hydra*, by M. Korotneff.—On the comparative structure of *Lepidodendron* and *Sigillaria*, by M. B. Renault.

Vienna

Imperial Academy of Sciences, July 18.—Critical researches on the species of the natural family of the stags (Cervi), (third part), by Dr. Fitzinger.—Influence of temperature on the galvanic conductivity of liquids, by Drs. Exner and Goldschmiedt.—On some new or imperfectly-known fish-species, by Dr. Steindachner.—On the Orthoptera of Istria, by Dr. Kraus.—On new Cymothoides, by Herr Kölbel.—On the form of spark-waves, by Prof. Mach and Herr Weltrubsky.—The origin of tubes in the Nostoc-colonies in Blasia, by Herr Waldner.—Influence of the density and the temperature on the spectra of vapours and gases, by Herr Ciamician.—Action of chloroform and ether on respiration and circulation of the blood, by Dr. Knoll.—On Roussin's binitro-sulphuret of iron, by Herr Demel.—On a new method of quantitative investigation of gold and silver alloys, by Herr von Jüptner.—On the spinal ganglia and cord of Petromyzon, by Herr Freud.—Rocks from Greece, by Herr Becke.—On nitrocuminol and its derivatives, by Prof. Lippmann and Herr Strecker.—On compounds of nickel and cobalt chloride with tar bases, by Prof. Lippmann and Herr Vortmann.—On the Malabrian kinogumi, and a new substance got from it, kinoïn, by Herr Etti.—On Borneo camphor, by Herr Kachler.—On cinchonidin, by Herren Skraup and Vortmann.—Action of water on the haloid compounds of alcohol radicals, by Herr Niederist.—On the geological formation of the western part of Central Greece, by Prof. Neumayr.—On the geological formation of the island Eubœa, by Dr. Teller.

CONTENTS

THURSDAY, SEPTEMBER 26, 1878

THE SUPERFICIAL GEOLOGY OF SOUTH-WEST LANCASHIRE

The Superficial Geology of the Country adjoining the Coasts of South-West Lancashire. By C. E. De Rance, F.G.S. 1877.

THE memoir of the Geological Survey by Mr. De Rance recently published is an interesting contribution to our knowledge of the superficial deposits of the area between the Mersey and the Ribble, which carries the classification of the Cheshire Plain as far to the north as Morecambe Bay. The whole of this district is covered with glacial drift and recent sands, gravels, and peat-bogs, except here and there where the solid rocks come to the surface in the hills. The drift forms an inclined plane dipping from the hills towards the sea, and probably deposited during subsidence upon an old rocky plain of marine denudation, bounded to the east by a line passing from Eccleston to Euxton and Ribchester, and thence through Broughton, Garstang, and Cockerham to the present sea-margin. Were the superficial deposits stripped off this area, the rock-surface would be seen to be not very far from the present sea-level, although the surface of the ground is often 170 feet above it. This plain also dips gently seaward, and has been worn into hollows by the denuding forces before the glacial period. Very generally it has been cut up into hills and valleys by pre-glacial streams, as, for example, the buried valley of the Mersey described by Mr. Mellard Reade, now filled with 200 feet of sands, gravels, and clays. These buried valleys may be traced inland, rising nearer to the present surface of the ground as we approach the high ground, until at last their tributaries come to the active surface in the higher hills, and are traversed by the same streams as those now finding their way to the surface, and through the accumulation of drift filling their ancient lower courses. It seems tolerably certain that the hill and valley system of Lancashire and Cheshire was produced by sub-aërial agents before the glacial period, and that the ice merely acted on the solid rock by rounding off and smoothing the raw edges left by the streams and rivers. Indeed, as a rule, it may be said that the relative importance of the agency of rain and rivers, and of ice in scenery making is precisely that of chisel and sand paper, the one carves, while the other rounds off, smooths, and polishes. But whatever view may be held of the cause of this uneven surface below the mantle of the drift, it is a most important fact to be noted, that the surface configuration bears little or no relation to the rock-surface below, as engineers have frequently found out by experience in making reservoirs. In one case, for example, the "puddle trench" had to be carried down 160 feet, so as to render a ravine filled with drift water-tight, and this ravine, with the big boulders at the bottom left in the bed of the stream, by which it was hollowed, was intercepted twice in the course of the works.

The drift of the district under consideration is treated by Mr. De Rance in three divisions, the lower and upper boulder clays, separated from each other by the middle glacial marine sands and gravels. There appears to be no important physical difference between these clays, and their relative age can only be ascertained by their relation to the sands above mentioned. This, however, is not an infallible guide, because there are lenticular strata of sand and gravel intercalated here and there in the boulder clays. According to Mr. De Rance they are absent from the base of the lower boulder clay, a position which Mr. Binney has shown them to occupy in other districts in Lancashire and Cheshire. Our author notices also the glacial striæ, *roches moutonnées*, and the moraines on the higher grounds overlooking his area, and points out very justly (p. 46) that "the till" and "lower moraine drift" of other districts may have been formed at the same time that the lower boulder clay was being accumulated. It may also be pointed out that the local glaciation of North Wales and of the Pennine chain, and of the hills of Cumberland may have been produced while the upper boulder clay was being formed. Nevertheless, we cannot obtain an accurate idea of the relation of the various glacial phenomena to one another in point of time in different parts of Britain, until we can ascertain the sequence and extent of the changes of level, which has not as yet been made out. To my mind three great changes only have been proved to have taken place over a wide area: two periods when Britain stood at a higher level than it does now, with an intervening period, during which the region north of the lower Thames and Severn was submerged to a depth of 1,200 to 1,500 feet on the flanks of Snowdon. There were three corresponding climatal changes, the first period of elevation being marked by a very low temperature; the second, or that of depression, by temperate conditions; and the third or last period of elevation being also marked by severe climatal conditions. It is obvious that, during changes such as these, the sands, gravels, and clays, termed "glacial drift," would be so extremely complicated and so various in different places, that it is difficult, if not impossible, to ascertain the contemporaneity of the more minute sub-divisions of the glacial strata. While sands and shingle were being accumulated along the coast-line, melting icebergs were dropping their burdens to form boulder-clays in the adjacent sea, and on the land the moraines of the retreating glacier were being heaped together, or the advancing glacier was ploughing its way downwards. All these operations were going on simultaneously in different parts of the glacial area of Great Britain, and their results are rendered infinitely more complex by the oscillations of the level of the land, which may have been local, and by changes of climate as yet imperfectly understood. From these considerations it is evident that the clays and shingles and sands cannot be severally classified together, excepting in the strict *homotaxial* sense, and apart from all ideas of contemporaneity, and that the sequence of the minute divisions of the glacial period in Scotland, published by Dr. James Geikie in his able work, and based upon the relation of clays to sands and gravels, cannot in the nature of things apply to Lancashire, or any other areas beyond Scotland. Mr. De Rance has acted prudently in confining his attention to the drifts of the area treated in the memoir without dealing with the general question, which, to my mind, is

not yet sufficiently known to be dealt with in any other fashion than that of the threefold classification of Ramsay, Jamieson, and Lyell. The refinements on this classification attempted by several authors are based upon phenomena which have not yet been proved to be other than local.

One of the more interesting sections of this memoir relates to the growth and accumulation of peat; the author's conclusion that the rate at which peat grows is very uncertain and dependant on local conditions, is confirmed by the recent researches of Dr. Angus Smith, and his observations regarding the manner in which forests have been destroyed by its growth are probably true. The presence, however, of large oaks at great elevations in Britain need not necessarily "point to warmer summers than at present," but may be accounted for by the fact of their having grown in a primeval forest, one under the shelter of another, thus attaining a height and reaching a size which they could not do on our bare hills exposed to the high winds. An example of this may be seen in the fine tall trees growing in the sheltered valley in which Furness Abbey stands, as compared with the stunted growth on the exposed hill-sides around. Nor can the Scotch firs on the peat of the south of England be taken to prove the inclement winters of the prehistoric period, since they now flourish also in the south of England at levels but little above the sea. For the same reason also the peat bogs cannot be looked upon as proving a lower temperature then than now. In Somersetshire the turf moss extending from Glastonbury to Highbridge is growing at the rate of from 4 to 6 feet in fifteen years, so that the places where the peat is cut are filled up in that time. These, however, are unimportant points in a valuable memoir which deals with the district in a very comprehensive manner and in a small space.

It should be remarked, in conclusion, that the price of 17*s*. for a small octavo of 139 pp. in paper wrappers is without precedent and unreasonable, and that the policy of absurdly high prices for Survey Memoirs, which, as it appears from the two last publications, is being pursued by the Stationery Office, is certain to restrict the sale, and thus render them comparatively useless. They cannot be expected to pay their cost any more than the Reports of Parliamentary Commissions; they ought to be issued at a mere nominal sum, and distributed with a liberality like that shown in similar cases by the American Government.

W. BOYD DAWKINS

SCIENTIFIC HORTICULTURE

The Parks and Gardens of Paris. By W. Robinson, F.L.S. Second Edition, Revised. (London: Macmillan and Co., 1878.)

THE Science of Horticulture are words often used and too often misused. That there is science in horticulture, or that it is capable of being based on scientific principles, cannot be denied. There is sufficient in the cultivation of plants and flowers, and in their proper disposition in the garden, to occupy a highly refined and cultivated intellect. A garden, according to its dimensions and capabilities, has always been, and is still, more or less a delight to its owners. Like everything else, the taste exercised in the science of gardening has in different ages shown itself in various ways. The hideous clipping of hedges and shrubs into the forms of animals, birds, &c., still occasionally to be seen in some old English gardens, are records of one of the worst periods of gardening in this country, and the modern system of carpet or ribbon bedding, aptly termed by Mr. Robinson the "coloured cotton handkerchief" style, is not one whit more defensible, but rather, we should say, even more reprehensible, considering what has been done and written of late with the view of elevating public taste in matters of science and art generally.

To no book can we point with so much satisfaction on the subject of laying out or grouping plants or trees in parks or gardens as to that which now lies before us. No writer on this, or on kindred subjects, has discoursed more pleasantly than has Mr. Robinson. That his theme has inspired his pen as that of a ready writer is self-evident; and that he is a true lover of plants for their own sake is also apparent from his frequent references to individual species. But he is something more than this, for it will be found from a perusal of his book that he possesses a thorough knowledge of his subject.

It must not be imagined from the title of the book that the parks and gardens of Paris are, in their entirety, held up for our admiration and imitation. On the contrary the author distinctly points out, and separates the good from the bad, the true from the false, retaining, so to speak, the wheat and consuming the chaff with the fire of a powerful criticism. Notwithstanding this, there can be no two opinions as to the general superiority of the French capital over that of our own in point of picturesque beauty. No visitor to Paris—and no one probably has ever visited that city without visiting also the Bois de Boulogne—can have failed to compare in his own mind the sylvan beauty of the Bois, and the ragged uncared-for appearance of our own London parks; and the contrast is even greater in the squares and gardens of the two capitals, and yet as Mr. Robinson points out, there are excellent sites and splendid opportunities in London to make it a city suitable for other purposes besides those of business and toil. To properly effect this of course architecture and horticulture must join hands. Nevertheless much depends upon a proper provision by the architect for the horticulturist and landscape gardener to exercise their skill. As an illustration of how this may be done, Mr. Robinson draws attention to the new avenue between the new Opera House and the Rue de Rivoli in Paris, and points out that they "have not only been made without cost to the town, but even with a balance on the right side, the vastly increased value of sites for business premises in these new and noble streets having more than repaid the cost of their formation and the removal of the old houses through which they were driven. Abroad, every little capital possessing enough interest to occupy one for two hours, is furbishing up its attractions, while we in London are neglecting advantages the like of which are not possessed by any other city in Europe. The river, the bridges, the suburbs, the surroundings are infinitely superior to Paris, but owing to stupid absence of plan many of the good points are lost, many of the best suburbs being unknown ground even

to thousands of Londoners owing to the impossibility of reaching them without struggling through narrow and mean streets and roads."

The first chapter of Mr. Robinson's book opens with a consideration of the Bois de Boulogne, and from it we may learn much that is good both in the way of artistic grouping, planning, and in the selection of individual plants. Unlike what we too often see in public parks and gardens, the vegetation along the banks of the lakes in the Bois is properly diversified, "so that at one place we meet with conifers, at another rock shrubs, at another magnolias, and so on, without the eternal repetition of common things which one too often sees." The author next proceeds to point out the great advantage of permanent planting over that of plants which show only a fleeting annual display. In spring the early bloom and budding leaves are in themselves things of beauty, and are even more so when seen collectively or in company with each other. In summer they furnish an infinite variety of form and consequent depth of tone, while in autumn "the number and richness of the tints of the foliage afford a varied picture from week to week, and in winter the many graceful forms of the deciduous trees among the evergreen shrubs and pines offer as much to interest an observant eye as at any other season."

On the subject of sub-tropical gardening, which is well exemplified in the Park Monceau, the author writes in his pleasantest manner, as the following extract will show:—"We may be pleased by the wide spread of purple on a heath or mountain, but when we go near and examine it in detail we find that its most exquisite aspect is seen in places where the long moss cushions itself beside the Ling, and the fronds of the Polypody peer forth around little masses of heather. Everywhere we see nature judicious in the arrangement of her highest effects, setting them in clouds of verdant leafage, so that monotony is rarely produced—a state of things which it is highly desirable to attain, as far as possible, in the garden. We cannot attempt to reproduce this literally, nor would it be wise or convenient to do so; but assuredly herein will be found the chief source of true beauty and interest in our gardens; and the more we keep this fact before our eyes the nearer will be our approach to truth and success.

"We should compose from nature, as landscape artists do. We may have in our gardens—and without making wildernesses of them—all the shade, the relief, the grace, the beauty, and nearly all the irregularity of nature. This bold growth of 'fine-foliaged plants' has shown us that one of the greatest mistakes ever made in the garden was the adoption of a few varieties of plants for culture on a vast scale, to the exclusion of interest and variety, and too often of beauty or taste. We have seen how well the pointed, tapering leaves of the cannas carry the eye upwards; how refreshing it is to cool the eyes in the deep green of those thoroughly tropical castor-oil plants, with their gigantic leaves, how noble the Wigandia, with its fine texture and massive outline looks, after we have surveyed brilliant lines and richly painted leaves; how, too, the bold tropical palm leaves beautify the garden. In a word, the system has shown us the difference between the gardening that interests and delights all beholders, and not the horticulturist only, and that which is too often offensive to the eye of taste and pernicious to every true interest of what has been called 'the purest of human pleasures.'"

Notwithstanding the general interest kept up throughout Mr. Robinson's book, no part is of greater interest than the chapter on the cultivation of mushrooms in the caves under Paris, where, at a depth of from sixty to eighty feet below the surface of the ground, in old stone quarries, this edible fungus is grown systematically on a very large scale. These caves furnish not only the daily supply of the Paris markets, but to a large extent those of England and other countries also, preserved mushrooms to the extent of 14,000 boxes annually being exported to this country by one house alone. It is estimated that in and around Paris the daily production of mushrooms amounts to about twenty-five tons, of the value of about 1,000*l.*, or close upon 400,000*l.* per annum. One large grower near St. Denis is described as employing nineteen horses and fifty men. Mushrooms are very extensively used in France, not only in their fresh state, but preserved in various ways, either by tinning, bottling, preserving in butter or oil, or reducing to powder.

The book is exceedingly well printed and carefully got up.

OUR BOOK SHELF

Elements of Physiography. By Prof. D. T. Ansted, M.A., F.R.S. (Allen and Co.)

We are glad to see a book on physiography from the pen of Prof. Ansted, whose name has so long been associated with the literature of physical geography. If the new subject, however, is to be treated as it is in the present work, we fail to see the advantage of any change. The distinctive parts of physiography are all got through in an introduction of some eighteen pages, which we fear are too much written up to the *ipsissima verba* of the syllabus issued by the Science and Art Department to be of much educational value.

And, in fact, it is on this point that we find most fault with the book. Take these two paragraphs concerning aqueous vapour, pp. 101, 102, and note the absence of the why in every case.

"Aqueous vapour is frequently rendered visible as mist, fog, or cloud. These are varieties of the same condition. Mist is formed when currents of air of different temperature, both containing invisible vapour, meet near the earth. In valleys such mixtures are very frequent, and in mountainous countries very striking. Fogs are formed in the same way in temperate climates at various seasons, and hang over shores and the mouths of certain streams.

"Clouds are produced when mixtures of currents take place at some distance above the ground, and the visible vapour is entirely detached from the earth. There are several varieties of clouds, some floating at a height of many miles, some hovering in large masses in mid-air, some drooping downwards and almost touching the earth. They are rarely alike long together. They assume, as we know, the most fantastic shapes, and are occasionally decorated with brilliant tints of colour! It is only the clouds that form in large masses and approach the earth that dissolve into heavy and long-continued rain, but all clouds are capable of yielding rain, and drops sometimes, though rarely, fall through air perfectly clear and cloudless."

A student had better not be taught at all than be taught in this manner, and, in fact, a student of average intelligence, after reading such a string of assertions without the least

attempt at explanation—while everyone of them might have been made part of his nature for ever afterwards, by giving the simple reason in each case—if he does not doubt the competence of his teacher, will have no more to do with him.

La Spectroscopie. By A. Cazin. (Paris: Gauthièr-Villars.)

THE talented author of this work has passed away since the MS. was completed. This is by no means a systematic treatise, but it contains a large amount of information—some of it out-of-the-way information—and it will repay perusal. As much of M. Cazin's information on the celestial applications has been gathered from Secchi's works its complete accuracy is not to be relied on, but the explanation given of the different methods employed is very clear.

The part of the book which perhaps will be read with the greatest interest is that dealing with radiation and absorption spectra. In this part the author includes a notice of much of his own work, which is of great interest and importance. The *historique* of the question as to the existence of double or multiple spectra is interesting, and the author's leaning is against the view held by Ångström and Thalèn. He gives special observations of his own concerning nitrogen, and indeed was engaged on an extension of them at the time of his lamented death.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Sun-spots and Rainfall

IF the sun-spot cycles were all of the same length the simplest way of comparing the yearly sun-spot areas with the yearly amounts of rainfall in order to see whether the two phenomena were more or less numerically related, would be to find the annual means of the greatest possible number of cycles, care being taken to place the years of maximum and minimum in the same two groups respectively, and the intervening years in due succession. But as the sun-spot cycles are not of the same length, we must in employing the method of arithmetical means, make some modifications calculated to suit the circumstances of the case.

1. We may, for example, take any cycle whatever may be its duration, and commencing with its first and ending with its last year, compare with the sun-spots the rainfall at any station, or the means of the rainfall at a number of stations for the same years.

2. If some of the cycles be of the same length, we may take these alone and compare them with the rainfall, still taking care that the years of maximum and minimum sun-spots shall be respectively in the same groups.

3. The average length of the sun-spot cycle being, as far as is yet known, about eleven years, we may take any number of cycles of different lengths and make two separate comparisons, in one of which the maximum years are to be placed in the same group, and in the other the minimum years in the same group, the number of the other groups preceding and following the epochal groups being determined by the fact that the mean interval from minimum to maximum is about 3·7 years, and from maximum to minimum about 7·4 means.

I have tried these and several other methods, with, I think, considerable success. The method just mentioned (3), which is the old one of arithmetical means somewhat modified to meet the conditions of the case, possesses several advantages, one of the most important of which is that it enables us to compare directly the rainfall with the sun-spots in the epochal years, and in at least two years before and two years after them, thus affording a fair comparison for nearly the whole cycle of eleven years—a most essential point, which, it would appear, has been overlooked by some writers on the subject.

Having in my last communication (NATURE, vol. xvii. p. 448) given an example of the first half of the above method, together with the results obtained by it for several localities, I intended on this occasion to submit only one or two examples of the second half. But finding that the method has been criticised by Mr. Buchan, and having now Prof. Wolf's latest edition of his relative sun-spot numbers, as well as the rainfall of Madras for 1877, it may be proper to give instances of the application of the whole process.

Let us begin with Wolf's relative numbers, which are so arranged in the following table that those for the years of maximum sun-spot, from 1811 to 1877, are all in the sixth line. This table has already been given by Mr. Buchan (NATURE, vol. xvii. p. 506), but as references will be made to it now, and also in future discussions of the rainfalls of various parts of the globe, it is desirable to reproduce it here.

TABLE I.—*Wolf's Sun-spot Numbers (Maximum Years in Sixth Line).*

Year.	1811–1823.	1824–1836.	1832–1844.	1843–1855.	1855–1867.	1865–1877.	Means.	Mean Cycle.	Variation.	Year of Cycle.
1	1·6	8·1	26·3	13·1	7·7	31·4	14·7	—	—	—
2	4·9	16·2	9·4	19·3	5·1	14·7	11·6	14·9	−33·9	1
3	12·6	35·0	13·3	38·3	22·9	8·8	21·8	25·4	−23·4	2
4	16·2	51·2	59·0	59·6	56·2	36·8	46·5	48·8	0·0	3
5	35·2	62·1	119·3	97·4	90·3	78·6	80·5	77·0	+28·2	4
6	**46·9**	**67·2**	**136·9**	**124·9**	**94·8**	**131·8**	**100·4**	**91·9**	**+43·1**	**5**
7	39·9	67·0	104·1	95·4	77·7	113·8	83·0	83·0	+34·2	6
8	29·7	59·4	83·4	69·8	61·0	99·7	65·7	65·6	+16·8	7
9	23·5	26·3	61·8	63·2	45·4	67·7	48·0	49·0	+ 0·2	8
10	16·2	9·4	38·5	52·7	45·2	43·1	34·2	34·6	−14·2	9
11	6·1	13·3	23·0	38·5	31·4	18·9	21·9	24·6	−24·2	10
12	3·9	59·0	13·1	21·0	14·7	11·3	20·5	22·5	−26·3	11
13	2·6	119·3	19·3	7·7	8·8	7·0	27·5	—	—	—

It will be seen that each of six of the columns in the above table gives the sun-spot numbers for thirteen years, and that the first term of what is called the "mean cycle" is obtained from the expression, $\frac{a + 2b + c}{4}$, where a, b, c, are the means of the sun-spots for the first, second, and third years of the thirteen years, the remaining terms being obtained in a similar manner. The "variation" is simply the deviations from the mean value of the sun-spots for the "mean cycle."

The next table gives the sun-spot numbers, from 1816 to 1872, arranged so that the minimum years are in the eighth line:—

TABLE II.—*Wolf's Sun-spot Numbers (Minimum Years in Eighth Line).*

Year.	1816–1828.	1826–1838.	1836–1848.	1849–1861.	1860–1872.	Means	Mean Cycle.	Variation.	Year of Cycle.
1	46·9	35·0	119·3	95·4	94·8	78·3	—	—	—
2	39·9	51·2	136·9	69·8	77·7	75·1	73·1	+ 23·3	1
3	29·7	62·1	104·1	63·2	61·0	64·0	64·3	+ 14·5	2
4	23·5	67·2	83·4	52·7	45·4	54·4	54·6	+ 4·8	3
5	16·2	67·0	61·8	38·5	45·2	45·7	44·2	− 5·6	4
6	6·1	59·4	38·5	21·0	31·4	31·3	30·8	− 19·0	5
7	3·9	26·3	23·0	7·7	14·7	15·1	17·3	− 32·5	6
8	**2·6**	**9·4**	**13·1**	**5·1**	**8·8**	**7·8**	**12·7**	**− 37·1**	**7**
9	8·1	13·3	19·3	22·9	36·8	20·1	24·4	− 25·4	8
10	16·2	59·0	38·3	56·2	78·6	49·7	51·6	+ 1·8	9
11	35·0	119·3	59·6	90·3	131·8	87·2	80·7	+ 30·9	10
12	51·2	136·9	97·4	94·8	113·8	98·8	94·6	+ 44·8	11
13	62·1	104·1	124·9	77·7	99·7	93·7	—	—	—

We can now compare the rainfall of Madras with the values of the sun-spots in Tables I. and II., except for the years 1811 and 1812, for which there are no observations:—

TABLE III.—*Rainfall of Madras* (*Maximum Years in Sixth Line*).

Year.	1811–1823.	1824–1836.	1832–1844.	1843–1855.	1855–1867.	1865–1877.	Means.	Mean Cycle.	Variation.	Year of Cycle.
	in.	in.	in.	in.	in.	in.	in.	in.	in.	
1	—	33.7	18.4	*50.3	32.3	41.6	35.3	—	—	—
2	—	56.0	*37.1	65.4	*47.0	51.4	51.4	45.3	− 3.2	1
3	45.1	60.7	39.0	38.1	52.9	*24.4	43.4	48.3	− 0.2	2
4	32.4	88.4	41.5	79.8	48.5	41.4	55.3	51.3	+ 2.8	3
5	56.0	37.9	44.8	81.0	55.1	32.3	51.2	51.2	+ 2.7	4
6	**41.2**	**36.9**	**49.3**	**54.8**	**27.6**	**74.1**	**47.3**	**48.1**	**− 0.4**	**5**
7	63.6	32.4	52.3	39.8	37.2	56.3	46.9	48.7	+ 0.2	6
8	76.2	44.3	53.1	36.9	38.2	73.7	53.7	50.4	+ 1.9	7
9	36.3	18.4	58.6	64.3	54.6	51.8	47.3	51.5	+ 3.0	8
10	70.0	*37.1	58.3	72.7	47.2	62.9	58.0	50.7	+ 2.2	9
11	47.1	39.0	36.5	35.8	41.6	37.1	39.5	45.4	− 3.1	10
12	59.6	41.5	*50.3	43.2	51.4	21.5	44.6	42.1	− 6.4	11
13	*26.6	44.8	65.4	32.3	*24.4	45.0	39.7	—	—	—

The "mean cycle" in the above table has been formed (in the way already mentioned) with the view of reducing the effects of what are called "accidental" irregularities in the rainfall, and its mean value is 48.5 inches, while the mean of the thirteen "means" is 47.3 inches. Now it will be observed that, as was the case when only the years 1813 to 1867 were taken (NATURE, vol. xvii. p. 449), there is apparently a double oscillation of the Madras rainfall during the sun-spot cycle. It will be seen also—and it is important to bear this point in mind—that the mean rainfall of the seven maximum years not only does *not* exceed, but barely reaches, the mean for the whole cycle. There are apparently two maxima and two minima, and one of the minima seems to occur very soon after the sun-spot maximum. In fact, there was a great deficiency of rainfall at Madras in the maximum year 1860, and in the years 1830, 1861, and 1869, immediately following or preceding a maximum year, but whether there were famines in one or more of these years, I do not know. (The years of minimum sun-spots are marked with an asterisk.)

Coming now to the second half of the method, so far as Madras is concerned, we get the following results :—

TABLE IV.—*Rainfall of Madras* (*Minimum Years in Eighth Line*).

Year.	1816–1828.	1826–1838.	1836–1848.	1849–1861.	1860–1872.	Means	Mean Cycle.	Variation.	Year of Cycle.
	in.	in.	in.	in.	in.	in.	in.	in.	
1	*41.2	60.7	44.7	39.8	*27.6	42.8	—	—	—
2	63.6	88.4	*49.3	36.9	37.2	55.1	51.7	+ 2.7	1
3	76.2	37.9	52.3	64.3	38.2	53.8	52.3	+ 3.3	2
4	36.3	*36.9	53.1	72.7	54.6	50.7	51.0	+ 2.0	3
5	70.0	32.4	58.6	35.8	47.2	48.8	48.8	− 0.2	4
6	47.1	44.3	58.3	43.2	41.6	46.9	45.5	− 3.5	5
7	59.6	18.4	36.5	32.3	51.4	39.6	40.8	− 8.2	6
8	**26.6**	**37.1**	**50.3**	**47.0**	**24.4**	**37.1**	**40.0**	**− 9.0**	**7**
9	33.7	39.0	65.4	52.9	41.4	46.5	43.3	− 5.7	8
10	56.0	41.5	38.0	48.5	32.3	43.4	49.0	0.0	9
11	60.7	44.8	79.8	55.1	*74.1	62.9	57.4	+ 8.4	10
12	88.4	*49.3	81.0	*27.6	56.3	60.5	58.8	+ 9.8	11
13	37.9	52.3	*54.8	37.2	73.7	51.2	—	—	—

The above table shows that the minimum rainfall, both for the "means" and the "mean cycle," coincides with the minimum of sun-spots (see Table II.), and that, upon the whole, the spots and the rain decrease and increase together. But as the maximum years (with an asterisk) are not all in the same line, nor the minimum years in Table III., all in the same line, it is necessary to confine our attention to the results for about two years on either side of the epochal years in Tables III. and IV., and in doing so we find evidence of a double oscillation.

To ascertain whether there is some probability of such an oscillation in the rainfall of Madras, we must have recourse to the more efficient method of the harmonic analysis. I have not had leisure to do so in this particular case, but Mr. J. Allan Broun (NATURE, vol. xvi. p. 334), in a thorough examination of the rainfalls of Madras and Trevandrum for the years 1838-76, gives for the mean oscillations of the Madras rainfall during that period the following equation, where y is the mean yearly rainfall in inches :—

$$y = 5.4 \sin (\theta + 50°) + 4.6 \sin (2\theta + 252°) ;$$

and he remarks that these angles give the epochs of *minimum* rainfall both in the years of minimum and of maximum sun-spots, and that the single oscillation (of about five years) has held good in seven successive periods. Now this is nearly what we should expect from Tables III. and IV.

Leaving the Madras rainfall for the present, let us come to that of Edinburgh for the years 1824 to 1872. The following table gives a comparison of the Edinburgh rainfall with the sun-spots from 1824 to 1867 :—

TABLE V.—*Rainfall of Edinburgh* (*Maximum Years in Sixth Line*).

Year.	1824–1836.	1832–1844.	1843–1855.	1855–1867.	Means	Mean Cycle.	Rain Var.	Spot Var.	Year of Cycle.
	in.	in.	in.	in.	in.	in.	in.		
1	24.8	23.2	*23.8	20.3	23.0	—	—	—	—
2	22.1	*20.9	20.9	*28.5	23.1	22.8	− 2.8	− 37.2	1
3	15.3	21.0	26.6	24.9	22.0	23.8	− 1.8	− 22.8	2
4	32.6	25.2	31.5	24.3	28.4	26.3	+ 0.7	+ 4.4	3
5	25.2	33.0	22.8	25.9	26.7	28.0	+ 2.4	+ 33.0	4
6	**30.0**	**26.8**	**30.6**	**33.4**	**30.2**	**28.9**	**+ 3.3**	**+ 43.8**	**5**
7	33.2	31.0	22.2	28.6	28.8	28.4	+ 2.8	+ 32.9	6
8	24.5	23.4	21.3	33.9	25.8	26.1	+ 0.5	+ 14.3	7
9	23.2	25.5	22.8	25.6	24.3	25.2	− 0.4	− 2.9	8
10	*20.9	26.2	31.5	28.1	26.7	24.6	− 1.0	− 16.6	9
11	21.0	16.9	[1]21.8	23.6	20.8	23.1	− 2.5	− 24.7	10
12	25.2	*23.8	20.9	27.2	24.3	23.9	− 1.7	− 24.0	11
13	33.0	20.9	20.3	*31.0	26.3	—	—	—	—

An inspection of the above table will show that there is a remarkable coincidence between the rainfall and sun-spot variations—much more remarkable than at Madras. The years of maximum and minimum rainfall and sun-spot for the mean cycles coincide, and, on the whole, there is a regular gradation from minimum to maximum and from maximum to the next minimum.

The next table (the second half of the process for Edinburgh) gives almost equally remarkable results.

TABLE VI.—*Rainfall of Edinburgh* (*Minimum Years in Eighth Line*).

Year.	1826–1838.	1836–1848.	1849–1861.	1860–1872.	Means	Mean Cycle.	Rain Var.	Spot Var.	Year of Cycle.
	in.	in.	in.	in.	in.	in.	in.		
1	15.3	33.0	22.2	*33.4	26.0	—	—	—	—
2	32.6	*26.8	21.3	28.6	27.3	27.2	+ 1.2	+ 24.7	1
3	25.2	31.0	22.8	33.9	28.2	27.8	+ 1.8	+ 15.9	2
4	*30.0	23.4	31.5	25.6	27.6	27.6	+ 1.6	+ 5.6	3
5	33.2	25.5	21.8	28.1	27.2	26.4	+ 0.4	− 5.4	4
6	24.5	26.2	20.9	23.6	23.8	24.1	− 1.9	− 20.4	5
7	23.2	16.9	20.3	27.2	21.9	23.4	− 2.6	− 36.3	6
8	**20.9**	**23.8**	**28.5**	**31.0**	**26.0**	**24.4**	**− 1.6**	**− 42.1**	**7**
9	21.0	20.9	24.9	28.6	23.8	24.6	− 1.4	− 28.6	8
10	25.2	26.6	24.3	22.2	24.6	25.2	− 0.8	+ 2.9	9
11	33.0	31.5	25.9	*22.1	28.1	26.8	+ 0.8	+ 35.3	10
12	*26.1	22.8	*33.4	23.2	26.4	28.2	+ 2.2	+ 48.9	11
13	31.0	*30.6	28.6	38.2	32.1	—	—	—	—

We find from the preceding table that the year of minimum rainfall was, on an average, the year immediately before the year of minimum sun-spot, and that the year of maximum sun-spot coincided with the year of maximum rainfall. Another coincidence is that the ratio of the rainfall to the sun-spots in the eleventh year of the "mean cycle" is nearly the same as the corresponding ratio in the first year of the cycle. Whether these relations are constant is another question ; in a case of

[1] Interpolated.

this kind we can scarcely venture to go beyond actual experience.

It would be easy to multiply similar examples and results of the method which I ventured to submit in my former paper to NATURE—a method the main object of which was to refer, as nearly as possible (without using the more laborious method of the harmonic analysis), the rainfalls of remote localities to the epochs of maximum and minimum sun-spots. I will, however, for the present only give, further, the results that have been obtained for the Paris rainfall from 1824 to 1867, and from 1816 to 1870.

TABLE VII.—*Rainfall of Paris* (*Maximum Years in Sixth Line*).

Year.	1824–1836.	1832–1844.	1843–1855.	1855–1867.	Means	Mean Cycle.	Rain Var.	Spot Var.	Year of Cycle.
	m.	m.	m.	m.	m	m.	m.		
1	572	456	*542	344	478	—	—	—	—
2	469	*503	571	*565	527	502	− 11	− 37·2	1
3	410	421	581	492	476	493	− 20	− 22·8	2
4	501	438	564	466	492	501	− 12	+ 4·4	3
5	585	611	430	545	543	541	+ 28	+ 33·0	4
6	**560**	**547**	**575**	**655**	**584**	**563**	**+ 50**	**+ 43·8**	**5**
7	573	542	597	458	543	554	+ 41	+ 32·9	6
8	529	580	563	516	547	522	+ 9	+ 14·3	7
9	456	455	469	426	452	487	− 26	− 2·9	8
10	*503	527	597	366	498	472	− 41	− 16·6	9
11	421	342	454	542	440	484	− 29	− 24·7	10
12	438	*542	614	644	559	520	+ 7	− 24·0	11
13	611	571	344	*565	523	—	—	—	—

The above table shows that, whether we take the "means" or the "mean cycle," the rainfall was greatest in the years of maximum sun-spot; that it was least in the ninth year of the "mean cycle;" and that, on the whole, the rainfall and sun-spots, notwithstanding some discrepancies, increased and decreased together.

The next table, in which the arrangement is inverted, gives similar results for Paris.

TABLE VIII.—*Rainfall of Paris* (*Minimum Years in Eighth Line*).

Year.	1816–1828.	1826–1838.	1836–1848.	1849–1861.	1860–1872.	Means	Mean Cycle.	Rain Var.	Spot Var.	Year of Cycle.
	m.	m.	m.	m.	m.	m.	m.	m.		
1	*546	410	611	597	*655	564	—	—	—	—
2	565	501	*547	563	458	527	531	+ 20	+23·3	1
3	432	585	542	469	516	509	525	+ 14	+14·5	2
4	615	*560	580	597	426	556	516	+ 5	+ 4·8	3
5	378	573	455	454	366	445	501	− 10	− 5·6	4
6	584	529	527	614	542	559	501	− 10	−19·0	5
7	424	456	342	344	644	442	492	− 19	−32·5	6
8	**457**	**503**	**542**	**565**	**565**	**526**	**502**	**− 9**	**−37·1**	**7**
9	572	421	571	492	512	514	510	− 1	−25·4	8
10	469	438	581	466	477	486	499	− 2	+ 1·8	9
11	410	611	564	545	*418	510	510	− 1	+30·9	10
12	501	*547	430	*655	—	533	529	+ 17	+44·8	11
13	585	542	*575	458	—	540	—	—	—	—

We see that the minimum rainfall occurred, on an average, in the year immediately preceding the year of minimum sun-spots, as at Edinburgh, but that the variation was not so regular.

As formerly remarked, the rainfalls of Edinburgh and Paris—especially that of Edinburgh—are more favourable to the theory than the rainfall of Madras.

Mr. Buchan considers the method which has now been sketched a new one, and, "as such, deserving of a careful examination as to how far it is applicable to the data submitted for discussion." This examination consists almost wholly in showing that by placing the maximum years in the same line or group the minimum years are spread over six out of thirteen groups, and that by placing the minimum years in the same group the maximum are also spread over six groups. Hence he concludes that this double arrangement is inferior to a single one in which the maximum and minimum years together are "compactly" spread over six out of eleven groups. But it seems to me that he has in great measure lost sight of what should be a main object of comparisons of sun-spots and rainfall, namely, the closest possible reference of the rainfall to the *epochs* of maximum and minimum sun-spots, and that however compact the arrangement he recommends may be considered, it is fundamentally objectionable. By placing the maximum and minimum years respectively in the *same* groups there is certainly a much greater chance of finding any connection that may exist between the two phenomena than by spreading them over six groups out of eleven.

How far the method defended by Mr. Buchan is applicable to the data will appear from the following table of the Madras rainfall, in which Dr. Hunter's arrangement is adopted. The maxima and minima years are marked with an asterisk.

TABLE IX.—*Rainfall of Madras* (*Maximum and Minimum Years in Six Groups*).

Year.	1811–1821.	1822–1832.	1833–1843.	1844–1854.	1855–1865.	1866–1876.	Means	Rain Var.	Spot Var.
	in.	in.	in.	in.	in.	in.	in.		
1	—	59·6	*37·1	65·4	32·3	51·4	49·2	+ 0·9	− 37·6
2	—	*26·6	39·0	38·1	*47·0	*24·4	35·0	− 13·3	− 35·0
3	45·1	33·7	41·5	79·8	52·9	41·4	49·1	+ 0·8	− 14·2
4	32·4	56·0	44·8	81·0	48·5	32·3	49·2	+ 0·9	+ 16·6
5	56·0	60·7	49·3	*54·8	55·1	*74·1	58·3	+ 10·0	+ 45·0
6	*41·2	88·4	52·3	39·8	*27·6	56·3	50·9	+ 2·6	+ 37·0
7	63·6	37·9	53·1	36·9	37·2	73·7	50·4	+ 2·1	+ 24·5
8	76·2	*36·9	58·6	64·3	38·2	51·8	54·3	+ 6·0	+ 11·0
9	36·3	32·4	58·3	72·7	54·6	62 9	52·9	+ 4·6	− 2·4
10	70·0	44·3	36·5	35·8	47·2	37·1	45·1	− 3·2	− 15·4
11	47·1	18·4	*50·3	43·2	41·6	21·5	37·0	− 11·3	− 29·2

The mean rainfall for the cycle is 48·3 inches. Now the mean rainfall for the fifth group is 58·3 inches, and the mean value of the sun-spots for the same group 45·0, which is the *maximum*. It is thus made to appear that, on an average, the maximum rainfall of Madras coincides with the maximum of sun-spots. But this is contrary to fact. We know, as a matter of observation (see Table III.), that the mean rainfall of Madras in the maximum years was not above the average, and yet the arrangement recommended by Mr. Buchan makes it *ten* inches *above* the average.

Applying the same arrangement to the rainfall of Edinburgh, we get the following results.

TABLE X.—*Rainfall of Edinburgh* (*Maximum and Minimum Years in Six Groups*).

Year.	1822–1832.	1833–1843.	1844–1854.	1855–1865.	Mean Rain.	Mean Spots.	Rain Var.	Spot Var.
	in.	in.	in.	in.	in.		in.	
1	26·1	*20·9	20·9	20·3	22·1	8·1	− 3·6	− 43·2
2	*30·3	21·0	26·6	*28·5	26·6	14·8	+ 0·9	− 36·5
3	24·8	25·2	31·5	24·9	26·6	37·4	+ 0·9	− 13·9
4	22·1	33·0	22·8	24·3	25·6	72·3	− 0·1	+ 21·0
5	15·3	*26·1	*30·6	25·9	24·5	92·4	− 1·2	+ 41·1
6	32·6	31·0	22·2	*33·4	29·8	86·4	+ 4·1	+ 35·1
7	25·2	23·4	21·3	28·6	24·6	73·2	− 1·1	+ 21·9
8	*30·0	25·5	22·8	33·9	28·0	63·3	+ 2·3	+ 12·0
9	33·2	26·2	31·5	25·6	29·1	50·9	+ 3·4	− 0·4
10	24·5	16·9	21·8	28·1	22·8	41·5	− 2·9	− 9·8
11	23·2	*23·8	20·9	23·6	22·9	23·9	− 2·8	− 27·4

In the above table the sun-spot maximum occurs in the 5th group, and the rainfall is made to be 1·2 inch below the mean; that is, according to this arrangement the people of Edinburgh are supposed to get *less* than their average allowance of rain in the maximum year. But according to the observations published by the Scottish Meteorological Society, that again is contrary to fact, for (see Table V.) the rainfall of Edinburgh is not 1·2 inch *below* the mean for the maximum years, but, taking the thirteen "means" five inches *above* it, and, taking the "mean cycle" 3 inches above it.

Which of the two methods, then, is the more applicable to the data for discussion?

Of all the methods, that of the harmonic analysis is doubtless the best. It enables us to see whether there is any parallelism, and if there is a cycle, what is its probable length with respect to the sun-spot cycle, the range of variation, the times of maximum and minimum, with their intervals, &c. I have applied this method to yearly values of the rainfalls, and of the levels of rivers of various countries, and have come to the conclusion that, notwithstanding all apparent irregularities, there is an intimate connection between sun-spots and rainfall.

If the rainfall generally was above its mean in the years of maximum sun-spot, and below it in the years of minimum sun-spot, we should get for the mean yearly rainfall of a number of stations the equation $\frac{S-s}{s'-S} = \frac{R-r}{r'-R}$, where S is the mean value of the sun-spots for the period examined, s the mean value of the spots when below S, s' their mean value when above S, and R, r, r' the corresponding values for the rain for the years from which S, s, and s' were obtained. This formula applied to the public observations of different countries shows that with very few exceptions the rainfall for the periods examined were above the average. The results for the mean rainfall of fifty-four stations in Great Britain, and thirty-four in America from 1824 to 1867 are as follows:—

$$\text{Great Britain} \ldots \ldots \begin{cases} \dfrac{S-s}{s'-S} = \dfrac{24{\cdot}9}{29{\cdot}8} = {\cdot}8356 \\ \dfrac{R-r}{r'-R} = \dfrac{+0{\cdot}75}{+0{\cdot}90} = {\cdot}8333 \end{cases}$$

$$\text{America} \ldots \ldots \ldots \begin{cases} \dfrac{S-s}{s'-S} = \dfrac{24{\cdot}9}{29{\cdot}8} = {\cdot}8356 \\ \dfrac{R-r}{r'-R} = \dfrac{+0{\cdot}94}{+1{\cdot}13} = {\cdot}8407 \end{cases}$$

In other words, the rainfall of fifty-four stations in Britain from 1824 to 1867 was 0·75 inch below the mean when the sun-spots were below their mean and 0·90 inch above it when the spots were in excess, and the corresponding values for America were 0·94 and 1·13 inch.

C. MELDRUM

Sun-spots and Weather

In NATURE, vol. xvii. p. 326, Dr. Balfour Stewart concludes an article with the following remark:—

"It is nearly, if not absolutely, impossible from observations already made, to tell whether the sun be hotter or colder as a whole when there are most spots on his surface. The sooner we get to know this the better for our problem."

The Bombay barometric observations appear to me to afford fairly conclusive evidence in favour of the sun being hottest about the time of maximum spotted area, and coldest when the spotted area is at its minimum.

It is well known that in Central Asia the annual variation of the barometric pressure is greater than in any other portion of the globe, and it is universally admitted that this variation is due to the great variation of temperature between summer and winter, the barometer being low when the temperature is high, and *vice versâ*. If, therefore, the absolute heat of the sun is subject to considerable variations, we ought to find the barometric pressure in Central Asia responding to those variations just as it does to the annual variations of temperature; in other words, the summer barometric minimum should be lowest in those years when the sun is hottest, and the winter maximum should be highest in those years when the sun is coldest.

Similar results should be obtainable from the barometric records of any station where the annual variation of pressure is considerable and of the same character as in Central Asia. Bombay is such a station, and one where cyclonic disturbances are less frequent and violent than at most other Indian coast stations. I give below the mean barometric pressure at Bombay for the summer and winter half-years from 1847 to 1877:—

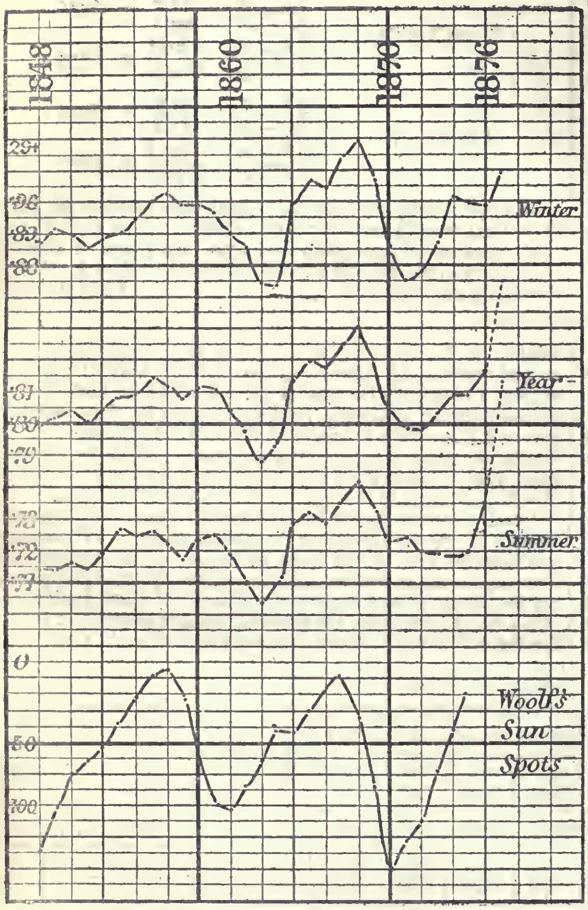
Mean Barometric Pressure at Bombay.

October to March.	1847–48.	1848–49.	1849–50.	1850–51.	1851–52.	1852–53.	1853–54.	1854–55.	1855–56.	1856–57.	1857–58.	1858–59.	1859–60.	1860–61.	1861–62.	1862–63.
	29+ ·884	29+ ·888	29+ ·894	29+ ·886	29+ ·888	29+ ·891	29+ ·891	29+ ·897	29+ ·905	29+ ·901	29+ ·898	29+ ·901	29+ ·894	29+ ·886	29+ ·886	29+ ·862
	1863–64.	1864–65.	1865–66.	1866–67.	1867–68.	1868–69.	1869–70.	1870–71.	1871–72.	1872–73.	1873–74.	1874–75.	1875–76.	1876–77.	1877–78.	—
	29+ ·885	29+ ·912	29+ ·902	29+ ·906	29+ ·925	29+ ·913	29+ ·903	29+ ·872	29+ ·879	29+ ·878	29+ ·897	29+ ·906	29+ ·893	29+ ·903	29+ ·916	— —
April to September.	1847.	1848.	1849.	1850.	1851.	1852.	1853.	1854.	1855.	1856.	1857.	1858.	1859.	1860.	1861.	1862.
	29+ ·707	29+ ·722	29+ ·703	29+ ·730	29+ ·704	29+ ·719	29+ ·737	29+ ·712	29+ ·743	29+ ·712	29+ ·718	29+ ·723	29+ ·729	29+ ·722	29+ ·707	29+ ·705
	1863.	1864.	1865.	1866.	1867.	1863.	1869.	1870.	1871.	1872.	1873.	1874.	1875.	1876.	1877.	—
	29+ ·698	29+ ·751	29+ ·720	29+ ·738	29+ ·722	29+ ·760	29+ ·726	29+ ·721	29+ ·731	29+ ·713	29+ ·725	29+ ·713	29+ ·722	29+ ·723	29+ ·773	— —

The winter means correspond in time to the beginning of each year, the summer means to the middle of each year. Taking the mean of each pair of winter means, we obtain a new set of numbers which correspond to the middle of each year, and which give a somewhat smoother curve than the original numbers, and performing a similar operation twice upon the summer means, we obtain a similarly smoothed set of numbers also corresponding to the middle of each year. These two sets of smoothed numbers, and their means, are given below, and graphically represented by the accompanying curves, along with the inverted sun-spot curve.

—	1848.	1849.	1850.	1851.	1852.	1853.	1854.	1855.	1856.	1857.	1858.	1859.	1860.	1861.	1862.
	29+	29+	29+	29+	29+	29+	29+	29+	29+	29+	29+	29+	29+	29+	29+
Winter	·886	·891	·890	·887	·889	·891	·894	·901	·903	·899	·899	·897	·890	·886	·874
Summer	·713	·714	·716	·714	·719	·726	·725	·727	·721	·717	·723	·725	·719	·710	·703
Year	·799	·802	·803	·800	·804	·808	·809	·814	·812	·808	·811	·811	·804	·798	·788
—	1863.	1864.	1865.	1866.	1867.	1868.	1869.	1870.	1871.	1872.	1873.	1874.	1875.	1876.	1877.
	29+	29+	29+	29+	29+	29+	29+	29+	29+	29+	29+	29+	29+	29+	29+
Winter	·873	·898	·907	·904	·915	·919	·908	·887	·875	·878	·887	·901	·899	·898	·909
Summer	·712	·729	·732	·729	·735	·742	·733	·724	·724	·720	·719	·718	·719	·735	[·773]
Year	·792	·813	·819	·816	·825	·830	·820	·805	·799	·799	·803	·809	·809	·816	[·845]

It will be seen that there is a remarkable degree of resemblance in the progression of these phenomena from year to year, but that the barometric curve "lags behind" the sun-spot curve, particularly in the years of maximum sun-spots.



The winter curve is more regular than the summer one, probably because the weather generally in India is more settled in the winter than in the summer, but on the whole the two curves support each other in showing a low pressure about the time of sun-spot maximum, and a high pressure at the time of sun-spot minimum. We may therefore conclude that the sun is hottest about the time when the spots are at a maximum, and coldest when they are at a minimum.

The range of the variation of the year by mean pressure from the minimum of 1862 to the maximum of 1868, is ·042 of an inch, and the mean range of the barometer from January to July is ·291, from which it appears that the variations of pressure produced by absolute variations of the sun's heat are, in comparison with the usual seasonal changes, by no means insignificant.

These results appear to harmonise well with the decennial variations of the rainfall in India, and to throw light upon the inverse variation (compared with the sun-spots) of the winter rainfall of Northern India. Mr. Archibald has attempted to explain this latter phenomenon on the assumption that the sun is coldest when it is most spotted, but the inverse winter variation of the rainfall of Northern India, as well as the direct variation at Madras, Bombay, Trevandrum, and elsewhere, appear to me to follow more naturally from the contrary view; for if the winter rainfall in Northern India is really due to the cold of winter we should expect it to be greatest when the sun is coldest, just as the summer rainfall is expected to be greatest when the sun is hottest. FRED. CHAMBERS

Bombay, August 23

The Norwegian Arctic Expedition

THE *Vöringen*, Capt. Wille, returned to Christiania on the 10th, from Spitzbergen, after a most successful cruise. No less than 375 stations have been thus explored by sounding, dredging, and trawling during the last three summers; and the *Morgenbladet* reminds us that only 354 of such stations were recorded in the notice of the *Challenger* expedition. The number of stations in the *Porcupine* cruises of 1869 and 1870 was 148.

Prof. G. O. Sars informs me that in every department of zoology a vast amount of material was procured in his last cruise, and that especially the Mollusca are abundantly represented, not only by magnificent specimens of rare Arctic species, such as *Fusus kroyeri* and *F.* (*Neptunea*) *deformis*, but also by several interesting new forms. He adds that the conchological collection from this cruise is indeed much richer than both of those made in his former two cruises. Herr Friele will work out all the Mollusca.

I cannot omit expressing my admiration of the recent work of Prof. G. O. Sars, entitled "Mollusca Regionis Arcticæ Norvegiæ." His descriptions are excellent, and his figures (all by his own hand, and autographed) are inimitable. The work contains 466 pages and 52 plates, besides a chart.

Ware Priory, Herts, September 23 J. GWYN JEFFREYS

Albinism in Birds

WHEN I was forming the Government Central Museum at Madras, an albino crow was brought to me, which was stuffed and placed in the museum. It was mentioned to me at the same time that there is a colony of albino crows at a part of the Malabar coast, but I have never been able to verify the statement. That district is daily becoming more frequented by Europeans, and some of them may soon be in a position to ascertain how far the report was correct. EDWARD BALFOUR

2, Oxford Square, September 19

As Mr. Page says, in NATURE (vol. xviii. p. 540), he has only heard of one white swallow, it may be interesting to him and your readers to learn that in the Newcastle Museum there is a specimen, also white specimens of the rook, pheasant, curlew, sparrow, and starling; in the same collection will also be found a "pale rose" coloured specimen of the bullfinch.

Newcastle-upon-Tyne, September 20 WM. LYALL

"Hearing of Insects"

I AM able to confirm the accounts given by Mr. Simson in your last number as to the probability of the hearing of insects. When travelling on the River Magdalena, New Granada, in 1861, the mode of which is by a long boat, arched over with bamboo, on which the sailors (bogas) passing from one extremity to the other, propel it with long poles, hugging the river bank, accompanied with wild cries and execrations, I observed on several occasions that these cries suddenly ceased, a dead silence following, and on inquiring the cause they pointed to nests high up in the trees, whispering the word vispa (wasp). As the bogas pursue their avocations in a state of semi-nudity, they have the greatest dread of these insects, fearing to speak aloud, as their only alternative if attacked by them is to plunge into the stream, where alligators abound. The wasp is long, slender, and black in colour.

117, Cromwell Road, S.W., Sept. 21 W. L. DUDLEY

The Meteor Shower of Andromedes I.

MR. GREG's meteor radiant (No. 103 of his 1876 catalogue) at R.A. 7°, Dec. 33° N., for July 21 to August, really consists of two well-defined showers near α Andromedæ. The meteors are quite distinct, and, moreover, there is a difference of 10° in declination. My observations since 1873 indicate two positions as below:—

I. July 6 to August 16, 6° + 37°	60 very swift streak-leaving meteors.
II. July 6 to August 11, 3° + 27°	23 slow, bright, trained meteors.

Schiaparelli and Zezioli, Greg and Herschel, and Tupman, found the former some years ago, and Denza gives the latter at 2° + 29° August 8–13. Mr. Greg averages these with several others (including one by Schmidt at 7° + 30°, and another by Heis at 11° + 30°, both for August) and finds a centre at 7° + 33° for the whole. There is no doubt, however, that there are *two* conspicuous contemporary radiants of entirely

different meteors. This year, on August 10, in bright moonlight, I traced five meteors from $6° + 37°$, and the epoch and place fall near Comet II. 1780, August 14, $3\frac{1}{2}° + 38\frac{1}{2}°$, but the comet was only visible for three days after its discovery by Montaigne and others on November 28, 1780, and hence the orbit is not likely to have been exactly determined. At the nodal passage the comet's orbit lies far within the orbit of the earth, so that an encounter of the earth with the comet-particles is only possible on the thesis of Weiss and Schiaparelli that "some part of the cometary materials repelled from its proper orbit by the sun in the form of the tail or other luminous appendage emitted by the comet near its perihelion passage extends to such a distance in its orbital plane as to intersect the orbit of the earth" (see B.A. *Report*, 1873, pp. 401–2).

Ashleydown, Bristol, September 16 W. F. DENNING

The Zoological Record

IN the third number of vol. iii. of the *Niederländisches Archiv für Zoologie* (Leiden: E. T. Brill), I published in German a "Catalogue Raisonné" of zoological works and papers that appeared in the Netherlands during 1875 and 1876. You noticed the appearance of this paper in your "Notes" (NATURE, vol. xvi. p. 112).

The principal reason of my publishing this bibliography was my wish to make known in other countries what is done in the Netherlands in the zoological department. For the same purpose, about the end of May, 1877, I sent a copy of my paper to the *Zoological Record* and addressed it "Solely to the Editor of the *Zoological Record*, care of Mr. Van Voorst, 1, Paternoster Row, London."

Afterwards studying vol. xii. and xiii. of the said "Record," I found that about twenty of the papers recorded in my catalogue were not mentioned in these volumes. Of course this might have been occasioned by the unimportance of these twenty unlucky papers; but conscientiously comparing their value with that of the other sixty of my bibliography, and as far as possible in general with the papers mentioned in the *Record* I got the conviction that this could not be the reason.

I feel a great deal of admiration, and at the same time of gratitude for the immense amount of work done by the contributors of the *Record*, and I quite agree with you (NATURE, vol. xviii. p. 485) that it would be to the everlasting disgrace of zoologists (not only of your tongue, but of all tongues) if its existence should be prematurely brought to a close. But only when I find in the *Record* as much completeness as possible, the use of it will spare me the endless trouble of looking for every detail over the totality of zoological literature.

Now I don't believe that in the case mentioned here (to secure this completeness) much care has been taken.

September 19 P. P. C. HOEK

Earth Pillars

SHOULD you deem the following of sufficient interest, will you kindly insert it in NATURE?

A few days since I saw an interesting example of minute earth-pillars on the shore of the Hecht Sea, above Kiefersfelden, Inn Thal. In a cove to the north the beach for many yards formed a perfect forest of little pillars, whose height ranged from a quarter to three-quarters of an inch. On the top of most lay a small stone, a fragment of wood or shell; but some, which had lost their coverings, were wearing away. The shell fragments (from a Unio, I fancy) seemed to form the most complete protection, and these often fitted the pillars like helmets; in fact, it required no great stretch of the imagination to fancy the whole a marching army, and the jutting wood fragments spears.

South Tyrol is by no means the only place in this country where earth-pillars occur, though the Bozen pillars are probably the finest. Amongst others in North Tyrol there is a very interesting example of large earth-pillars on the Brenner railway, between Innsbruck and Patsch, on the right—going south. JAMES H. MIDGLEY

Brixlegg am Inn, Tyrol, September 17, 1878

Indian Building Timber

IN NATURE, vol. xviii. p. 317, it is stated "much or most of the wood used in Peking in building houses, temples, and palaces is said to come from Corea;" it is further remarked editorially, "we think, however, our contemporary is in error in stating, without qualification, that 'the great wooden masts which support the noble temples and gatehouses of the Imperial City of Peking (all enormous, beautiful, and enduring spars) come from Corea.'"

Having had some experience in the timber and timber-trees of Burma I am inclined to the opinion that this valuable timber "nan-mu" therein referred to will prove to be identical with the wood used for the same purposes generally over Burma. The wood is called in Lower Burma "Pyenkadoo," it has a wide distribution under a variety of names, according to the different provincial dialects of the districts it is found in. Its great length of bole without branches, the different sizes at which it can be obtained renders it from its great durability, readiness to polish, and its variegated and coloured grain (brown mahogany colour) most suitable for the supports or pillars of "kyoings," or temples. It belongs to the natural order Leguminoseæ, specific name *Ingá xylocarpa*.

Besides this there are several other woods highly esteemed by the Burmese for durability, and these chiefly are found amongst the Cassias and Dalbergias.

Inga xylocarpa has great toughness—a piece of three feet long by one inch square I find stood a breaking weight of 1,153 pounds; its specific gravity is nearly double that of teak and it does not float.

The objection to the introduction of the different ornamental and useful timbers of Burma is their toughness, hardness to work, and hence increased labour and wear of tools.

Whitby R. BENSON

[With reference to the question of the identity of the wood of the "nan-muh" tree with that of *Inga xylocarpa* we may point out that from material received at Kew the former has been referred to a Lauraceous tree, probably *Phœbe pallida*. From comparison of the two woods microscopically they present something in common, the annual rings, however, are much more apparent in the "nan-muh" than in the "Pyenkadoo." This latter is of a dark reddish brown, extremely heavy, as described by Col. Benson, while the nan-muh is of a dull umber colour and much lighter in weight.—ED.]

OUR ASTRONOMICAL COLUMN

THE INTRA-MERCURIAL PLANET.—The particulars of Mr. Lewis Swift's observations during the totality of the recent eclipse, given in his letter which appeared in NATURE last week, are satisfactory so far as they afford independent testimony to the existence of an unknown body in the vicinity of the star θ Cancri, or in the locality where Prof. Watson, a few minutes previously, had observed an object which he considers to have been an intra-Mercurial planet. In other respects Mr. Swift's letter is indefinite and contradictory in itself. He tells us that he observed two red stars "with large, round, and equally bright discs," estimating the distance between them at about 7′ or 8′; and, one of the objects being identified with θ Cancri, he intimates that the proximity of the other to this star enabled him to estimate its position with great exactness, especially in declination. But in a subsequent paragraph, where the place of the star is adopted from the Astronomer-Royal, the unknown object is fixed to a position which makes its distance from θ Cancri 30′, or four times as great as mentioned previously. The place of the supposed planet, according to Prof. Watson, was, as stated last week, in right ascension 8h. 27m. 24s., and declination 18° 16′ N.; and as the apparent place of the star at the time was in right ascension 8h. 24m. 39·9s., and declination 18° 30′ 19″, the distance between the two was 42′, on an angle at the star, of 110°. With regard to Mr. Swift's concluding observation as to the position of the presumed planet in its orbit, it is evident that, to present a round or nearly round disc, it must have been situate, as Prof. Watson infers, in the superior part of the orbit, and being to the west of the sun, would be approaching superior conjunction.

Prof. Watson states that the magnitude of the object in question was 4 to $4\frac{1}{2}$, and that of the second unknown star, which he alone appears to have observed, was $3\frac{1}{2}$, and adds, "they were probably really brighter, because

the illumination of the sky was not considered in the estimates." Before he entered upon the reduction of his observations, he had thought the second object might be ζ Cancri (though surprised to find it so bright) because he did not see that star—a well-known double star, components $5\frac{1}{2}$ and $6\frac{1}{2}$,—a gust of wind which occurred just before the end of totality having possibly disturbed the telescope. Yet, the circumstance of his not having remarked ζ Cancri would be accounted for by his statement that he did not sweep further than the second object, which his reading places in right ascension 8h. 9m. 24s., declination 18° 3′, and which he believes to be correct—the sun coming out immediately afterwards, and of course putting an instantaneous termination to his observations.

VARIABLE STARS.—The following are the Greenwich times of geocentric minima of Algol and S Cancri in the last quarter of the present year, which fall between dusk and 13h. M.T.

ALGOL.

		h. m.				h. m.				h. m.
Oct.	7	12 27	...	Nov.	2	7 46	...	Dec.	12	11 11
,,	10	9 16	...	,,	19	12 39	...	,,	15	8 0
,,	13	6 5	...	,,	22	9 28	...	,,	18	4 49
,,	30	10 57	...	,,	15	6 17	...	–		

S CANCRI.

		h. m.				h. m.				h. m.
Oct.	2	11 46	...	Nov.	9	10 12	...	Dec.	17	8 38
,,	21	10 59	...	,,	28	9 35				

The maximum of Mira Ceti occurs on October 11, and the minimum of χ Cygni on October 26, according to Prof. Schönfeld.

THE METEOR OF DECEMBER 24, 1873.—Mr. Cleveland Abbe, Director of the Observatory of Cincinnati, sends us a discussion of the observations of a remarkable meteor seen on Christmas Eve, 1873, to which the attention of the Washington Philosophical Society had been particularly directed soon after its occurrence, and, on the suggestion of the late secretary of the Smithsonian Institution, a committee formed with the view of collecting and discussing observations. Though about fifty accounts of the appearance of the meteor were thus brought together, they have only sufficed to give a general idea of its track and altitude. Nearly all the observers describe it as equalling the full moon in brightness, with conical form moving base forward, but not followed as in so many cases, by any regular train; colour bright yellow, sparks or flames of red and blue proceeding from it. Mr. Abbe finds that it "entered the earth's atmosphere at some point vertically above the northern part of the State of Delaware, so that its apparent altitude, as seen at Danbury, Conn., was 30°, and at Washington, D.C., about 45°," whence he concludes its real altitude above the earth's surface to have been about ninety miles. Its after-course was downward to beyond Fairfax county, and at its nearest approach to the earth it was distant twenty miles. Its entire visible path of about 120 miles was described in from three to five seconds. An explosion occurred at a distance of about thirty miles from Washington, where the meteor was observed, amongst others, by Profs. Newcomb, Hilgard, and Holden.

MR. THOMAS GRUBB, F.R.S.

WE regret to announce the death of Mr. Thomas Grubb, F.R.S., and we cannot do so without referring to his marked talents and the important service he has rendered to science.

Mr. Grubb was originally intended for a mercantile career, but his natural tastes proved too strong to allow him to continue in the life he had begun. He became a mechanical engineer, and soon his workshops became known not only for excellent workmanship but still more for marked originality of conception in design. His *chef-d'œuvre* in both these respects is probably the machinery for engraving, printing, and numbering the notes of the Bank of Ireland.

Whilst Mr. Grubb's workshops were rising in reputation, he was quietly and steadily developing marked powers as a practical optician. During the construction of the great 6-feet equatorial at Parsonstown, the late Lord Rosse frequently acknowledges his obligations to Mr. Grubb for valuable suggestions. The system of levers on which Lord Rosse supported his specula, to avoid flexure, was of Mr. Grubb's invention. Of his chief scientific works we may mention :—

1. The equipment of nearly forty British magnetic stations under the direction of Provost Loyd.
2. A 15-inch reflecting telescope equatorially mounted at the Armagh Observatory.
3. The equatoreals of the Markree and Dunsink Observatories—refractors of 13 and 12 inches aperture respectively. And lastly, the great Melbourne telescope of 4 feet aperture, equatorially mounted, and driven by clockwork. After this last achievement Mr. Grubb retired from business life, succeeded by his son, Mr. Howard Grubb.

Such works as the 15-inch refractors in the observatories of Lord Lindsay and Dr. Huggins, and the great refractor of 27 inches aperture now in course of construction for Vienna show that the son is no unworthy representative of the father.

Mr. Thomas Grubb was elected a Fellow of the Royal Society of London in 1864, in recognition of his successful completion of the great Melbourne telescope, and of his other successful labours in the cause of science. He was also a member of the Royal Irish Academy. He died on the 19th instant at his residence in Dublin, and leaves to mourn his loss a large circle of friends whom his kindly genial manners and ever interesting conversation had drawn round him.

MR. THOMAS BELT, F.G.S.

THE scientific world will hear with regret the recent death of the well-known naturalist and geologist, Mr. Thomas Belt, F.G.S., which has just been telegraphed from Colorado. It is believed to have been caused by mountain fever. Elected a Fellow of the Geological Society in 1866, the geological world owes to him the division of the Lingula flags into Maentwrog, Ffestiniog, and Dolgelly flags, proposed in 1867. In 1874 appeared his well-known and deservedly popular "Naturalist in Nicaragua," in which he showed how his professional avocations as an engineer had lent keenness to his observing faculties, and how an acute reasoner can utilise his observations. The work conveyed much information on protective mimicry, plant-fertilisation, sexual selection, and the other collateral issues of the theory of evolution. It contained the first sketch of those views on glacial geology which were the most prominent subject of the author's study for the rest of his life. These views were given in considerable detail in the *Geological Magazine* for April, 1874, and were well expounded by Mr. Henry Woodward, F.R.S., in his presidential address of that year to the Geologists' Association. Mr. Belt skilfully answered his opponents in NATURE, vol. x., his controversial speaking and writing being always marked by a candour and temper which, if it did not carry conviction, could not fail to elicit admiration from perfect strangers and mere spectators. In November, 1875, he read a paper to the Geological Society "On the Drift of Devon and Cornwall" (*Quart. Jour. Geol. Soc.*, vol. xxxii.), and another "On the Steppes of Southern Russia" (*Quart. Jour. Geol. Soc.*, vol. xxxiii.), in June, 1877. He also contributed various papers to the *Quarterly Journal of Science*, amongst others one "On the Loess of the Rhine and the Danube," in January, 1877, and one "On the Glacial Period in the Southern Hemisphere," in July, 1877.

ON THE NATURE OF VIBRATORY MOTIONS[1]

On the Nature of Sound

SOUND is the sensation peculiar to the ear. This sensation is caused by rapidly-succeeding to-and-fro motions of the air, which touches the outside surface of the drum-skin of the ear. These to-and-fro motions may be given to the air by a distant body, like a string of a violin. The string moves to and fro, that is, it *vibrates.* These vibrations of the string act on the bridge of the violin, which rests on the belly or sounding-board of the instrument. The surface of the sounding-board is thus set trembling, and these tremors, or vibrations, spread through the air in all directions around the instrument, somewhat in the manner that water-waves spread around the place where a stone has been dropped into a quiet pond. These tremors of the air, however, are not sound, but the cause of sound. Sound, as we have said, is a *sensation;* but, as the cause of this sensation is always vibration, we call those vibrations which give this sensation *sonorous vibrations.* Thus, if we examine attentively the vibrating string of the violin, we shall see that it looks like a shadowy spindle, showing that the string swings quickly to and fro; but, on closing the ears, the sensation of sound disappears, and there remains to us only the sight of the quick to-and-fro motion which, the moment before, caused the sound.

Behind the drum-skin of the ear is a jointed chain of three little bones. The one, H of Fig. 1, attached to the

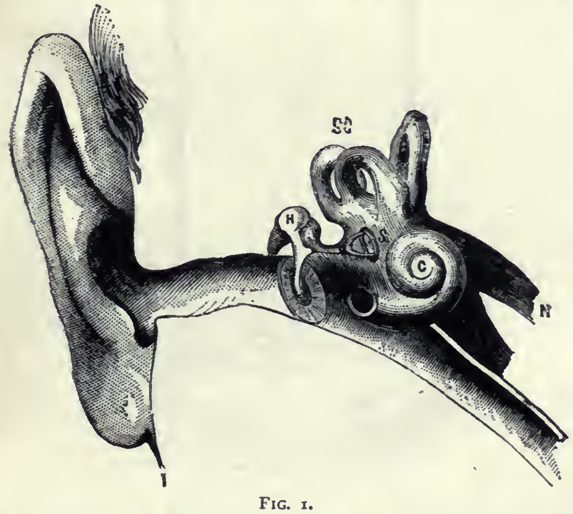


FIG. 1.

drum-skin, is called the *hammer;* the next, A, is called the *anvil;* the third, S, has the exact form of a stirrup, and is called the *stirrup-bone.* This last bone of the chain is attached to an oval membrane, which is a little larger than the foot of the stirrup. This oval membrane closes a hole opening into the cavity forming the *inner ear;* a cavity tunnelled out of the hardest bone of the head, and having a very complex form. The oval hole just spoken of opens into a globular portion of the cavity known as the vestibule, and from this lead three semicircular canals, S C, and also a cavity, C, of such a marked resemblance to a snail's shell that it is called *cochlea,* the Latin word for that object. The cavity of the inner ear is filled with a liquid, in which spread out the delicate fibres of the auditory nerve.

Let us consider how this wonderful little instrument acts when sonorous vibrations reach it. Imagine the violin-string vibrating 500 times in one second. The sounding-board also makes 500 vibrations in a second. The air touching the violin is set trembling with 500 tremors a second, and these tremors speed with a velocity of 1,100 feet in a second in all directions through the surrounding air. They soon reach the drum-skin of the ear. The latter, being elastic, moves in and out with the air which touches it. Then this membrane, in its turn, pushes and pulls the little ear-bones 500 times in a second. The last bone, the little stirrup, finally receives the vibrations sent from the violin-string, and sends them into the fluid of the inner ear, where they shake the fibres of the auditory nerve 500 times in a second. These tremors of the nerve—how we know not—so affect the brain that we have the sensation which we call sound. The description we have just given is not that of a picture created by the imagination, but is an account of what really exists, and of what can actually be seen by the aid of the proper instruments.

A body may vibrate more or less frequently in a second; it may swing over a greater or less space; and it may have several minute tremors while it makes its main swing. These differences in vibrations make sounds higher or lower in pitch, loud or soft, simple or compound. It is easy to say all this, but really to understand it, one must make experiments and discover these facts for himself.

On the Nature of Vibratory Motions

The character of a sound depends on the nature of the vibrations which cause it, therefore our first experiments will be with vibrations which are so slow that we can study the nature of these peculiar motions. These experiments will be followed by others on vibrations of the same kind, only differing in this—that they are so rapid and frequent that they cause sounds. A correct knowledge of the nature of these motions lies at the foundation of a clear understanding of the nature of sound. We hope that the student will make these experiments with care, and keenly observe them.

Experiment 1.—At the toy-shops you can buy for a few pence a wooden ball having a piece of elastic rubber fastened to it. Take out the elastic and lay it aside, as we shall need it in another experiment. Get a piece of fine brass wire, about 2 feet (61 centimetres) long, and fasten it to the ball. The weight of the ball should pull the wire straight, and, if it does not, a finer wire must be used. Hold the end of the wire in the left hand, and with the right hand draw the ball to one side. Let it go, and it will swing backward and forward like the pendulum of a clock. This kind of movement we call a *pendulous or transverse vibration.*

Experiment 2.—Cut out a narrow triangle of paper, 4 inches (10 centimetres) long, and paste it to the bottom of the ball. Twist the wire which supports the ball by turning the latter half round, and watch the paper pointer as it swings first one way and then the other. Here we have another kind of vibration, a motion caused by the twisting and untwisting of the wire. Such a motion is called a *torsional vibration.*

Experiment 3.—Take off the wire and the paper, and put the elastic on the ball. Hold the end of the elastic in one hand, and with the other pull the ball gently downward, then let it go. It vibrates up and down in the direction of the length of the elastic. Hence we call this kind of motion a *longitudinal vibration.*

These experiments show us the three kinds of vibrations, transverse, torsional, and longitudinal. They differ in direction, but all have the same manner of moving; for the different kinds of vibration, transverse, longitudinal, and torsional, go through motions with the same changes in velocity as take place in the swings of an ordinary pendulum. These vibrations all start from a position of momentary rest. The motion begins slowly, and gets faster and faster till the body gains the position it naturally has when it is at rest—at this point it has its

[1] From a forthcoming work on "Sound: a Series of Simple, Entertaining, and Inexpensive Experiments in the Phenomena of Sound, for the Use of Students of every Age." By Alfred Marshall Mayer, Professor of Physics in the Stevens Institute of Technology. Communicated by the author.

greatest velocity. Passing this point, it goes slower and slower till it again comes momentarily to rest, and then begins its backward motion, and repeats again the same changes in velocity.

It is now necessary that the student should gain clear ideas of the nature of this pendulous motion. It is the cause of sound. It exists throughout all the air in which a sound may be perceived, and, by the changes in the number, extent of swing, and combinations of these pendular motions, all the changes of pitch, of intensity, and of quality of sound are produced. Therefore the knowledge which we now desire to give the reader lies at the very foundation of a correct understanding of the subject of this book.

An experiment is the key to this knowledge. It is the experiment with

The Conical Pendulum

An ordinary pendulum changes its speed during its swings right and left exactly as a ball *appears* to change its speed when this ball revolves with a uniform speed in a circle, and we look at it along a line of sight which is in the plane of the circle.

Experiment 4.—Let one take the ball and wire to the farther end of the room, and by a slight circular motion of the end of the wire he must cause the ball to revolve in a circle. Soon the ball gets into a uniform speed around the circle, and then it forms what is called a conical pendulum; a kind of pendulum sometimes used in clocks. Now stoop down till your eye is on a level with the ball. This you will know by the ball appearing

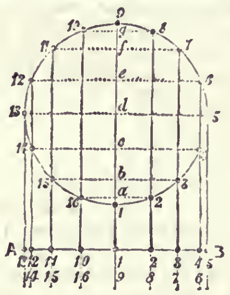


FIG. 2.

to move from side to side *in a straight line*. Study this motion carefully. It reproduces exactly the motion of an ordinary pendulum of the same length as that of the conical pendulum. From this it follows that the greatest speed reached during the swing of an ordinary pendulum just equals the uniform speed of the conical pendulum. That the apparent motion you are observing is really that of an ordinary pendulum you will soon prove for yourself to your entire satisfaction; and here let me say that one principle or fundamental fact seen in an experiment and patiently reflected on is worth a chapter of verbal descriptions of the same experiment.

Suppose that the ball goes round the circle of Fig. 2 in two seconds; then, as the circumference is divided into sixteen equal parts, the ball moves from 1 to 2, or from 2 to 3, or from 3 to 4, and so on in one-eighth of a second. But to the observer who looks at this motion in the direction of the plane of the paper the ball *appears* to go from 1 to 2, from 2 to 3, from 3 to 4, &c., on a line A B, while it really goes from 1 to 2, from 2 to 3, from 3 to 4, &c., in the circle. The ball when at 1 is passing directly across the line of sight, and, therefore, appears with its greatest velocity; but when it is in the circle at 5 it is going away from the observer, and when at 13 it is coming toward him, and, therefore, although the ball is really moving with its regular speed when at 5 and 13, yet it appears when at these points momentarily at rest. From a comparison of the similarly numbered positions of the ball in the circle and on the line A B, it is evident that the ball appears to go from A to B and from B back to A in the time it takes to go from 13 round the whole circle to 13 again. That is the ball appears to vibrate from A to B in the time of one second, in which time it really has gone just half round the circle. A comparison of the unequal lengths 13 to 12, 12 to 11, 11 to 10, &c., on the line A B, over which the ball goes in equal times, gives the student a clear idea of the varying velocity of a swinging pendulum.

Fig. 3 represents an upright frame of wood standing on a platform, and supporting a weight that hangs by a cord. A A is a flat board about 2 feet (61 centimetres) long and 14 inches (35·5 centimetres) wide. B B are two uprights so high that the distance from the under side of the cross-beam C to the platform A A is exactly 41$\frac{1}{10}$ inches (1 metre and 45 millimetres). The cross-beam C is 18 inches (45·7 centimetres) long. At D is a wooden post standing upright on the platform. Get a lead disk, or bob, 3$\frac{3}{16}$ inches (8 centimetres) in diameter, and $\frac{5}{8}$ inch (16 millimetres) thick. In the centre of this is a hole 1 inch (25 millimetres) in diameter. This disk may easily be cast in sand from a wooden pattern. At the tinner's we may have made a little tin cone 1$\frac{3}{16}$ inch (30

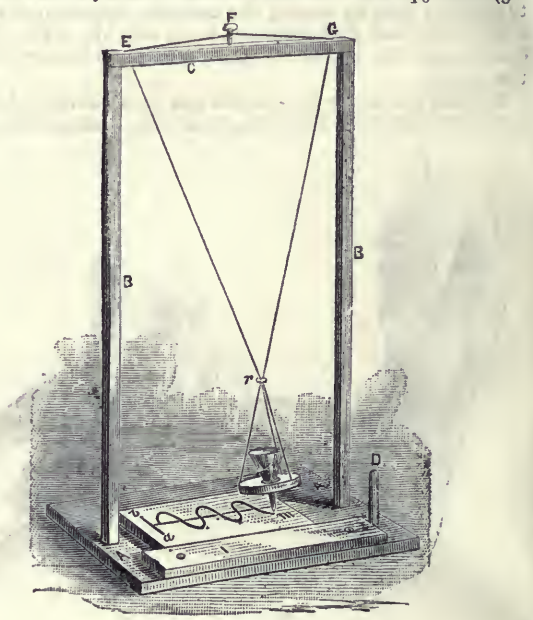


FIG. 3.

millimetres) wide at top, and 2¼ inches (57 millimetres) deep, and drawn to a fine point. Carefully file off the point till a hole is made in the tip of the cone of about $\frac{1}{16}$ inch in diameter. Place the tin cone in the hole in the lead disk, and keep it in place by stuffing wax around it. A glass funnel, as shown in the figure, may be used instead of the tin cone. With an awl drill three small holes through the upper edge of the bob at equal distances from each other. To mount the pendulum, we need about 9 feet (271·5 centimetres) of fine strong cord, like trout-line. Take three more pieces of this cord, each 10 inches (25·4 centimetres) long, and draw one through each of the holes in the lead-bob and knot it there, and then draw them together and knot them evenly together above the bob, as shown in the figure. On the cross-bar, at the top of the frame, is a wooden peg shaped like the keys used in a violin. This is inserted in a hole in the bar—at F in the figure. Having done this, fasten one end of the piece of trout-line to the three cords of the bob, and pass the other end upward through the hole marked E; then pass it through the hole in the key F; turn the key round several times; then pass the cord through the

hole at G, to the bob, and fasten it there to the cords. Then get a small bit of copper wire and bend it once round the two cords just above the knot, as at *r* in the figure. This wire ring, and the upright post at the side of the platform, we do not need at present, but they will be used in future experiments with this pendulum.

Tack on the platform A A a strip of wood I. This serves as a guide, along which we can slide the small board *m*, on which is tacked a piece of paper.

Experiment 5.—Fill the funnel with sand, and, while the pendulum is stationary, steadily slide the board under it. The running sand will be laid along L M, Fig. 4, in a straight line. If the board was slid under the sand during exactly two seconds of time, then the length of this line may stand for two seconds, and one-half of it may stand for one second, and so on. Thus, we see how time may be recorded in the length of a line.

Brush off the heaps of sand at the ends of the line, and

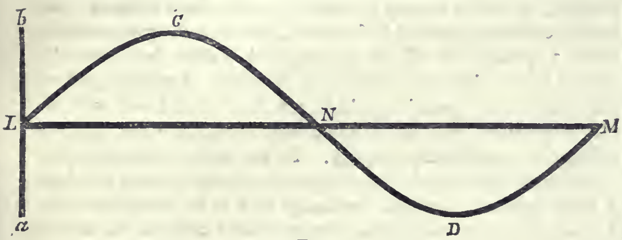


FIG. 4.

bring the left-hand end of the sand-line directly under the point of the funnel, when the latter is at rest. Draw the lead bob to one side, to a point which is at right angles to the length of the line, and let it go. It swings to and fro, and leaves a track of sand, *a b*, which is at right angles to the line L M, Fig. 4.

Suppose that the pendulum goes from *a* to *b*, or from *b* to *a*, in one second, and that, while the point of the funnel is just over L, we slide the board so that, in two seconds, the end M of the line L M comes under the point of the funnel. In this case the sand will be strewed by the pendulum to and fro, while the paper moves under it through the distance L M. The result is that the sand appears on the paper in a beautiful curve L C N D M. Half of this curve is on one side of L M, the other half on the opposite side of this line.

The experimenter may find it difficult to begin moving the paper at the very instant that the mouth of the funnel

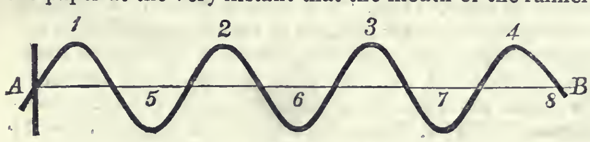


FIG. 5.

is over L; but, after several trials, he will succeed in doing this. Also, he need not keep the two sand-lines, L M and *a b*, on paper during these trials; he may as well use their traces, made by drawing a sharply-pointed pencil through them on to the paper.

By having a longer board, or by sliding the board slowly under the pendulum, a trace with many waves in it may be formed, as in Fig. 5.

As the sand-pendulum swung just like an ordinary pendulum when it made the wavy lines of Figs. 4 and 5, it follows that these lines must be peculiar to the motion of a pendulum, and may serve to distinguish it. If so, this curve must have some sort of connection with the motion of the conical pendulum, described in Experiment 4. This is so, and this connection will be found out by an attentive study of Fig. 6.

In this figure we again see a wavy curve, under the same circular figure which we used in explaining how the motion of an ordinary pendulum may be obtained from the motion of a conical pendulum. This wavy curve is made directly from measures on the circular figure, and certainly bears a striking resemblance to the wavy trace made by the sand-pendulum in Experiment 5. You will soon see that to prove that these two curves are precisely the same is to prove that the apparent motion of the conical pendulum is exactly like the motion of the ordinary pendulum.

The wavy line of Fig. 6 is thus formed:—The dots on A B, as already explained, show the apparent places of the ball on this line, when the ball really is at the points correspondingly numbered on the circumference of the circle. Without proof, we stated that this apparent motion on the line A B was exactly like the motion of a pendulum. This we must now prove. The line L M is equal to the circumference of the circle stretched out. It is made thus:—We take in a pair of dividers the distance I to 2, or 2 to 3, &c., from the circle, and step this distance off sixteen times on the line L M; hence L M equals the length of the circumference of the circle. *In time* this length stands for two seconds, for the ball in Experiment 4 took two seconds to go round the circle. This same length, you will also observe, was made in the same time as the sand-line L M was made in Experiment 5. In Fig. 6 the length L M, of two seconds, is divided into sixteen parts; hence each of them equals one-eighth of a second, just as the same lengths in the circle equal eighths of a second. Thus the line L M of Fig. 6, as far as a record of time is concerned, is exactly like the sand-line L M of Experiment 5, and the line A B of Fig. 6, in which the ball appeared to move, is like the line *a b* of Fig. 4, along which the sand-pendulum swung.

Now take the lengths from I to 2, I to 3, I to 4, I to 5, and so on, from the line A B of Fig. 6, and place these lengths at right angles to the line L M at the points I, 2, 3, 4, 5, and so on; by doing so, we actually take the distances at which the ball appeared from I (its place of greatest velocity), and transfer them to L M; therefore, these distances correspond to the distances from L M, Fig. 4, to which the sand-pendulum had swung at the end of the times marked on L M of Fig. 6.

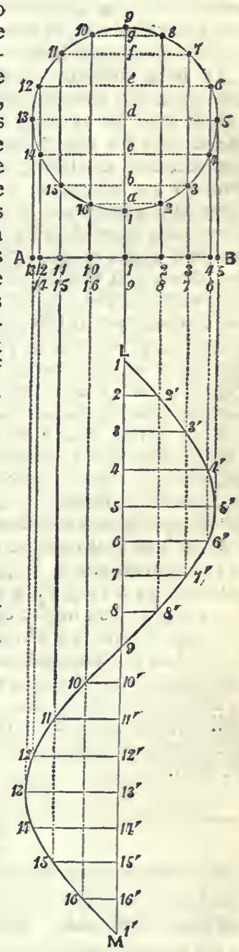


FIG. 6.

Join the ends of all these lines, 2 2′, 3 3′, 4 4′, &c., by drawing a curve through them, and we have the wavy line of Fig. 6.

This curve evidently corresponds to the curve L C N D M of Fig. 4 made by the sand-pendulum; and it must be evident that, if this curve of Fig. 6 is *exactly* like the curve traced by the sand-pendulum in Experiment 7, it follows that the *apparent* motion of the conical pendulum, as seen in the plane in which it revolves, is exactly like the *real* motion of an ordinary pendulum.

Experiment 6.—To test this, we make on a piece of paper one of the wavy curves exactly as we made the one in Fig. 6, and we tack this paper on the board L M of

the sand-pendulum, being careful that when the board is slid under the stationary pendulum the point of the funnel goes precisely over the centre line L M (Fig. 9) of the curve.

Now draw the point of the funnel aside to a distance from the line L M equal to one-half of A B, or, what is the same, from 5 to 5′ of Fig. 9. Pour sand in the funnel, and let the bob go. At the moment the point of the funnel is over L, slide the board along so that when the point of the funnel comes the third time to the line L M, it is at the end M of this line. This you may not succeed in doing at first, but after several trials you will succeed, and then you will have an answer from the pendulum as to the kind of motion it has, for you will see the sand from the swinging pendulum strewed precisely over the curve you placed under it. Thus you have conclusively proved that the apparent motion of the conical pendulum, along the line A B, is exactly like the swinging motion of an ordinary pendulum.

As it is difficult to start the board with a uniform motion at the very moment the pendulum is over the line L M, it may be as well to tack a piece of paper on the board with no curve drawn on it, and then practise till you succeed in sliding the board under the pendulum, through the distance L M, in exactly the time that it takes the pendulum to make two swings. Now, if you have been careful to have had the swing of your pendulum just equal to A B, or from 5 to 5′ on the drawing of the curve, you will have made a curve in sand which is precisely like the curve you have drawn; for, if you trace the sand-curve on the paper by carefully drawing through it the sharp point of a pencil, and then place this trace against a window-pane with the drawing of the curve, Fig. 6, directly over it, you will see that one curve lies directly over the other throughout all their lengths.

This curve, which we have made from the circle in Fig. 6, and have traced in sand by the pendulum, is called *the curve of signs*, or the *sinusoid*. It is so called because it is formed by stretching the circumference of a circle out into a line and then dividing this line, L M of Fig. 6, into any number of equal parts. From the points of these divisions 1, 2, 3, 4, 5, &c., of L M, we erect perpendiculars 2 2′, 3 3′, 4 4′, 5 5′, &c., equal to the lines *a* 2, *b* 3, *c* 4, *d* 5, &c., in the circle. These lines in the circle are called *sines*, so when we join the ends of these lines, erected to the straightened circumference by a curve, we form the curve of sines, or the sinusoid.

The sinusoid occurs often during the study of natural philosophy. We may meet with it again in our book on the nature of light, and it certainly will occur in our book on heat.

(*To be continued.*)

NOTES

UP to the present time the ignorance of those who did not know that the Archbishop of Canterbury was a degree-giving body was pardonable. It is so no longer. A serious alteration in the arrangements of these diplomas is now announced. Archbishop Tait, while he intends to dispense doctorates as before at his will and pleasure, has determined that his degree of M.A. is from December next to be a matter of examination. The standard is to be that of "honour examinations in the Universities." There is to be due choice of subjects, among which, however, Greek and Latin are not to be compulsory, though English literature is. To qualify for examination, formal testimonials required for University matriculation, with the addition of a certificate from the Bishop of the diocese whence the candidates come, are required. As the *Daily News* puts it, "the Archbishop has evidently determined to make himself into a university with all the paraphernalia which the modern conception of such a body requires." Both the London Examining Board (commonly called the London University) and Owens College are to be congratulated on the publicity now given to this singular system of granting degrees. The London Examining Body is not a teaching body, neither is the Archbishop, but the Archbishop is a university, therefore the London University system is perfect, and all methods of education whatever may be disregarded so that a standard of instruction is reached. Owens College as a teaching centre which has won its way to general esteem and confidence, may now bide its time, for this last grotesque thing calling itself a university will either make the power of granting degrees, and degrees themselves ridiculous, or direct attention to the whole subject.

ALTHOUGH the Paris meeting of the Iron and Steel Institute has not called for any lengthy notice at our hands, there are passages in Dr. Siemens' admirable address to which we cannot too strongly draw attention, and which we are anxious to place on record in our columns. He remarked that "Whilst the English, to realise a novel proposition, make bold attempts, not always carefully matured beforehand, the French systematically study a question in all its aspects, and fortify their views by careful inquiry into the experience obtained elsewhere, before they commence operations which are then carried out with all the economical and other advantages resulting from such an exhaustive preliminary inquiry. If we seek a cause for the remarkable aptitude of adapting means to special ends, to which I have referred, we shall probably find it in the advantages France and other continental countries have enjoyed for at least a generation of a more extended technical education than we could boast of, and of the personal influence which has been exercised by a line of scientific writers and experimentalists, of whom I shall only mention here such honoured names as those of Réaumur, Ebelmen, Régnault, Pouillet, Péclet, Thomas, and Le Châtelier, as belonging to the past, and of Deville, Grüner, Lan, Laurens, Jordan, Frémy, and Dumas, who are fortunately still among us. It is chiefly to such men as these that France owes her admirable system of education, which enables her to place her metallurgical establishments under the guidance of men who are scientifically qualified for the discharge of their respective duties, and for the attainment of practical results which may well excite our admiration." The organisation of the École Centrale, the creation of M. Dumas, recommends itself, as it may well do, to Dr. Siemens, and he points out that the only establishment in Great Britain comparable with the École Centrale as regards metallurgy is our School of Mines, which, "if it were installed in a capacious building, and had other branches of knowledge added to its curriculum, might easily, under the guidance of such men as Percy, Smyth, Frankland, and Huxley, be developed into an institution which would give rise to beneficial results difficult to overestimate." Had Dr. Siemens been speaking in England he would doubtless have added that this was the distinct recommendation made by the Duke of Devonshire's Commission after a long inquiry. The Government has not yet acted upon this recommendation, and the result is that students of the School of Mines have to get their mathematics when and how they can; they form no part of the curriculum. Many may think that such schools in France are too heavily weighted with mathematics, but to omit the subject altogether is to court Scylla with a vengeance. Why should not each student of the School of Mines receive, as at the École Centrale, a three years' course of general scientific education, including the higher branches of mathematics, as well as physical science, pure and applied chemistry, geology, mechanics, metallurgy, and mineralogy.

MR. H. FORBES, F.L.S., is about to leave this country to investigate the fauna and flora of Celebes, Borneo, and adjacent islands. He proposes to devote five or six years to the work.

VESUVIUS is now giving some very definite signs of an eruption.

MR. JOHN PENN, F.R.S., the eminent marine engineer, died on Monday last, at Lee, in his seventy-third year. Mr. Penn's various patents for marine engines were considered so good that no fewer than 740 British war vessels were fitted with his machinery. Among them were the *Warrior*, the *Black Prince*, the *Achilles*, the *Hercules*, and the *Sultan*. Messrs. Penn also supplied the engines for nearly all the largest war ships for the Italian, Spanish, Brazilian, German, Danish, and Peruvian Governments, and those for the yachts of the Queen, the Emperor of Russia, the Khedive, the Sultan, and the Emperor of Austria. Mr. Penn was elected a member of the Institution of Civil Engineers in 1828, and a Fellow of the Royal Society in 1859. He was also a past president of the Society of Mechanical Engineers, and had received many marks of distinction from various foreign governments.

MESSRS. BARRAUD AND JERRARD are to be congratulated on the latest issue of their fine plate containing a collection of portraits of Fellows of the Royal Society produced from photographs and printed in "permanent print." Most of the portraits and many of the *poses* are very lifelike, and the introduction of so many portraits into one picture, while retaining a certain artistic effect, has not been accomplished without a considerable overcoming of difficulties. The full-length portraits of the Astronomer-Royal, Sir J. Lubbock, Dr. Richardson, Dr. Siemens, Dr. Lister, Dr. Spottiswoode, and Prof. Martin Duncan, in the fore-front, are all excellent. A convenient key accompanies the plate.

THE Seth Thomas Clock Company have recently introduced a time-keeper with a novel and most useful addition. This consists of a perpetual calendar, the day, date, and month being shown on a second dial. We have personally tested the action of this part and can state that the mechanism takes ordinary leap years perfectly into account. Indeed, the novelty is a marvel of ingenuity. We hope later to be able to give more details.

THE Trustees of the Australian Museum seem not only to have an unhappy knack of getting into hot water, but a strong feeling that it is good to remain in it. Without entering into the quarrel between them and the late curator, any one will regret the course of action thus referred to in the last report "to his Excellency, the Governor-in-Chief":—"The Trustees regret to state that, notwithstanding their strenuous endeavours to bring all disputes with their late Curator to a satisfactory conclusion, and to relieve the institution of the custody of the whole of the property which they could admit to belong to him, that gentleman has thought fit to bring an action to recover certain medals awarded to him as Curator of the Museum in respect of property of the Museum exhibited on various occasions, at the expense of the Museum, and certain specimens and articles of clothing and furniture alleged to have been detained by the Trustees. The Crown Solicitor was authorised by the Minister of Justice and Public Instruction to defend the action, which came on for trial in the Supreme Court on November 6, and resulted in a verdict against the Trustees of 50*l*. damages for the temporary detention of the plaintiff's property, which had been returned to him before the commencement of the action, and the further sum of 850*l*. in addition to the sum of 25*l*. which the Trustees had been advised to pay into court as amply sufficient to cover the value of the articles to which the plaintiff could show even a colour of title. Notwithstanding the astonishment of the Trustees at this most unexpected result, they yielded to the advice offered by their counsel and by the court, *and offered by way of compromise to give up the medals and other property claimed, and to pay the further sum of* 175*l*. *in addition to the* 25*l*. *paid into court*, but this offer was rejected by the plaintiff, whereupon a *rule nisi* for a new trial was granted by the court."

THE daughter of Laplace has offered an excellent picture of her father to be copied. The family of Arago have likewise offered a picture of the illustrious astronomer. A large and excellent picture of Leverrier was in the hands of M. Bischofsheim, who wrote a letter to Admiral Mouchez, wishing him to take possession of it, at his earliest convenience, on behalf of the Observatory.

ON October 4–6 the annual meeting of ornithologists will take place at Berlin. The following papers will be read:—On the birds of the Danube forty years ago and at the present time, by E. von Homeyer (Stolp), president of the society; On the recent researches in the osteology and myology of birds with regard to classification, by Prof. Blasius (Brunswick); On an ornithological excursion to Hungary and Croatia, by Dr. Brehm (Berlin); On the latest collection of birds from Eastern Africa, by Prof. Cabanis (Berlin); On the birds of the Caucasus, by Dr. Radde (Tiflis); On the latest acquisitions of the Zoological Museum of Berlin, with special reference to the nests and eggs of African birds, by Dr. Reichenow (Berlin); On the importance of splanchnology with regard to systematics, by Herr Gadow (Algiers); On the progress of ornithology since 1875, by Herr Schalow (Berlin).

A LARGE and very brilliant meteor was observed at numerous places in Central Germany on September 6. Near Hanau it appeared in the south-east about 9.10 P.M., and looked like a large comet, with a solid nucleus and a long train of light stretching across the sky to the north-west. It appeared suddenly like a flash of lightning, and, when the nucleus had disappeared, the line of light yet remained, and little stars could be noticed in it by the naked eye. The total duration of the phenomenon was about thirty seconds.

ON September 15, at 6 to 7 P.M., a large meteor was observed at Montpellier. On the same day and at about the same time, one was witnessed at Tenez, and Constantine in Algeria, and a number of places at great distances from each other. It is not yet known whether it was the same body or if the earth met in its course a meteor swarm. The bolides were notable for their brilliancy and their duration. It is reported that at Constantine the phenomena were accompanied by noise.

A SHORT but violent shock of earthquake was felt at Buir, near Düren (Rhenish Prussia), on September 2, at 9.15 P.M. Indeed it is stated that since the great earthquake of August 26 shocks have been felt in that district almost daily. Another shock is reported from Remagen, on the Rhine, on September 3, at 1 A.M., and a third one from Wiesbaden and neighbourhood on September 14, at 11.35 P.M. The latter was particularly violent.

ACCORDING to the last official calculation the total receipts of the Exhibition from entrance-money will reach 13,000,000 francs up to the end of October. There are other sources of revenue and a subvention from the City of Paris. It is supposed that the deficit will not exceed 400,000*l*., which will be more than covered from various sources.

QUITE recently a most remarkable new cave has been discovered in the United States near Glasgow Junction, Kentucky. It has been investigated to a distance of no less than twenty-three miles in one direction and sixteen in another. Most of its passages are very broad, and it is stated that a carriage with a pair of horses has room to drive some eleven miles into the interior. It contains several very deep rivers, one of which has been traced to a distance of fourteen miles; further progress was then arrested, the cave narrowing too much to allow a boat to pass. The cave is described to be "most wonderful," by an American contemporary, and is said to surpass in grandeur all

other caves hitherto known, the Mammoth Cave not excepted. Some human remains, resembling Egyptian mummies, were also found in the cave. They were contained in stone sarcophagi of rough structure. The floor of the cave is extremely uneven, full of fissures and irregularities, so that it seems as if violent volcanic phenomena had taken place here. The new cave has been baptised with the more or less appropriate name Grand Crystal Cave. Our readers are doubtless aware that the Mammoth Cave is also situated in the vicinity of Glasgow Junction, Kentucky.

AMONG the resolutions passed by the International Congress on Weights, Measures, and Coins, at Paris, was the following:—The Congress learns with pleasure the progress of the metric system; it deplores that England, Russia, and the United States have not yet entered into the same path; and it is of opinion that the Governments of those countries should be solicited to give effect as early as possible to an act of progress so eminently useful to science, commerce, and international relations." The British and American members had a separate meeting, and resolved to petition their respective Governments to appoint a mixed commission to consider the adoption of the metric system by both countries, and to make all necessary recommendations for the proper legislation to secure the desired end.

THE conchological collection formed by the late Dr. Marie, of New Caledonia, has been purchased by Mr. Bryce-Wright.

AT the Royal Foundry of Munich a colossal monument cast in bronze was exhibited from September 8 to 11. It is intended for St. Louis, U.S., and represents Alexander von Humboldt; the design is by Herr von Müller, jun. The figure is some 3½ metres in height, and represents Humboldt in the freshness of manhood, leaning against a stump of a tree, upon which he has deposited his mantle; in the right hand he holds a map. The statue will be placed upon a stone pedestal, which is in course of construction at St. Louis, and which will be adorned by three relief medallions by the same artist, the one on the front showing the features of the founder of the monument, Mr. Henry Shaw, of St. Louis.

THE following are the more important addresses delivered and papers read at the sectional meetings of the German Naturalists' Association at Cassell:—Mathematical and Astronomical Section: On some new solutions of the problem of the division of the ball, by Prof. E. Hess (Marburg); On the old calculating machine of Leibnitz, by Prof. Listing (Göttingen); On the solar parallax, by the same. Prof. Listing gives the value $\pi = 8''\cdot8786$ as the most correct one at present.—Physical and Meteorological Section: On some new magnetic phenomena, by Dr. Fromme (Göttingen); On the influence of interior friction upon resonance, by Dr. Auerbach (Breslau); On a new method of employing the induction of the earth for the determination of the magnetic inclination, by Dr. Schering (Göttingen); On a new apparatus for the distillation of mercury, by Dr. Weber (Kiel); On a case of pleochroism, by Prof. Staedel (Tübingen); On a new method and apparatus to determine low tensions of vapours, by Dr. Moser (Berlin); On some applications of total reflexion, by Prof. Kohlrausch (Würzburg); On the theory of the exchange of air through porous walls, by Prof. Recknagel (Kaiserslautern); Remarks on galvanometry, by Prof. Töpler (Dresden); On the point of gravitation in curves, planes, and solid bodies, by Dr. Feussner (Marburg); Record of observations of solar spots and protuberances, by Prof. Spörer (Potsdam); On the behaviour of different heat colours in the reflection of polarised rays from metals, by Prof. Knoblauch (Halle); On the mathematical theory of friction electricity, by Dr. Schering (Göttingen); On an improvement in the hair hygrometer, by Dr. Nippoldt (Frankfort-on-the-Main). Chemical Section: On the chemical constitution of the turnip molasses, by Prof. Gunning (Amsterdam); On some new platina compounds, viz., platinofulminates, by Prof. von Meyer (Leipzig); On pyromeconic acid, by the same; On a new apparatus for the combustion of organic substances containing halogens, by Dr. Kopfer (Bonn); On the conversion of starch into sugar, by Dr. Salomon (Brunswick); On a physically isomeric modification of dinitrochlorobenzol, by Prof. Daubenheimer (Giessen); On the conditions of life of the lower organisms, by Prof. Gunning (Amsterdam); On a new method in the preparation of bromine, by Dr. Frank (Stassfurt); On some new phenomena concerning gases, by Prof. Mitscherlich. Geological and Palæontological Section: On the heat of the earth, by Herr E. Dunker (Halle); On some animal footmarks in the coloured sandstone of Carlshafen, by Dr. Hornstein; On the geological condition of the Büdingen forest, by Dr. Bücking; On an explanation of earthquakes, by Herr von Dücker; On the tridymit of Friedrichroda (Thuringia), by Dr. Luedecke; On basalt and its decay, by Dr. Franke; On the geological condition of the Kyffhäuser mountain, by Dr. Moesta. Geographical Section: On the botanical aspect of the Caucasus, by Dr. Raddé; On an exploring expedition to San José de Cucuta (South America), by Dr. K. Müller (Halle); On mountain and valley winds and their effect upon the vegetation of volcanic mountains, by Prof. Rein (Marburg); Ethnological researches in the Island of Cyprus, by Herr Schmölder (Frankfort); On the progress of the commercial relations between Europe and Western Siberia, by Dr. M. Lindemann (Bremen). Numerous papers and treatises were read at the zoological, botanical, anatomical, and physiological section-meetings, but by far the greater part were of minor interest, most of them having special reference to medicine only. The medical sections were as numerous as last year at Munich, and the attendance was, if anything, greater than on any previous occasion.

THE Aeronautical Society has issued its twelfth annual report. To it, in accordance with the expressed intention to reprint any matter of interest which might be otherwise unattainable, is annexed a reprint of a pamphlet printed in the year 1810 by Thos. Walker, of Hull, its title being "A Treatise on the Art of Flying by Mechanical Means." Mr. Walker was a portrait painter. An American would say that the treatise was a little "mixed."

THE physicists of the French Central Bureau of Meteorology are engaged in establishing a nomenclature to diminish the number of letters used in signalling.

THE American Academy of Arts and Sciences is probably one of the most active and efficient scientific societies anywhere; its *Proceedings* will bear comparison with those of any society in the old country. The part before us, including the period from November, 1877, to May, 1878, contains a number of papers of great value, the titles of which we shall give meantime, in the hope of being able to notice some of them in detail shortly. The Moon's Zodiacal Light; Undulations Observed in the Tail of Coggia's Comet, 1874; Sudden Extinction of the Light of a Solar Protuberance; On Saturn's Rings, by L. Trouvelot; Supplementary Note on the Theory of the Horizontal Photoheliograph, by Prof. William Harkness, U.S. Navy; Researches on the Substituted Benzyl Compounds, by C. Loring Jackson; Remarks on the Brain, illustrated by the Description of the Brain of a Distinguished Man, by Thomas Dwight, M.D.; Theory of Absorption-Bands in the Spectrum, and its Bearing in Photography and Chemistry, by Dr. Robert Amory; Surfaces of the Second Order, as treated by Quaternions, by Abbott Lawrence Lowell; On the Synonymy of some Species of Uredineæ, by W. G. Farlow; Metasomatic Deve-

lopment of the Copper-bearing Rocks of Lake Superior, by Raphael Pumpelly; Investigations in Quaternions, by Washington Irving Stringham; On a New Method for the Separation and Subsequent Treatment of Precipitates in Chemical Analysis, by F. A. Gooch; On Peirce's Criterion, by Benjamin Peirce; Note on the Measurement of Short Lengths, by Leonard Waldo; Contributions to the Botany of North America, by Asa Gray; Spherical Conics, by Gerrit Smith Sykes; On the Influence of Internal Friction upon the Correction of the Length of the Second's Pendulum for the Flexibility of the Support, by C. S. Peirce; Colour Perception, by G. Stanley Hall; On the Intensity of Terrestrial Magnetism at Cambridge, by Henry Goldmark. Among the foreign honorary members we find the names of J. C. Adams, Airy, Cayley, Sylvester, Clerk-Maxwell, Balfour Stewart, Stokes, Sir Wm. Thomson, Darwin, Joule, W. H. Miller, A. C. Ramsay, Sabini, Bentham, Hooker, Owen, Max Müller, Rawlinson, Gladstone, Tennyson.

THE receipts of the Giffard Captive Balloon on the first sixty days have been more than 500,000 francs. The sum spent on the construction of the balloon has been realised. It is supposed that the receipts for the month of October will be sufficient to cover the working expenses, so that M. Giffard will be rewarded for his enterprise by the possession of the balloon, machinery, and gas-producing apparatus.

A MILLION tickets have been sold at one franc each for the Paris Exhibition Lottery. Two-thirds of the sum are to be spent in purchasing prizes, the other third being destined to assist the Government in paying the travelling expenses of the working men visiting the Exhibition.

THE additions to the Zoological Society's Gardens during the past week include two Ostriches (*Struthio camelus*) from Africa, presented by the Hon. H. C. Vivian, H.B.M. Consul-General; two Secretary Vultures (*Serpentarius reptilivorus*) from South Africa, presented by C. Rivers Wilson, C.B.; an Oriental Eagle Owl (*Bubo orientalis*) from Karenee, Siam, presented by Mr. Charles Fowler; two Prairie Marmots (*Cynomys ludovicianus*) from North America, two Smaller Rattlesnakes (*Cerotalus miliarius*) from Canada, presented by Mr. Wilfred G. Marshall; two Egyptian Gazelles (*Gazella dorcas*) from Egypt, presented by Mr. Thomas Moss; three Reddish Finches (*Spermophila nigro-aurantia*), one Bluish Finch (*Spermophila cærulescens*), one Half-white Finch (*Spermophila hypoleuca*), one Tropical Seed Finch (*Oryzoborus torridus*) from South America, presented by Mr. R. C. Batterbee; three Rufous Tinamous (*Rhynchotus rufescens*) from Brazil, presented by Mr. J. A. Iliffe; two Lesser Black-backed Gulls (*Larus fuscus*), British Isles, presented by Mr. A. H. Cocks, F.Z.S.

THE FIGURE AND SIZE OF THE EARTH[1]

II.

IN addition to the measurement by Picard above-mentioned, two other arcs were measured in a north and south direction; La Hire measured northward towards Dunkirk, and Cassini southwards towards Perpignan. The result, published by Cassini in the year 1718, was as follows:—The southern arc gave 57098 toises (Picard's was 57060), and the northern 56960. This result was quite opposed to Newton's theory; it indeed favoured an elongated figure for the earth. There now began among the learned of the time a controversy which was carried on with much bitterness, between the supporters of Newton and Huyghens on the one hand, and of Cassini on the other. Cassini published the results of his measurements in his work, "De la Grandeur et de la Figure de la Terre" (Paris, 1722), and in consequence of the high reputation which he, as Director of the Observatory and member of the Academy, possessed over all France, nearly all the French *savants* took his side. But the arguments adduced by him were not such as could convince the great number of Newton's followers in all other nations. The French results were all the more objected to, that the measured arcs were much too small to allow one to base thereon a conclusion as to the form of the earth. In order to bring to an end the controversy carried on with so much violence on both sides, the French Government sent out in the year 1735 an expedition, consisting of the astronomers Bouguer, de la Condamine, and Godin, to Peru to measure the length of an equatorial degree. A second expedition, consisting of the academicians Maupertuis, Clairaut, and Lemonnier, was sent to Lapland; and while the former found the length of a degree at the equator to be 56753 toises, the latter, in connection with Celsius, found the result in latitude 66° 20′ to be 57437 toises. These results, obtained from the most careful observations and the most accurate calculations, gave the palm to the Newtonian theory, and the amount of flattening as ascertained so nearly agreed with Newton's calculation as to give the greatest confidence in his works.

It should, however, be mentioned that the Lapland measurement was much behind the Peruvian in correctness. Indeed it was soon seen that it was much more inaccurate than Picard's, and therefore the flattening of the earth was based only on Picard's and the Peruvian measurements. But the admirable execution of the Peruvian measurements was of importance also in another respect; in it, two base-measurements had been made, the southern base being considered a base of verification. This importance, which appeared at a later period, consisted in the fact that the unit of measure used in laying down the base-line, the "Toise of Peru," after it had with the greatest care been brought to Paris uninjured, was instated as the French normal measure, and this standard, at a temperature of 13° R., was appointed as the legal measure of length for France.

At a later period Freiherr von Zach reduced the length[1] of an equatorial degree to the sea-level and found it to be 56732 toises. He made use for this purpose of a second measurement at the equator, carried out by the Spaniards (assisted by Godin) between Cuenza and Mira, which embraced a length of 3° 26′ 52″ and gave 56768 toises as the length of a degree. Since the careful measurement of a degree in Peru—which put a final end to the opposition to the Newtonian theory of the figure of the earth as opposed to the view of Cassini, and proved to all the world as an undoubted fact, that the inhabitants of the earth did not live upon a perfectly spherical planet, but on one flattened at the poles; since then has also grown the universal desire for accurate knowledge of the dimensions of the earth, as from the amount of its deviation from the spherical form, we expect to form important conclusions as to the origin and development of our planet. Meantime the great progress made in methods of measurement and in instruments, combined with the beautiful results of the constantly-developing mathematical sciences, now promised the best results for new undertakings in reference to measurements of the earth.

Although the degree measurements carried out up to the middle of last century left no further doubt as to the spheroidal form of the earth, yet, as already said, the Peruvian measurement was the only one that had been carried out with the greatest possible accuracy. But on combining this arc with the French, Lapland, and other known measurements, only differing from it in point of accuracy, results differing much from each other were obtained. If we represent the polar and equatorial semi-diameters by the letters a and b, the quotient $\frac{a-b}{a}$ represents the amount of flattening. We thus obtain, by combining the Peruvian and the Lapland measurements, the value $\frac{1}{215}$, the Peruvian and French $\frac{1}{304}$, and lastly, the French and Lapland $\frac{1}{145}$. To understand in what way, by means of the data for the length of a degree, and the combining of the data for two or more different parts of the earth's surface, it would lead us to the amount of the flattening, requires considerable knowledge of the higher mathematical methods, and we must not, therefore, enter here on this part of the subject.

The great differences between the three values referred to, showed how untrustworthy were the measurements hitherto obtained, and this led to endeavours in many quarters to come nearer to the truth by better measurements. Lacaille availed himself in 1750 of his stay at the Cape of Good Hope to carry out the measurement of a degree, and found for S. lat. 33° 18′ 30″,

[1] Continued from p. 558.

[1] This probably refers to the reduction by the Baron de Zach of the observations made in the Peruvian operations.—*Mon. Corresp.*, xxvi., p. 52.

the length to be 57037 toises; and if this measurement was not carried out with the greatest care, since Lacaille could only devote two months to it, yet it was so far of no small importance, that it was the first which had been effected in the southern hemisphere. In the years 1751–53, Boscovich and Le Maire carried out a triangulation in the States of the Church, in 40° N. lat., and found the length of the degree to be 56973 toises. A degree-measurement made in the plains of Turin in 1768, between Andrate and Mondovi, gave for 44° 44′, N. lat., a degree length of 57024 toises. Mention should be made also of a series of extended measurements in Austria; and we may remark that most of the operations above referred to, as well as some of the following, were undertaken at Boscovich's instigation, while the Austrian operations were initiated and carried out by the Jesuit Liesganig. He found the length of a degree for N. lat. 48° 43′ to be 57086 toises, and for 45° 57′, 56881 toises. It may be seen from a comparison of these two values that, notwithstanding the small difference of latitude, they indicate a flattening at the poles, and if the calculation based upon these values alone give an anomalous result, it must be ascribed to the much too small difference of latitude. Indeed, errors of measurement were subsequently found in them. Finally we have to mention as belonging to this period the measurements after the old method which were carried out by direct measurement of a long meridian distance by means of a surveyor's chain. The one was in America, on the plains of Pennsylvania, by Mason and Dixon[1] over a distance of 1⅓ degree, and it gave, in N. lat 39° 11′ 56″, 56888 toises for the degree-length. The second measurement of this kind was in Bengal in 1790, by Burrow and Dalby, and it resulted in giving 56725 toises as the length of degree in N. lat. 23° 18′.

During the first forty years of the latter half of the eighteenth century a great number of geodetic operations were undertaken in various parts of the earth, and it was sought by various combinations of these measurements to ascertain the amount of polar flattening; but it was soon found that, with the exception of the Peruvian undertaking, they were too full of errors to yield a satisfactory result. The scientific men of that time soon became convinced of this drawback, and efforts were made by various academies not only to discover improved methods of measurement, but also by offering prizes to induce mechanics to perfect instruments, more especially the chronometer, so indispensable to astronomical observations. Both courses were followed with good results, and by English mechanics especially astronomical and geodetic instruments of measurement were brought to a high degree of perfection.

Strange though it may seem, France, with her revolutionary troubles coming fast upon her, was the first to commence the subsequent highly accurate geodetic operations. The multiplicity of units of measure had at this time reached its *ne plus ultra*. Not only each little territory, each separate province, but often each town had its own peculiar measure of length; and the case was nearly as bad with regard to weight, endless difficulties and disputes being the result. It was first resolved in 1790, in the French National Assembly, to come to an understanding with England on the length of the seconds pendulum, but after a year the French *savants* declared that, seeing that the seconds pendulum would be of different lengths at different parts of the earth, it would be more advantageous to adopt a given measure of the earth itself as unity, and that as such the ten-millionth part of the earth's quadrant should be taken. But, to settle this point definitely, it was necessary to measure a long arc of meridian with the greatest possible accuracy, and accordingly, March 30, 1791, it was decided to measure the meridian arc between Dunkirk and Barcelona, from which the length of the quadrant and its ten-millionth part, the metre, could be inferred. After the length of the seconds pendulum in France had been accurately observed, measuring operations were at once commenced, and thus began the great geodetic operation in France, afterwards carried on to the Balearic Isles, and in our own time but little surpassed. Notwithstanding revolutionary storms the operations were carried on and with unvarying accuracy. This measurement, effected by the method of triangulation, consisted of 120 triangles, connecting the two points Dunkirk and Montjouy, near Barcelona. The length of the arc between the two points was found to be 551584 toises.

There were also three intermediate points determined astronomically, and in order that the amount of the earth's oblateness might be inferred from this measurement alone, on Mechain's representation it was carried to the Balearic Isles, and thus the middle point of the measured arc coincided approximately with the middle point of the earth's quadrant. This extension was carried out in the years 1806-8 by Biot and Arago. The entire measured arc had now an amplitude of 12° 22′ 13·44″, the length being 705,188·8 toises, and the final result for the length of a meridian degree at 45° N. lat. was 57047 toises. It is characteristic of that time that in order to obtain the length of the metre, the conclusion of this measurement, which was undertaken for this purpose, was not waited for; but a preliminary metre measure was obtained from the results of the Peruvian, the Lapland, and the old French measurements, equal to 443·443 lines of the toise of Peru. As the results of the first-measured distance, Dunkirk to Barcelona, were known in 1797, the length was changed to 443·296 lines, and two platinum rods of that length (at a temperature of 0° C.) were prepared as standard measure, one of which was deposited in the Archives of the Republic, and the other in the Paris Observatory; two copies of these in steel served as the normal measure. On December 10, 1799, the Metre was instated as the legal measure in France, while in England the length of the seconds pendulum in the latitude of London remained as the unit of measure.[1] But the original object of the great French degree measurement, to obtain a natural measure of length, was not attained, and it is erroneous to imagine that the metre is in reality exactly the ten-millionth part of the earth's quadrant; for the length of the metre was, in subsequent degree measurements, ascertained more accurately and differently. But what was then attained was more accurate information as to the extent of the earth's oblateness.

Simultaneously with these French operations was the measurement of a degree in England, which was carried out with extreme accuracy in the year 1784, with the view of a general triangulation of the country; the measurements were made by Gen. Roy, with an accuracy not previously attained. While the angles were observed with theodolites constructed with the greatest accuracy, Roy effected the measurement of a base line with long glass tubes. Again, in the years 1800–2, was a similar geodetic operation undertaken, for the purpose of measuring a degree; the result was that for latitude 51° 20′ 54″ the length of the degree was found to be 57180 toises, and for 52° 50′ 29″·8, 57017 toises The great ellipticity of the earth resulting from these numbers gave rise to the idea that the measurements were inaccurate; but it was considered later on that mountain-masses must have exercised a disturbing influence on the plummet, and that the error must be due to this cause. In the years 1801–3 a new degree measurement under the polar circle was carried out by Svanberg and Ofverbom, the results of which, determined with great care, proved the inaccuracy of the earlier Lapland measurements by Maupertuis. For lat. 66° 20′ 12″ the length of the degree was found to be 57209 toises. The operations of Major Lambton in the East Indies, not hitherto surpassed in extent, were begun in 1802, and, as the final result, the length of the degree was found in four different places between 8° and 18° N. lat.

After so great a series of degree measurements obtained at so many different parts of the earth, it was now endeavoured, on the strictest mathematical principles, to submit them all to calculation and test their accuracy. The result was that the great French, the second north polar measurement, and the last measurement carried out in England, were shown to be of such a degree of accuracy as was needed to permit of a certain determination of the figure of the earth. All other measurements had to be cast aside as inadequate; in most of them the sources of error were pointed out, and the degree of accuracy noted, but as factors in the working-out of the final results, they could not stand. In the majority of the measurements of the latter kind the sources of error belonged mainly to two classes. The one was the rude, unsatisfactory construction of the geodetic instruments; the other concerned the astronomical part of the operation, and consisted not only in the want of accuracy in the instruments, but chiefly—and this reproach touched the greater part of the *savants* concerned—in the ignorance of the use of astronomical instruments, for of the majority it is certain that before they began their geodetic measurements, they never had an astronomical instrument in their hands.

[1] The measurement by Mason and Dixon was made with deal rods.

[1] The seconds pendulum never was the unit of length in England. But in the Act of 1824 legalising the "standard yard," reference is made to the seconds pendulum, and the length of the latter (vibrating in London) is given in inches of the standard yard, with the intention that should the standard yard be destroyed, it might through the seconds pendulum be restored.

At this period, when it was resolved to overhaul the accumulated material, subjecting to further mathematical treatment what was valuable, and leaving unregarded what was faulty, then it was resolved to work out the problem theoretically in so clear and accurate a manner as to be worthy of the high standpoint of mathematical and natural science. Thus it appeared, above all, necessary to get rid of the inequalities of the earth's surface, to reduce all measurements to an ideal form of surface, the most suitable being that (according to Gauss's definition) which the still water of the ocean would assume if it covered the whole surface of the earth. It is also defined as a surface which is at every point at right angles to the direction of a free-falling body. But in order that this ideal surface might be observed in reality, researches on a large scale would have to be undertaken on the tides, in order to obtain a mean water-level. At that time, also, theory fell upon a new and suitable method of ascertaining the amount of the earth's oblation, in the theoretical perfection of the long-known phenomena of precession and nutation. And while both the theory and the practical methods of measurement were being carried to a high degree of perfection, in spite of the political storms in nearly all European nations, new preparations were made to find a worthy solution of this problem by means of the newest and best acquisitions of science. Especially now was there a people who not only emulated the noble efforts of other nations, but whose *savants*, the first of their time, were able soon to place themselves, through their thoughtful and ingenious researches, supported by a liberal people, at the head of the efforts made by nearly all civilised nations to obtain a knowledge of the truth—this was the German.

In what follows we shall explain these acquisitions as to a knowledge of our earth which have been made in our century, and in great part by our people.

The operations which have been undertaken during the present century for the purpose of obtaining an accurate idea of the figure of the earth and its dimensions, have by no means been confined, as nearly all the early operations were, to the carrying out of degree measurements; but even in the earlier periods a method already mentioned was brought prominently forward, which would not only show the form of the surface, but from which it was also expected that conclusions could be drawn as to the internal physical condition of the crust of the earth, and the manner in which the mass under the surface is distributed—we refer to pendulum measurements. We have already seen how Richer found a difference of lengths of the pendulum in Paris and Cayenne, and after Bouguer in Peru and Lacaille at the Cape had made similar observations, an idea was obtained of the law of variation of the lengths of the pendulum at different latitudes. It was soon seen from this that the differences in length of the pendulum at the extreme points, the pole and the equator, would only be very small, and that the very nicest observations would be necessary to allow conclusions to be drawn as to the form of the earth. The Spaniards were the next who, in two ships of war, carried out measurements in very different parts of the earth, but which unfortunately proved not to be of the requisite accuracy. Shortly afterwards, a new triangulation was undertaken in France, and while Laplace sought by it as far as possible to obtain data as to the oblateness of the earth, several other *savants*, especially Biot and Arago, carried on pendulum measurements along the meridian of the great degree-measurement (Dunkirk). In connection with this new triangulation, extended and exceedingly accurate measurements of longitude were carried out. In England efforts were now made to utilise triangulation for both methods of measuring degrees, and now, especially in the southern hemisphere, pendulum observations were accomplished on a scale and with an accuracy such as had not previously been known. These observations established the fact that the southern hemisphere had no essentially different condition from the northern hemisphere. There was used for this purpose a very delicate pendulum apparatus, the "Reversible Pendulum," the inventor of which was Bohnenberger, a German. From 1822 to 1824 such observations were carried out at many coast stations as far north as the Arctic Ocean, embracing an extent of 93° of latitude.

Accurate methods of observation of this kind, as also very exact and ingeniously-constructed pendulum apparatus, were now invented and brought into use mainly by German astronomers; Bessell especially has done lasting service in this respect, his method, perfected with the greatest ingenuity, being still fruitful in results.

The principle of this method, viz., from various pendulum measurements to obtain the figure of the earth—cannot be here explained, on account of the mathematical principles involved, and we can only give some of the results obtained from the above-mentioned measurements. The first Spanish measurements gave the oblateness of the earth as $\frac{1}{300}$; the French, $\frac{1}{315}$; the English results varied between $\frac{1}{230}$ and $\frac{1}{598}$. The value obtained from the earliest mentioned astronomical observations (precesssion and nutation) was $\frac{1}{299}$. The difference of the results obtained by means of pendulum measurements could not be ascribed to erroneous observations, but rather to the unequal density of the earth, as was shown quite clearly by later measurements. It was sought, especially in Germany, to discover the amount of this disturbing influence, and to obtain observations free from these disturbances. Already, in the year 1806, a German published the thus improved results of the measurements, and obtained from the various methods of observation the following nearly accordant results:—Newton's theory gave $\frac{1}{304}$; precession and nutation, $\frac{1}{304}$; the theory of the moon's motion, $\frac{1}{304}$; pendulum measurements, $\frac{1}{321}$; and

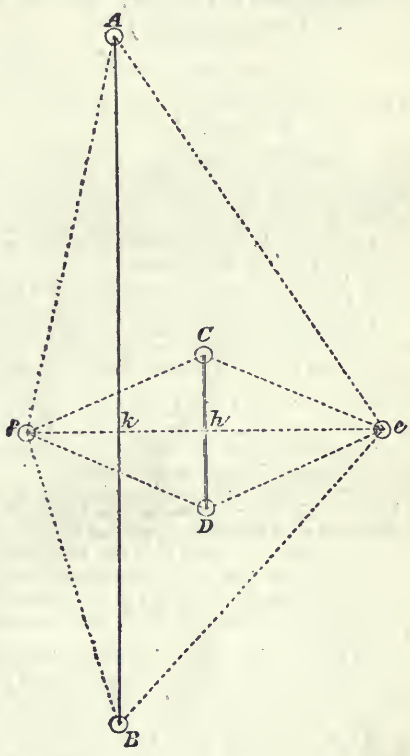


FIG. 2.

degree-measurements, $\frac{1}{304}$. Laplace and Sabine deduced, according to the newer mathematical methods, the most probable value of the oblateness of the earth, from all these results, and found, the former $\frac{1}{306}$, the latter $\frac{1}{289}$. German *savants* also repeated this calculation, and obtained, certainly not exactly the same, though very similar results; but more accurate results could only be based on more delicate measurements.

These more accurate measurements were soon carried out, mainly in Germany and Russia. Gauss, in 1821–24, measured the distance between Göttingen and Altona, and obtained for latitude, 52° 2′ 17″, 57126 toises as the length of the degree. For this purpose he had adopted greatly improved methods of observation. Schumacher made a new measurement in Denmark, and found for 54° 8′ 13″·5 the degree-length to be 57092 toises.

It may, perhaps, be interesting to show here what the improvement was which had been introduced at that time into these operations in Germany; it was the method of enlargement of the base-line, whereby the very difficult labour of base-line measurement was considerably reduced and more accurate measurements thereby became possible. For example, let the length A B be the distance to be measured, but with only a small plain surface on which a base-line can be measured (Fig. 2). The small

base-line C D is then measured with the greatest possible accuracy, and the angle formed at C and D in the directions e and f are obtained. Thus the two triangles C D e and C D f are completely given; for in each is a side (C D) and the two adjacent angles known. But thus also are their heights eh and fh given, and these added give the side ef in the great triangles. From e and f the two triangles formed towards A and B are measured, and this gives completely the two triangles A fe and B fe, as also their heights A k and B k, which added give the distance sought.[1] It will be seen at once that this method offers great advantages, especially if it be possible to obtain with the greatest accuracy the small base-line. This latter condition Bessel fulfilled at first to an astonishing degree, as he, by the introduction of a base-apparatus, attained the greatest accuracy. Bessel and Baeyer accomplished a degree-measurement between Memel and Trunz in 1831–36. They obtained for the mean latitude of the measured arc (54° 58′ 25″·5) a degree-length of 57142 toises. An operation was carried out by Maclear between 1836 and 1848 at the Cape of Good Hope, by which for south latitude 35° 43′ 20″ a degree-length of 56933 toises was obtained.

(*To be continued.*)

ON THE PRECESSION OF A VISCOUS SPHEROID[2]

I HAVE been engaged for some time past in the investigation of the precession of a viscous spheroid, with the intention of seeing whether it would throw any light on the history of the earth in the remote past. As some very curious results have appeared in the course of the work, I propose to give an account of part of them to the British Association.

The subject is, however, so complex and long, that no attempt will be made even to sketch the analytical methods employed.

In a paper of mine read before the Royal Society in May last, a theory was given of the bodily tides of viscous and imperfectly elastic spheroids; and this paper formed the foundation of the present investigation.

For convenience of diction I shall speak of the tidally disturbed body as the earth, and of the disturbing bodies as the moon and sun; moreover, in all the numerical applications, the necessary data were taken from these three bodies.

The effect of the internal friction called viscosity, is that the bodily tides in the earth lag, and are less in height, than they would be if the earth were formed of a perfect fluid.

An analytical investigation proved that the action of the sun and moon on the tides in the earth is such that the obliquity to the ecliptic, and the lengths of the day and month all become variable; the alteration in the length of the year remains, however, quite imperceptible.

But I will now explain, from general considerations, how the lagging of the tides produces the effects above referred to.

Let the figure represent the earth as seen from above the south pole, so that S is the pole, and the outer circle the equator. The rotation of the earth will then be in the direction of the curved arrow close to S. Within the larger circle is a smaller concentric one, one-half of which is drawn with a full line, and the other half with a dotted line. The full line semicircle is part of a small circle in S. latitude and the dotted one part of another small circle in the same latitude, but to the north of the equator. Generally, dotted lines indicate parts which are behind the plane of the paper.

It will make the explanation somewhat simpler, if we suppose the tides to be raised by a moon and antimoon diametrically opposite to one another; this, as is well known, is a justifiable modification of the true state of the case.

Then let M and M′ be the projections of the moon and antimoon on to the terrestrial sphere.

If the substance of the earth were a perfect fluid, or were perfectly elastic, the apices of the tidal spheroid would be at M and M′. If, however, there be internal friction, the tides will lag, and we may suppose the apices of the spheroid to be at T and T′. In order to make the subject more intelligible, the tidal protuberances are then supposed to be replaced by two equal heavy particles T and T′, which are instantaneously rigidly connected with the earth. This same idea was, I believe, made use of by Delaunay, in considering the ocean tidal friction.

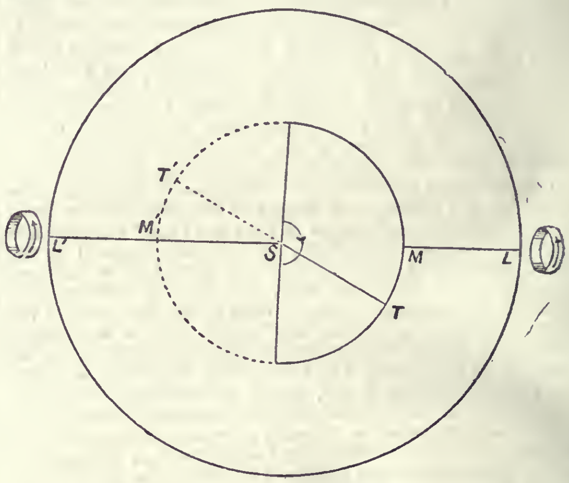
Then the attraction of the moon on T is greater than on T′; and that of the antimoon on T′ greater than on T. Hence, besides equal and opposite forces acting at the earth's centre, directly towards M and M′, there are small forces (varying as the square of the tide generating force) acting in the directions T M and T′ M′.

We will consider the effect on the obliquity first. These two forces, T M, T′ M′, clearly cause a couple about the axis L L′ in the equator, which lies in the same meridian as the moon. The couple is indicated by the curved arrows at L and L′. Now, if the effects of this couple be compounded with the existing rotation of the earth, according to the principle of the gyroscope, it is clear that the south pole S tends to approach M and the north pole to approach M′. Hence supposing the moon to move in the ecliptic, the inclination of the earth's axis to the ecliptic diminishes; in other words, the obliquity of the ecliptic increases.

Next with regard to tidal friction; the forces T M and T′M′ produce a couple about the earth's axis, S, which tends to retard the earth's rotation.

Lastly, since action and reaction are equal and opposite, and since the moon and antimoon produce the forces T M, T′ M′ on the earth, therefore the earth must cause forces on the moon and antimoon in the directions M T and M′ T′. These forces are in the same direction as the moon's orbital motion; hence the moon's linear velocity is augmented. The consequence of this is that her distance from the earth is increased, and with that increase comes an increase of periodic time round the earth.

The consequences of the lagging of the earth-tides, therefore, are an increase of the obliquity to the ecliptic, a retardation of the earth's rotation, and a retardation of the moon's mean motion.



In this general explanation it is assumed that the lagging tides are exactly the same as though the earth were perfectly fluid, and as though the tide-*raising* moon were more advanced in her orbit than the true moon, whilst the moon which attracts the tidal protuberances was the true moon. That is to say, it is assumed that the tides raised are exactly the same as though the earth were a perfect fluid, save that the time of high tide is late, and that the tides are reduced in height.

Now although this serves in a general way to explain the phenomena which result from the supposition of the earth's viscosity, yet it is by no means an accurate representation of the state of the case.

In fact the internal friction sifts out the whole tide-wave into its harmonic constituents, and allows the different constituents to be very differently affected as regards height and phase.

Thus the lagging tide-wave is not exactly such as the general explanation supposes, and the nearer does the spheroid approach to absolute rigidity the greater does the discrepancy become.

The general explanation is a very fair representation for moderate viscosities, but for large ones it is so far from correct that the tendency for the obliquity to vary may become nil, and for yet larger ones the obliquity may tend to decrease.

[1] It is here assumed that fe is at right angles to C D, and A B at right angles to fe. There is no necessity for this condition, and it could never actually occur.

[2] A paper read at the Dublin Meeting of the British Association, by G. H. Darwin, M.A., Fellow of Trinity College, Cambridge.

A complete analysis of this state of things for various obliquities and viscosities shows that there is a great variety of positions of dynamical equilibrium, some of which are stable and some unstable.

Although there is all this variety with respect to the change of the obliquity, yet the tidal friction always tends one way, namely, to stop the earth's rotation.

It has already been remarked in the general explanation that the effect on the moon is a force tangential to her orbit accelerating her linear motion, and thus indirectly retarding her angular motion. But it appears that for a very great degree of stiffness and for large inclinations of the earth's axis to the ecliptic, this force on the moon may be actually reversed; so that the retardation of the moon's motion may actually be replaced by an acceleration.

To a terrestrial observer, however, unconscious of the slackening of the earth's diurnal rotation, it would be indifferent whether the moon were undergoing true retardation or true acceleration, for in every case there would result an apparent acceleration of the moon's mean motion.

It is obvious from what has been said that we have the means of connecting the heights and lagging of the bodily tides in the earth with an apparent secular acceleration of the moon's mean motion. I have applied these ideas to the supposition that the moon has an apparent secular acceleration of 4″ per century, and I find that if the earth were a homogeneous viscous spheroid, then the moon must be undergoing a secular *retardation* of 3″·6 per century, while the earth (considered as a clock) must be losing 14 seconds in the same time. Under these circumstances the effective rigidity of the earth must be so great that the bodily diurnal and semi-diurnal tides would be quite insensible; the *bodily* fortnightly tide would, however, be so considerable that the *oceanic* fortnightly tide would be reduced to one-seventh of its theoretical amount on a rigid nucleus, and the time of high water would be accelerated by three days.

The supposition that the earth is a nearly perfectly elastic body leads to very different results, which, however, I must now pass over.

From this and various other considerations, I arrive at the conclusion that the earth has a very great effective rigidity, and that the apparent acceleration of the moon's motion affords no datum for determining the amount of tidal friction on the earth.

Sir William Thomson has made some interesting remarks about the probable age of the earth in connection with tidal friction, and he derived his estimate of the rate at which the diurnal rotation is slackening principally from the secular acceleration of the moon. He fully admitted that his data did not admit of precise results, but if I am correct in the present conclusion, it certainly appears that his argument must lose part of its force.

The investigation of the secular changes, which such a system would undergo, is surrounded by great mathematical difficulties, but I think that I have succeeded in surmounting them by methods partly analytical and partly arithmetical.

In a communication of the present kind it would be out of place to consider the methods employed, and I will therefore only speak of some of the results.

There are two standards by which we may judge of the viscosity in the present problem—first the ordinary one, in which it is asserted that it requires so many pounds of tangential stress to the square inch to shear an inch cube through so much in such and such a time; and secondly, when the viscosity is judged of by the amount by which the behaviour of the spheroid departs from that of a perfectly fluid one; a numerical value for this sort of measure is afforded by the angle by which the crest of the tidal spheroid precedes the moon, when the obliquity to the ecliptic is zero.

Now it appears that if the earth possessed a viscosity which was not at all great as estimated by the tidal standard, yet the materials of the earth, when considered in comparison with the substances which we know, would be found to be a substance of very great stiffness—stiffer than lead, and perhaps nearly as stiff as iron. I see, therefore, no adequate reason why some part of the changes, which will be considered presently, should not have taken place during geological history.

The problem was solved numerically for a degree of viscosity, which would make the changes proceed with nearly a maximum rapidity. Estimated by the tidal standard, this is neither a very great nor a very small viscosity, for the crest of the semi-diurnal tide precedes the moon by 17° 30′.

I found, then, that if the changes in the system are tracked back for fifty-six million years, we find the day reduced to six hours fifty minutes, the obliquity to the ecliptic 9° less than at present, and the moon's period round the earth reduced to one day fourteen hours.

This very short period for the moon indicates of course that her distance from the earth is small. As the moon goes on approaching the earth the problem becomes much more complex, and, for periods more remote than fifty-six million years ago, I abandoned the attempt to obtain a scale of times. The solution up to this point shows that the times requisite for these causes to produce such startling effects are well within the time which physicists have admitted to have elapsed since the earth existed.

From this point in the solution the parallel changes of the obliquity, day and month, were traced without reference to time.

It appears, then (still looking backwards in time), that the obliquity will only continue to diminish a little more beyond the point already reached; for, when the sidereal month has become equal to twice the day, there is no longer any tendency for the obliquity to diminish, and for yet smaller values of the month the tendency is to increase again.

From this we learn that, when the day is equal to or greater than half the month, the position of the earth's axis at right angles to the plane of the moon's orbit is one of dynamical stability. The whole decrease of obliquity from the present value back to the critical point, where the month is equal to twice the day, is 10°. From this point in the solution back to the initial state to which the earth and moon are tending, the obliquity to the plane of the lunar orbit was neglected. I then found that the limiting condition, beyond which it was impossible to go, was one in which the earth and moon are rotating, fixed together as a rigid body, in five hours and forty minutes. This condition was also found to be one of dynamical instability, so that, if the month had been a little shorter than the day, the moon must have fallen into the earth, but if the month had been a little longer than the day the moon must have receded from the earth, and have gone through the series of changes, which were traced backwards up to this initial condition.

This periodic time of the moon of five hours forty minutes corresponds to an interval of only 6,000 miles between the moon's centre and the earth's surface. Moreover, if the earth had been treated as heterogeneous instead of homogeneous, this interval between the primeval earth and moon would have been yet further diminished, as also would be the common periodic time.

The conclusion, therefore, appears to me almost irresistible that if the moon and earth were ever molten viscous bodies, then they once formed parts of a common mass.

With respect to the obliquity of the ecliptic, the question is one of considerable difficulty, but, on the whole, I incline to the view that, while a large part of the obliquity may be probably referred to these causes, yet that there remains an outstanding part which is not so explicable.

Besides the results, of which the outlines have been given, I have obtained some others which, as I believe, will aid in the formation of a modified edition of the nebular hypothesis—such as some of the changes to which an annular satellite would be subjected.

One of the collateral results, which appeared in considering the secular changes of such a system as the earth, moon, and sun, was that a large amount of heat would have been generated in the interior of the earth by means of friction. If, then, it is permissible to suppose that any considerable part of these changes had taken place during geological history, Sir William Thomson's problem of the secular cooling of the earth would require some modification.

The magnitude of the undertaking has not allowed me time as yet to apply these ideas to the questions of the eccentricity and inclination of the orbit of the satellite, nor to the cases of other planets besides the earth.

I think, however, that I see in Asaph Hall's wonderful discovery of the Martian satellites, a confirmation of this theory. Their extreme minuteness has, I think, preserved them as a standing memorial of the primitive period of rotation of that planet. The Uranian system, on the other hand, appears, at least at first sight, a stumbling-block.

It is easy to discern in the planetary system many *veræ causæ* which tend to change its configuration, but it is in general very hard to give any quantitative estimate of their effects.

It will have been seen that in the investigation of which I have given an imperfect account, free scope has been given to speculation, but that speculation has been governed and directed in every case by appeal to the numerical results of a dynamical problem, and I therefore submit that it stands on a different footing from the numerous general speculations to which the nebular hypothesis has given rise.

NATURAL SCIENCE IN HUNGARY IN THE LAST TEN YEARS

FEW of the readers of NATURE are aware that Hungary has of late years become the scene of active efforts in science, and especially the natural sciences.

The following sketch of an article, written by Mr. Coloman Szily, member of the Academy of Sciences, and Professor of Physics at the Polytechnic of Budapest, and published in the *Budapesti Szemli* (*Budapest Review*), may therefore not be altogether without interest.

The first active sign of native scientific life in this direction in Hungary was the founding of the Academy of Sciences in 1830. Up to that time there were single men of science, but no organised scientific life. But the chief object aimed at by the Academy was the cultivation of the national language, and the excessive zeal with which it pursued this aim did much harm to the cause of the natural sciences here. An erroneous attempt to substitute purely Hungarian words for the mathematical and other scientific expressions universally accepted elsewhere, threw great obstacles in the way of the progress of the natural sciences in our country.

This and other errors soon brought a reaction.

The "General Assembly of Physicians and Naturalists" was soon started amid general enthusiasm. The meetings of the assembly were held yearly in different cities from 1841 up to 1848, and then renewed in 1863, after a cessation of fifteen years, caused by political events. A yearly report was issued, containing the various papers read at the meeting, as well as an account of physical characteristics of the district in which it was held.

Far more brilliant was the success reached in the cultivation and promulgation of the natural sciences by the "Természettudományi Társulat" (Society of Natural Sciences), which was started at the same time. By 1848 the number of the members rose to more than 400. Its first yearly report was then issued, and a contract made for the starting of a scientific magazine, entitled "Magyar Iris." This powerful start, which was made independently of the Academy, and which proved of ever-increasing importance, could not remain without effect. In 1844 a proposition that the two classes of mathematicians and naturalists might hold their meetings and carry on their financial and other operations separately from the rest was partially accepted. Some years later (1861) it only required a single lecture (of Prof. Joseph Izabo's) to bring the whole Academy to pronounce a resolution against the attempt to Magyarise the nomenclature.

In the meantime the events of 1848-49 were followed by a long period of despotism, which tended to paralyze all attempts at association. The most distinguished men of science were forced off the field of action, the Academy could hold no meetings, the Society of Natural Sciences was on the brink of dissolution, its members were scattered, its collections had to be given away for lack of funds to pay the rent of the accommodation needed for them, and it was barely able within ten years to publish two of its yearly reports.

Between 1850 and 1860 the nation began to breathe more freely. Its very first efforts were turned towards the advancement of science. A very fine building was raised for the Academy, and its capital considerably increased, by means of private subscriptions. It thus became able to do much more than it did previously, both for the improvement of our native language and for the cultivation of the various branches of science. In 1860 it appointed a committee of mathematicians and naturalists, whose duty it was to explore the whole country and give an account of its natural and technical features. Ever since the year 1868 Government has devoted the yearly sum of 5,000 florins to the furtherance of the labours of this committee. But this sum frequently proves insignificant. Fourteen volumes of the publications of the committee, entitled, "Scientific Treatises Relating to Home Topics" (Termiszettudományi közleménysk, vonatkozólag a hazai viszonyokra), edited by its secretary, Prof. Joseph Szabo, have already appeared. At the same time with this the Academy started a second series of periodicals for publishing mathematical and scientific treatises, not confined to topics within the limits of our country. They appeared yearly from 1860 to 1867, six volumes in all.

After the renewal of constitutional life in 1867 our naturalists were also filled with a strikingly-increased zeal for labour. The Academy has up to the present day issued thirty-two volumes in all. Many articles, treating of the original researches of our naturalists, have appeared in foreign periodicals. The meetings of the department of natural sciences in the Academy have of late borne witness to a truly diligent and scientific spirit, there scarcely being one in which less than six or eight treatises have been presented upon topics of original research.

In 1868, though no preliminary agreement had taken place between the two institutions, the department of Naturalists of the Academy, having arrived at the conviction that the popularising of the natural sciences was not their calling, abandoned the attempt, and decided that they should henceforth direct their efforts solely to the cultivation of the sciences and the making of scientific researches in our country, while the Society of Natural Sciences took upon itself the spreading and popularising of them. To this end the Society started a monthly periodical; the number of members of the Society rising in the very first year of its existence from 600 to 1,600, the second year to 2,200, until at present it borders upon 4,800. A short time later the Society began to arrange lectures connected with experiments for the benefit of the public. These lectures have now been kept up for eight years, and the large lecture-hall in which they have been held has always been crowded with hearers. As long as it was possible these lectures were published in the *Journal of Natural Sciences*; now, however, they appear in the form of a new series of publications under the title, "Collection of Popular Treatises upon Topics pertaining to the Natural Sciences." In 1872 the Society again started a new undertaking, namely, the translating into Hungarian and issuing of foreign works of a popular kind upon the natural sciences. The result of this undertaking, which has enjoyed the support of 1,500 subscribers, as well as a yearly aid from the Academy, has up to the present time been the issuing of twelve volumes, such as "Geologie der Gegenwart," by von Cotta; Darwin's "Origin of Species," Helmholtz's "Populäre Vorlesungen," Huxley's "Lessons in Physiology," Lubbock's "Prehistoric Times," Proctor's "Other Worlds than Ours," and Tyndall's "Heat as Motion." An Anthology has also been compiled, containing a treatise from every scientific author who has contributed to the popularising of the natural sciences, from the time of Arago and Humboldt downwards, and a volume containing the complete works of the late Julius Greguss. These books always find a large number of purchasers.

But there is another branch of activity which is of more importance, perhaps, than all those, namely, its efforts for the encouragement of original research. From 1870 the legislature of our country, in appreciation of the labours of the Society, has voted a yearly sum at first of 5,000, and afterwards of 4,000 florins, for the promotion of such researches as stand most nearly connected with the interest of our country, and the publication of an account of the same. In this series of publications the following have appeared up to the present time:—"The Rise and Fall of Tide in Fiume Bay," by Emile Stahlberger; "The Ice Grotto of Dobsina," by Dr. Joseph Alexander Krenner; "Sketch of the Ligaridas of Hungary," by Dr. Géza Horváth; "The Spiders of Hungary: Vol. I. General Part," by Otto Hermann; "The Iron Ores and Iron Products of Hungary, with Special Reference to the Principal Chemical and Physical Qualities of the Iron," by Anton Kerpsly.

In searching out and making known the physical characteristics of our country, the Society of Geologists (Magyar földtani társulat), founded in 1851, can also boast considerable merits. The Society also publishes a monthly periodical under the title of *Geological Review*.

In 1872 there was also a Geographical Society founded, which did not aim so much at the advancement of geographical researches as at keeping the public informed of any progress made on this field by means of a two-monthly review.

This active interest in the natural sciences is not confined only to our capital, but has taken root throughout the country. To prove this we have but to note the interest manifested in the labours of the Society of Natural Sciences in all grades of

society everywhere, and the fact that societies under various titles, but all aiming at the cultivation of the natural sciences, were formed in many of our larger towns as early as between 1850–60, and more still of late years.

There is another circumstance worthy of notice, which affords perhaps a clearer illustration of the general interest in the natural sciences, and diligence on the field of the same, than even the rise and progress of the various societies already mentioned. It is the constant increase in the number of scientific periodicals, of which the following are already in circulation, besides the publications of the academy and the three societies of which we have spoken :—

(1) *Nature* (*Természet*), which appears every two weeks, and is now in the tenth year of its existence; (2) *Polytechnical Journal* (*Müessetemi lapok*), a monthly periodical for mathematical and technical treatises; (3) *Reports of the Meetings of the Society of Physicians and Naturalists in Klausenburg*, started in 1876; (4) *Botanical Papers*, a monthly, started in 1877; (5) *Leaves of Natural History*, a quarterly periodical, started in 1877, and edited by the Hungary National Museum. At the end of each volume is a review of the contents, written either in French or German, in order to be understood in foreign countries, and containing either a complete translation or an abstract of all the more important articles in the volume; (6) *Magazine of Natural Sciences*, a two-monthly periodical, started in 1877, and edited in Temesvár by the Association of Naturalists of South Hungary.

To recapitulate briefly what has been said, there are in Budapest, besides the department of natural sciences in the Academy, three societies, the object of which is exclusively the cultivation of the natural sciences, and one of which is of dimensions that, considering the total number of inhabitants, are scarcely equalled anywhere in Europe. In other parts of our country we have in all six societies of naturalists, and there are in circulation nine scientific periodicals, not one of which enjoys any aid from the State, all being supported exclusively by the readers.

These societies, however, are not the sources of science, but merely, so to speak, its conducting pipes. The sources of it spring from the collections and laboratories of the universities. The progress made here of late years has also been considerable. Our university, which was greatly neglected up to the year 1850, has of late taken such a start forward that its condition of ten years ago is not to be compared to its present state. The number of professors' chairs pertaining to the natural sciences (including those of the University and the Polytechnic of Budapest, and the University of Klausenburg) now amounts to three times what it was previously. The greater part of these chairs are occupied by young professors, who have been educated at foreign universities, under the instruction of the most distinguished men of science. Each one has a respectable sum of money at his disposal for cabinet and laboratory purposes. Separate buildings have been raised and equipped as institutes of natural sciences, such as the Chemical and Physiological Institute, connected with the University, which stands under the direction of Prof. Charles Than, and already enjoys a wide-spread renown in Europe. The buildings intended for a clinic are nearly completed, and soon the Institute for Physics, the Mineralogical and Zoological Institute, and the buildings of the Polytechnic will be raised in their turn. The number of the students of philosophy increases with striking rapidity, and some of the most distinguished of these are yearly sent out to foreign universities at state expense, with a view to their afterwards accepting appointments in their own country.

All these things clearly show that Hungary has within the last ten years made striking progress in the field of natural sciences, so that the distance which separated her in this respect from her western neighbours, has grown palpably less. Would that providence should permit her to continue the work thus begun! J. M. A.

PUBLISHERS' ANNOUNCEMENTS

MESSRS. WILLIAM BLACKWOOD AND SONS announce "The Transvaal of To-day: War, Witchchraft, Sport, and Spoils in South Africa," by Capt. Alfred Aylward, late Commandant, Transvaal Republic. Capt. Aylward commanded the Leydenberg volunteers on the Boers' frontier until the Republic was annexed by the British, and from the prominent part he played in Transvaal politics, as well as from his knowledge of the country and his experience of Kaffir warfare, his book may be expected to throw some light upon questions that are now attracting public attention.

MESSRS. KEGAN PAUL AND CO. announce that they will add to their International Scientific Series in October, a "History of the Growth of the Steam Engine," by R. H Thurston, Professor of Mechanical Engineering in the Stevens Institute of Technology, Brooklyn. The volume will give a history of the discoveries, inventions, and many ingenious experiments that gradually led to the success of the steam engine in the last century, and will be illustrated with fifteen portraits and 148 engravings on wood. This work will be followed speedily by Prof. Huxley's volume on "The Crayfish: an Introduction to the Study of Zoology." Prof. Alexander. Bain's "Education as a Science;" and Dr. H. Charlton Bastian's treatise on "The Brain as an Organ of Mind." Two translations from the French will also be included in the series before Christmas, namely, "The Human Race," by Prof. A. de Quatrefages, and "The Brain and its Functions," by Dr. J. Luys. The same publishers announce "The Geology of Ireland," by G. Henry Kinahan, M.R.I.A., &c., of Her Majesty's Geological Survey; with numerous illustrations and a geological map of Ireland; to be ready in October. "Etna; a History of the Mountain and its Eruptions," with maps and illustrations, by G. F. Rodwell, F.R.A.S., F.C.S. "Flowers and their Unbidden Guests," by Dr. A. Kerner, Professor of Botany in the University of Innsbruck; translation edited by W. Ogle, M.A., M.B.; with illustrations. "Mind in the Lower Animals in Health and Disease," by W. Lunder Lindsay, M.D., F.R.S.E., F.L.S., Hon. Member of the New Zealand Institute; Vol. I. Mind in Health; Vol. II. Mind in Disease. "History of the Evolution of Man," by Prof. Ernst Haeckel, Author of "The History of Creation;" two vols., with numerous illustrations. "Gaur: its Ruins and Inscriptions," by the Late John Henry Ravenshaw, B.C.S.; edited by his Widow; with forty photographic illustrations, and fourteen facsimiles of inscriptions. These three last books will be ready for publication in November.

MESSRS. REEVE AND CO. have just published the fourth edition of Bentham's "Handbook of the British Flora." The principal alteration in this edition has been the giving the first place to the Latin names of the genera and species. The attempt made in previous editions to establish an English scientific nomenclature, in imitation of the French and German ones introduced into several standard Continental floras, has, we regret to say, proved a failure.

MESSRS. MACMILLAN AND CO. will publish, in the course of the season, the following new books and new editions:—"Sport and Work on the Nepaul Frontier, or Twelve Years' Sporting Reminiscences of an Indigo Planter," by "Maori," with Map and Illustrations; "Coal, its History and its Uses," by Profs. Green, Miall, Thorpe, Rücker, and Marshall, of the Yorkshire College, Leeds, with numerous Illustrations, 8vo; "Chemistry: a Treatise on," by Profs. Roscoe and Schorlemmer, of the Owens College, Manchester, vol. ii., "Metals," part 1, 8vo; "Gegenbaur's Comparative Anatomy," a Translation revised with Preface by Prof. E. Ray Lankester, F.R.S., with numerous Illustrations, medium 8vo; "A System of Medicine," Edited by J. Russell Reynolds, M.D., F.R.S., vol. v., completing the work; "Modern Realism Examined," by the late Prof. Herbert, Edited by Prof. James M. Hodgson; "Science Lectures at South Kensington," vol. ii., crown 8vo; "The Theory of Sound," by Lord Rayleigh, M.A., F.R.S., vol. iii., 8vo; "Heat," an Elementary Treatise, by P. G. Tait, Professor of Natural Philosophy at Edinburgh, with numerous illustrations, crown 8vo; "Sound" (NATURE series): a series of simple, entertaining, and inexpensive experiments in the phenomena of sound, for the use of students of every age, by Alfred Marshall Mayer, Professor of Physics in the Stevens Institute of Technology, &c., &c.; "Ismailïa" by Sir Samuel W. Baker, Pasha, a narrative of the expedition to Central Africa for the suppression of the slave trade, organised by Ismail, Khedive of Egypt, with maps, portraits, and numerous illustrations by Zwecker and Durand, new and cheaper edition, one vol., crown 8vo; "A Ramble Round the World," by M. le Baron de Hübner, formerly Ambassador and Minister, translated by Lady Herbert, new and cheaper edition with numerous illustrations, crown 8vo.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

It has been determined to give a course of lectures on Agricultural Chemistry at the High School, Kelso, and the first of the series, at which Sir Geo. Douglas presided, was delivered on the 19th inst. by Mr. Wm. Ackroyd, from the Science Schools, South Kensington.

Prof. Marignac, after having filled for a period of thirty-eight years the Chair of Chemistry at the University of Geneva, with the greatest distinction, has recently resigned. He has been replaced in the chair by Prof. Graebe, from the University of Königsberg.

SCIENTIFIC SERIALS

Bulletin de l'Académie Royale de Belgique, No. 6, 1878.—In this number M. Spring communicates some interesting preliminary results obtained from enormous pressure exerted on the fine powder of some solid bodies (nitrates of sodium and potassium, sawdust, chalk, &c.). Homogeneous blocks were thus produced, harder and more resistant than if they had been obtained by fusion. Two of them were found translucid, and did not present the least vestige of the particles which were united to form them. A force of 40,000 atmospheres was employed, but of this—the friction of the apparatus being enormous—only about half was disposable pressure.—Some experiments with regard to the action of atropine and physostigmine on the heart of the frog, and in connection with the physiology of the vagus nerve, are described by MM. Putzeys and Swaen.—From a careful examination of some Chilopodan Myriapoda, especially several large Scolopendra from Java, M. Jules M'Leod has succeeded in detecting the true venomous glands in the substance of the forcipular foot-jaws. The structure of these organs is detailed.—M. De Heen has sought to determine the viscosity of liquids from the retardation of a small spheroidal glass runner allowed to descend in a tube of 3 m. height, filled with different liquids. He deduces numerical values, which he considers to represent the *coefficients of fluidity* of the liquid. Objection is taken, however, to his formula by MM. Valerius and Montigny, who report on the paper.

The *Zeitschrift der oesterreichischen Gesellschaft für Meteorologie* (vol. xiii., Nos. 19 and 20) contains the following more important papers:—On the conditions of temperature in the United States, by Dr. A. Woeikoff.—Appeal to meteorologists to make observations of clouds, by Dr. H. Hildebrandsson.—On a new balance thermograph, by Dr. A. Sprung.—On the estimation of ozone, by Albert Lewy.—On the origin of atmospheric ozone, by Dr. Leuder.—On the climate of Queensland, by Herr Hann.—On the local winds of the Hiberian peninsula, by Herr Hellmann.

Kosmos, August.—The inhabitants of the planets, by C. du Prel.—Harvey on generation: a study, by W. Preyer.—Insects as unconscious florists, part 2, by Hermann Müller; with wood-cuts illustrating potentilla, ranunculus, lychnis, daphne, crocus, &c.—On the struggle of languages in the Valais, by Alexander Maurer.

Zeitschrift für wissenschaftliche Zoologie, vol. xxxi. part 1.—On the Siphonophora (Hydrozoa) of deep water, with descriptions of new species of Rhizophyda and Bathyphysa, by Th. Studer; three plates.—Contribution to the morphology of Oxytrichidæ, by V. Sterki; one plate.—On *Trichaster elegans*, by H. Ludwig.—On Loxosema, by O. Schmidt.—On the Tomopteridæ, by F. Vejdovsky; two plates.—Contribution on the Caprellæ, by A. Gamroth; three plates.

Morphologisches Jahrbuch, vol. iv. part 2.—Contributions on the formation, fertilisation, and segmentation of the animal ovum, by Oscar Hertwig, part 3, continuing and confirming his previous researches; three plates, with figures of ova of Nausithoë, Physophora, Helix, Unio, Tellina, Sagitta, and many others.—On fossil vertebræ: the Cestracionts, by C. Hasse; fifty-five pages, three plates; one containing coloured diagrams of vertebræ to illustrate a scheme of the evolution of fishes.—On *Gorgonia verrucosa*, by G. von Koch.—On the retrogression of eyes in Arachnida, by A. Stecker; one plate, illustrating Chernes and Chelifer.—On the osteology of the gorilla, by C. Aeby; with five wood-cuts.

SOCIETIES AND ACADEMIES

London

Entomological Society, September 4.—Fred. Smith, vice-president, in the chair.—Mr. David Price of Horsham, Sussex, was elected as a Member, and Capt. Thos. Broun, of Auckland, New Zealand, as a Subscriber.—Mr. Rutherford exhibited two specimens of an orthopterous insect *Palophus centaurus*, West, from Old Calabar.—Mr. F. Smith exhibited a specimen of the fruit of the so-called "locust-tree" (*Hymenea conbaril*), from British Guiana, forwarded to Dr. Sharp from Mr. Harper. The fruit on being opened had been found to contain three living specimens of a weevil (*Cryptorhynchus stigma*, Linn.), a cocoon containing the chrysalis of a moth, together with the remains of one or more such cocoons, and lastly, a small parasitic hymenopteron (an Ichneumon allied to *Chelonus*). Mr. Smith also exhibited a specimen of *Melolontha vulgaris*, which had lately been found alive under turf at the bottom of a box in which the larva had been placed last April, thus making it appear probable that the insect assumes its perfect state underground a long time before making its actual appearance.—Mr. Champion exhibited a series of *Spercheus emarginatus*, taken at West Ham, Essex.—Mr. Jno. Spiller exhibited some so-called "jumping-seeds," received from Mexico, and contributed remarks thereon.—The secretary exhibited a photograph of a fossil butterfly, *Prodryas persephone*, Scudd., received from Mr. Scudder. The insect represented was in an excellent state of preservation, and had been found in the tertiary formation of Colorado.—Mr. Smith stated that having recently had occasion to refer to the Linnean collection in the apartments of the Linnean Society, he regretted to find it had been allowed to fall into a state of complete neglect.—Mr. Swinton communicated a paper on the vocal and instrumental music of insects.—Mr. Waterhouse read a paper entitled notice of a small collection of coleoptera from Jamaica, with descriptions of new species from the West Indies.

Philadelphia

Academy of Natural Sciences, April 30.—On the bridging convolutions in primates, by A. J. Parker.

May 14.—Elements of the sidereal system, by Jacob Ennis.—Descriptions of new species of North American bees, by E. T. Cresson.

May 28.—Transition forms in crinoids, and description of five new species, by C. Wachsmuth and F. Springer.

June 4.—The law governing sex, by T. Meehan.

July 9.—On pelagic amphipods, by T. H. Streets.

CONTENTS

THURSDAY, OCTOBER 3, 1878

DOBSON'S CATALOGUE OF BATS

Catalogue of the Chiroptera in the Collection of the British Museum. By George Edward Dobson, M.A., M.B. London. (Printed by Order of the Trustees. 1 vol., 8vo. 1878.)

THOUGH Mr. Dobson's work is modestly termed a "Catalogue," it amounts, in fact, to a complete monographic essay on what has hitherto been justly regarded as the most difficult and the least understood, as it is the most numerous, of all the orders of mammals. Not only are the families and genera of the Chiroptera well characterised in this volume, and all the known species described in concise though sufficiently explicit terms, but synopses of the members of each genus are added in order to facilitate their determination, and excellent notes on the comparative anatomy, habits, distribution, and position of these animals are given, whenever such information is available. Mr. Dobson, it is true, has had unusual opportunities in dealing with this subject, but it is not the less to his credit that he has taken such good advantage of them. Having commenced his studies upon the bats during his official residence in India, he has been able to make himself personally acquainted with the forms inhabiting that country, and likewise to examine the types of Blyth's descriptions in the Indian Museum at Calcutta, without sight of which it would have been impossible to recognise what was intended by them. In this country the late Dr. Gray turned his attention at various times to the Chiroptera, and besides describing numerous species even more hastily and imperfectly than Mr. Blyth, indulged himself in the evil practice of altering some of the types upon which he had found his genera and species. Having the national collection at his command, Mr. Dobson has been able to reduce all these eccentricities into order. On the continent the accomplished zoologist of Berlin, Dr. W. Peters, is almost the only naturalist who has of late years worked at this perplexing group of mammals. Dr. Peters has published many excellent memoirs on various genera of bats in the *Monatsberichte* and *Denkschriften* of the Berlin Academy, in the course of which he has given us an account of his examination of numerous obscure types of the older author. Mr. Dobson has worked up the results of these memoirs into his monograph, and has at the same time had the advantage of examining in the Museum of Berlin the materials upon which Dr. Peters has based many of his conclusions. Mr. Dobson has also visited the great museums of Leyden and Paris, and has studied the specimens described by Temminck, Geoffroy St. Hilaire, and A. Milne-Edwards, belonging to these collections. In fact he has had under his eyes nearly all the available materials for a study of the group, unless it be the specimens collected by Natterer in the Imperial Zoological Cabinet at Vienna, and a few types lately described by Mr. Allen in America. Under these circumstances Mr. Dobson's so-called "Catalogue" has, as we have already remarked, become a monograph of a very high order of merit, and one which reflects the greatest credit upon the talents and the industry of the author.

Mr. Dobson recognises 400 species of the order Chiroptera, which he divides primarily into "Mega-chiroptera" and "Micro-chiroptera." We do not quite see the advantage of these newly-coined names over the more usually employed terms of "Frugivora" and "Insectivora," even if they had been classically compounded, which, unfortunately, is not quite the case. Certain it is that the new terms are not more absolutely true than the older ones, as some of the smaller "Mega-chiroptera" are inferior in size to the larger "Micro-chiroptera." About seventy species of the Frugivorous bats are allowed full rank in the present work, the author, who holds very sound views on the true limits of species, having reduced to the grade of sub-species some of the forms recognised by previous writers. The more numerous "Micro-chiroptera" constitute five families, namely the Rhinolophidæ, Nycteridæ, Vespertilionidæ, Emballonuridæ, and Phyllostomidæ, according to Mr. Dobson. This arrangement is not very different from that of Dr. Peters in 1865. Dr. Peters' "Megadermata" do not quite correspond to Mr. Dobson's "Nycteridæ," and Mr. Dobson puts the "Brachyura" and "Molossi" of Dr. Peters together in his family "Emballonuridæ." There are other minor differences, but it is satisfactory to see that these two great authorities on an obscure group of mammals are so nearly agreed as to their general arrangement.

Mr. Dobson has not made any suggestion as to the special use of the extraordinary adhesive discs which are attached to the inferior surface of the thumbs of *Thyroptera tricolor*, except by designating them as "highly specialised climbing organs." This they certainly are, but to climb what are they highly specialised? We have been told by one who has observed them in their native haunts that these abnormal bats are always found adhering to the smooth stems of certain palm-trees on the upper Amazons, and it is for this purpose, no doubt, that these peculiar suckers are provided. As regards the vexed question of the true blood-sucking bats, Mr. Dobson seems to adhere to the now generally-received opinion that the only certainly-known sanguivorous species are the two Desmodontes (*Desmodus rufus* and *Diphylla ecaudata*), in which the alimentary canal is specially modified in relation to their peculiar diet. *Vampyrus spectrum*, formerly considered so formidable a blood-sucker, is probably purely frugivorous, and older writers have made similar accusations against certain species of *Glossophaga* and *Artibeus*. But *Desmodus* is the only culprit that has been actually "caught in the act," and naturalists are always somewhat incredulous in cases of merely circumstantial evidence. As regards the peculiar structure of the stomach of the Desmodontes it is not quite true, as stated by Mr. Dobson, that no other zoologists but Prof. Huxley and Dr. Peters "seem to have been aware of this remarkable departure from the simple type of that organ found in all other Micro-chiroptera." As Herr v. Petzeln. has lately told us,[1] this peculiarity did not escape the vigilant eyes of the late Johann Natterer, who recorded it in his note-book in 1824, although his remarks were never published, and, if we are not mistaken, Prof. Reinhardt has also noticed it.

Mr. Dobson's letter-press is illustrated by thirty litho-

[1] Sitz. k.k. zoolog.-botan. Ges. in Wien, vol. xxviii.

graphic plates drawn under his superintendence by R. Mintern. Although not so finished in execution as those of the late Mr. Ford, or quite so clear in detail as what Franz Wagner has done for Dr. Peters in the same style, these plates form a great addition to the volume, and exhibit some of the special structures of the group in a very efficient way.

Of the general merits of the Zoological Catalogues of the British Museum, and of the credit due to the staff of the Zoological Department for their preparation in the face of many difficulties, we have spoken in a previous notice of one of this same series.[1] It is much to be regretted, however, that more pains are not taken to make the existence of these most valuable publications known to the world. No publisher's name being on the title-page, it is difficult for the general public to know how to procure them, and no information on the subject is given in the volumes themselves. So far as we know, they are not advertised in any way, and no copies are sent out for review—certainly not to the office of NATURE;[2] so that it is only by chance that one becomes aware of their issue. On the Continent there are many complaints about the difficulty of procuring copies, and naturalists in London receive frequent applications from their brethren abroad on this subject. This might be all remedied by putting a publisher's name on the title-page—a course adopted by all our principal scientific societies for their publications—or even by adding to each volume a list of the series, with some directions as to how and where they are to be obtained. Another mystery connected with these catalogues which we have never been able to understand is why the authors of them should not be allowed to write their own prefaces. In some of the older volumes even the author's name is not given on the title-page. This privilege has been conceded of late years, but the prefaces continue to be written by the "keeper of the department." We are told this is a "regulation of the trustees"—an answer given, we may observe, about many other rules and regulations at the British Museum, of which no one can understand the utility.

TIDY'S "HANDBOOK OF CHEMISTRY"

Handbook of Modern Chemistry, Inorganic and Organic, for the Use of Students. By Charles Meymott Tidy, M.B., F.C.S. (London: J. and A. Churchill.)

THIS work is divided, as is usually the case with chemical text-books, into three large divisions; the first containing the chemistry of non-metallic bodies, the second the chemistry of the metals, and the third the chemistry of organic substances.

In the first two chapters, preliminary to those discussing systematically the natural occurrence, preparation, and properties of the non-metals, the author describes the more general principles involved in the science, embracing such topics as nomenclature, atomic and molecular combination, combination by volume, atomicity and quantivalence, &c. The subjects here touched upon are clearly dealt with, and Mr. Tidy's style of writing cannot fail to attract the attention of the reader.

The subsequent portion of the book, consisting of Chapters III. to IX., embraces the consideration of the individual properties of the different non-metallic elements; and although this part of the work abounds in valuable and, as far as we can see, accurate information, there is an important point with regard to it upon which we cannot thoroughly congratulate the author; namely, the order in which he has arranged the non-metallic elements. He commences with oxygen and finishes with hydrogen. We cannot at this moment see, nor can we find any explanation in the preface or otherwise, stating why this particular order should be adopted; and we are inclined to think that, for the sake of instruction in chemical order and classification, it indicates a defective appreciation of the wants of the student. It appears to us that, keeping this point in view, the order of such a text-book as Mr. Tidy's should be that in which the elements forming the least complex compounds are first taken, then those which possess a larger number of compounds and of a more complicated nature.

This becomes evident if it be considered what the student is met by in reading this portion of the book, where, instead of first being made acquainted with the properties of hydrogen, the body now almost universally adopted as our standard of reference for the atomic weights of the elements, for the densities of gases, &c., he has to consider oxygen "a common and important substance, certainly," but not one which is now taken as our standard, or which forms the simplest combinations, so far as its own relations are concerned, with other bodies. The reader then passes, in the next chapter, to the consideration of the group of elements, consisting of fluorine, chlorine, bromine, and iodine; bodies having a simpler volume relation to hydrogen than oxygen has: and he is then introduced to a long series of compounds, the oxyacids of the halogen series, where three elements take part in the combination before he has become acquainted with compounds containing only two, such as hydrochloric acid or water. Nay, more, he has to consider acid bodies containing hydrogen—reads equation after equation in which the body water occurs as a product of decompositions, without his previously having learned anything either about the preparation and properties of hydrogen, or the composition of water.

Again, in the arrangement of the subjects treated of in the chapter dealing with the special consideration of hydrogen (Chap. IX.) we think there is room for improvement; and that it would be better to adopt an arrangement depending on the simplest volume relations of the substances, placing them in the series monatomic, diatomic, triatomic, and so on, instead of first taking water where the ratio of hydrogen to the other substance is 2 : 1, then the halogen compounds of hydrogen where the ratio is more simple, viz., 1 : 1, then its compounds with nitrogen, phosphorus, and arsenic, where the ratio is 3 : 1, then back again to sulphuretted hydrogen, where the ratio is 2 : 1, and finally, to the simple compound of carbon and hydrogen with the ratio 4 to 1. If the student is at all thoughtful, and has paid any attention to the sections on "combination by volume," &c., which we have already stated to be clearly written, he will find himself at the end of the consideration of the non-metals rather at a loss to form any idea of order or classification as regards chemical bodies.

[1] See our review of Sharpe's "Catalogue of Birds," NATURE, vol. xvi. p. 541.

[2] Since the above was in type a copy of Dobson's "Catalogue" has been sent us by the author.—ED.

In the next portion of the work, comprehending Chapters X. to XVIII., devoted to the description of the metals, Mr. Tidy has introduced some very useful tables, in which he gives, under the more important metals, lists of their salts, with the different acids, as well as the common and constitutional formulæ of the compound, and in many cases the molecular weight, specific gravity, and percentage of metal in the salt. This tabular form has not usually been systematically adopted in text-books, and has many advantages, more especially when the work is used for consultation.

In connection with this part, however, which embraces also the general consideration of acids, bases, and salts, it is to be regretted that the definition of an acid comes so late in the volume as p. 252; we should have expected to have found some description at least of an acid a little earlier. With respect also as to what "an acid is," the index of the work is misleading; thus, for instance, on consulting it with regard to the above point we are referred to p. 527, where we find the definition of an organic acid; the term acid in its ordinary sense being only found under the term salt. It may be urged that an acid is a salt of hydrogen, but it is rather too much to expect one who may be reading a chemical text-book for the first time to know all this, more especially as he meets with no explanation of the matter as far as we have found till p. 252.

The last part of the work, comprising Chapters XIX. to XXXI., is occupied with the consideration of the organic division of the science. We regret that space does not permit us to enter very fully into a review of this portion of the book, but as far as we can judge the information contained in it is accurate and well arranged. Here we are glad to see, as in the chapters describing the metals, tables giving the names, formulæ, specific gravities, boiling points, &c., of the different substances arranged in their respective groups or series. These tables we have no doubt will prove of very great use to the student. An appendix contains an account of the recent experiments of Pictet on the liquefaction of oxygen, &c., and a description of Mendeleeff's "Law of periodicity of the chemical elements." Great clearness in style is given to the book by the tables just referred to, and to the methodical manner in which Mr. Tidy has arranged the individual consideration of each element under several heads, as "(1) History, (2) Natural History, (3) Preparation, (4) Properties—(α) sensible, (β) physical, and (γ) chemical," and so on; but notwithstanding this arrangement, which adds to the value of the book, we regret the order in which the author has placed the non-metallic elements, which we cannot help regarding as defective in the case of a text-book designed for the use of students.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

On the Proposed Observatory on the Summit of Mount Etna

Two years ago (September 22, 1876) Prof. Tacchini, of Palermo, read a paper before the Accademia Gioenia of Catania "Sulla Convenienza ed utilità di erigere sull' Etna una Stazione Astronomico-Meteorologica" (NATURE, vol. xv. p. 262). He proposed herein that an observatory should be erected at the Casa Inglesi, which is situated at the foot of the cone of the great crater 9,652 feet above the sea. In it daily observations, both astronomical and meteorological, should be taken during six months of the year, and the telescope should then be removed to Catania and the observations continued.

No more has been heard of this scheme but we sincerely trust that it will not fall to the ground, and that, if need be, our own astronomers will come forward to promote so good a work. No one who has not witnessed a cloudless starlit sky on a perfectly calm night from an elevation of two miles, can realise the difference between it and the same sky seen from the surface of the earth. When I ascended Etna in August, 1877, I was particularly struck by the extraordinary brilliancy of the midnight sky. It was one blaze of brilliant light. Myriads of stars which I had never seen before were visible, and the whole sky was studded with stars of every magnitude, colour, and brightness. The meteors which flashed across the sky were too numerous to count, and the stars themselves shone with extraordinary scintillations. I specially noticed a curious effect for which it is not easy to account, viz., the apparent lowness of the sky. It appeared to be almost pressing down upon one's head, and the larger stars seemed to be suspended below the sky. A good telescope brought to bear upon such a sky would reap a harvest of results. Tacchini noticed that Venus cast shadows, and Sirius appeared to rival Venus.

The observatory on Etna should be constructed on the most sheltered side of the mountain. It might be placed a little to the west of the Torre del Filosofo, the traditional observatory of Empedokles. It could be built of lava collected on the spot, and it would not be difficult to sink the foundations to a depth sufficient to ensure steadiness. It should be telegraphically connected with the observatory at Catania, and barometric and thermometric readings should be taken at the same instant of time at the two stations. It should be provided with a good 8 or 10-inch refractor, the lenses of which could be transported to a duplicate mounting in the observatory of Catania during the winter months. Moreover, it should possess a complete set of self-registering seismological instruments similar to those employed by Palmieri, and now exhibited in the Paris exhibition. Good spectroscopes should be provided, and a set of instruments for magnetic observations.

We are quite confident that considerable results would accrue to many of the sciences if systematic observations were carried out under the proposed conditions which have never yet been attempted, and we trust that astronomers both at home and abroad will not allow the subject to fall to the ground.

G. F. RODWELL

Compound Lightning Flashes

IN NATURE, vol. xviii. p. 67, an instance is given of several flashes of lightning following in the same path, and information concerning similar observations is asked for.

In almost every tropical thunderstorm the phenomena may be seen; to best advantage when the storm is distant. Three, four, and even more discharges may take place, the second and remaining flashes following in rapid succession through the identical path taken by the first. The intervals between the flashes vary; one may follow another so rapidly as to seem merely like a bright pulsation in the first, or there may be an appreciable interval of darkness; but it is certain that, if the eye can be trusted, these secondary flashes follow the exact course of their primary. The reason of this may be looked for in the heating effect of the lightning. The partial vacuum caused by the first discharge offers a line of comparatively small resistance to succeeding currents.

The singular part of the phenomenon is the rapidity with which the electricity must form or collect to admit of several discharges taking place at the same spot, for I do not think the secondary flashes can be regarded as merely residual.

During a severe storm at Mangalore on the 28th of last April two military buildings were struck by lightning, and, from the numerous paths taken by the electricity through the buildings, in its passage to the earth from the points struck, I was led to think the damage might be the result of these *compound* flashes; for it was conceivable that the destruction caused by each discharge might increase the resistance of the path taken, leaving succeeding flashes to follow in fresh directions of

less resistance. New lines might indeed be opened out by fragments of metal, such as nails, &c., scattered by the explosions.

In the case mentioned above, it was difficult, without some such theory, to account for the breaks in the courses followed by the electricity. E. H. PRINGLE

Bath, October 1

Gyno-Diœcious Plants

DURING the past summer I have found the following species in a gyno-diœcious condition, namely, *Ranunculus acris*, *R. repens*, *R. bulbosus*, and *Stachys germanica*, all of which have the corolla considerably reduced in size, and the stamens in *R. acris* and *S. germanica* either absent or reduced to scale-like bodies devoid of pollen. In *R. repens* and *R. bulbosus* the stamens are not so much reduced, but so far as I can judge they produced little or no pollen. The female form in *R. acris* is very common in Lancashire, but I failed to find any on the Lincolnshire coast although I searched carefully for it. In *R. repens* the female form is very rare, having seen only about thirty plants in all. I have also found the *Geum rivale* to be andro-monœcious. So far as I can ascertain these plants have not been noticed in the state described above.

Ashton-under-Lyne THOMAS WHITELEGGE

Wasps Under Chloroform

A FEW days ago a friend told me that she had often placed a bee under chloroform, and that the victims when they found they must die invariably brought their stings to their mouths and sucked the little drop of poison into their mouths. She offered to show me the experiment and endeavoured to catch a bee, but failing to do so she caught a wasp, an insect upon which she had not previously experimented in this way, and we both eagerly watched to see if the wasp would behave as the bees had done under the influence of the narcotic.

The wasp being put under an inverted tumbler in company with a piece of paper saturated with chloroform, in a very few seconds the insect fell on its back and almost immediately afterwards curled up the tail with the sting protruded and a drop of clear fluid on the end of the sting. The sting was brought to the mouth and the drop of fluid disappeared. The wasp then became motionless. After a few seconds the tumbler was removed and the air allowed to play freely on the insect, but no sign of life appeared, except once a slight twitch of the wing. To test whether the insect was really dead my friend placed it in a butterfly cage and left it out of doors all night. Next morning the wasp had disappeared; having perhaps crawled out by a little chink in the cage door.

Can you tell me whether so curious an action of these insects when subjected to chloroform is well known? Does it fulfil any good purpose? Is the poison a narcotic itself and taken by the insect to dull its pains when death seems inevitable? The revival of the wasp appears to show that neither the chloroform nor the poison of its own sting is deadly to the insect. W. M.

Clevedon, September 21

"Mercator" the Geographer

IN Prof. Huxley's "Physiography" it is stated that the real name of "Mercator," of "projection" fame, was Gerard Kauffmann. In a recent number, however, of the popular German journal, the *Gartenlaube*, there is a woodcut of Mercator, taken from an old sketch, under which is the legend—*Gerard Kremer genannt Mercator.* Now, as the word Kremer, or Krämer, means a small retail shopkeeper, the Latin pseudonym is equally applicable, although there is an appreciable difference, meaning excluded, between the two German surnames.

J. C. G.

Our Natural History Collections

IN your recent articles on "Our Natural History Collections," in which you criticised the Act of Parliament just passed authorising the removal of the natural history collections in the British Museum, I was surprised not to find any mention of the third clause, which inaugurates a new and enlightened policy in the disposal of duplicates. The clause was inserted at the instigation of Mr. A. J. Mundella, M.P., and Mr. J. Chamberlain, M.P., and is as follows:—"The trustees of the British Museum may also give away any duplicate works, objects, or specimens not required for the purposes of the museum, provided always that the powers hereby conferred shall not extend to any duplicate works in the royal library of King George IV., or in the Crackerode, Grenville, or Banksean libraries, or to any objects presented to the museum for use or preservation therein."

This important departure from the previous holdfast policy of the British Museum will be hailed with delight by all provincial students of natural history, as will also the paragraph in Prof. Sir C. Wyville Thomson's report to the British Association, referring to the disposal of the *Challenger* collections (*vide* NATURE, vol. xviii. p. 534).

Public museums are springing up all over the country, and any one acquainted with them knows well the difficulty of forming a natural history collection properly suited to educational requirements. If the power now conferred on the trustees of the British Museum is wisely and liberally used, I think as much material will be found stored away as will furnish provincial museums with the specimens required to make them educationally valuable. E. H.

Sheffield, September 26

OUR ASTRONOMICAL COLUMN

BIELA'S COMET.—The great swarm of meteors through which the earth passed on the evening of November 27, 1872, and which were found to be moving in the orbit of Biela's comet, must have been descending to a perihelion passage one month later, or about December 27·6 G.M.T. The comet not having been observed as such since the autumn of 1852, when both parts into which it was separated in 1846 were recovered, we may take this date as a new point of departure, assuming for the present that in following the great assemblage of meteoric bodies seen in November, 1872, we are following what now remains of the comet.

Hubbard's elements of the S. F. nucleus of 1852, with Michez's perturbations by Jupiter and Saturn, give the following elements for 1866, the latest year to which perturbations have been calculated:—

Perihelion Passage, 1866, January 27·6968 G.M.T.

	° ′ ″
Longitude of perihelion	109 39 48
,, ascending node	245 43 42
Inclination	12 22 3
Angle of eccentricity	48 46 19·4
Log. semi-axis major	0·5505333

We know that the comet did not arrive at perihelion at or near the above date in 1866, and hence that disturbance of its motion from an undiscovered cause must have taken place some time in the interval 1852–66. The period of revolution belonging to this orbit is 2445·67 days. There is no reason to suppose that the swarm of meteors is revolving in a shorter period, and we may consequently assume that it will not be again in perihelion before the date, which this period will give, if reckoned from December 27·6, 1872, or September 8, 1879; how much later the perihelion passage may fall it is impossible to foresee. We refer to this point on the present occasion with the view to suggest that a close watch for meteors of the Biela-comet stream should be instituted when the earth again passes the descending node of the comet's, orbit on November 27 next. With perihelion passage on September 8, 1879, the main cometary body would be in a true anomaly of $-135° 15'$. On December 6, 1798, when Brandes witnessed a great meteoric display, as the earth traversed the comet's orbit, it was in a true anomaly of about $-103°$, but in 1838, when under similar conditions meteors were observed in large numbers in Europe, Asia, and America, December 5–8, the comet's true anomaly was about $-128°$.

As regards the recovery of the comet in 1879, though perhaps not hopeless, a very strict examination of the

heavens in the neighbourhood of its projected track with considerable optical power may be necessary, and it will be particularly a case where the Astronomer-Royal's principle of mounting involved in his "orbit-sweeper" might be brought into useful requisition.

ENCKE'S COMET.—Mr. John Tebbutt, of Windsor, New South Wales, announces his re-observation of the comet of Encke, in the evening twilight on August 3, with the aid of the late Dr. von Asten's ephemeris, which had reached him on July 22. On August 5 it was pretty bright in a 4½-inch refractor, notwithstanding the moonlight, and Mr. Tebbutt hoped to secure a good series of positions. This comet has not passed unobserved at any appearance since its periodicity was discovered by Encke nearly sixty years ago.

BRORSEN'S COMET.—Prof. Schulze, of Döbeln, has published an ephemeris of Brorsen's comet of short period for the reappearance in 1879, founded upon a new discussion of the observations at the last two returns, and the application of the perturbations of Venus, the Earth, Mars, Jupiter, and Saturn. The comet will arrive at perihelion on March 30, and at its least distance from the earth on May 9, about which time its north declination will exceed 65°, so that it will be very favourably situated for observation in these latitudes. Prof. Schulze thinks the comet may be detected at the southern observations before the end of February.

NEW MINOR PLANETS.—Two new minor planets have been added to the list: the first detected by Prof. Peters, at Clinton, U.S., on September 18, the second by Prof. Watson, at Ann Arbor, on September 22—thus raising the number of known members of the group to 190.

AUGUSTUS HEINRICH PETERMANN

WE regret to record the sudden death, at Gotha, on September 26, of Dr. Augustus H. Petermann, one of the first cartographers of the present day. He was born at Bleicherode, in the neighbourhood of Nordhausen, Prussia, April 18, 1822. Although destined by his parents for the church, a pronounced taste for geographical study led to his entrance, at the age of seventeen, into the Geographische Kunstschule, founded shortly before at Potsdam, by Berghaus. Here, under the guidance of this famous geographer, he made rapid progress, and soon attracted the attention of leading *savants*. Among others Humboldt became interested in him, and entrusted him, when but nineteen years of age, with the preparation of the map accompanying his well-known work on Central Asia. Four years later, in 1845, he went to Edinburgh, in order to assist in the preparation of the English edition of the great Physical Atlas of Berghaus, issued by Johnston. Accompanying Petermann to Edinburgh was Henry Lange (now Dr. Lange, of the Berlin Statistical Bureau). These two, along with the late Keith Johnston, made an interesting tour through the Scottish Highlands, one result of which was a sketch or diagram of the Grampian range by Petermann, which he afterwards, we believe, published in London. In 1847 he settled in London, and was promptly elected into the Royal Geographical Society. His seven years' residence in London (1847-54) was one of continuous activity. Aside from the numerous maps which he executed, he contributed regularly to the *Athenæum* a *résumé* of the progress of geographical discovery, and issued, in union with Thomas Milner, an "Atlas of Physical Geography," and a fine folio atlas to illustrate Barth and Oberweg's travels in Africa. Two other maps which Petermann brought out during his stay in London still maintain their place on Stanford's list—a hydrographical and a population map of the British Islands. It was the publication of these maps, we believe, which enabled him to obtain the favourable notice of Baron Bunsen, who materially assisted Petermann in his progress. His connection with English publishers has always been close, and the last edition of the "Encyclopædia Brittanica" contains a number of admirable geographical articles from his pen. In 1855 he returned to his native land to take the management of Justus Perthes' Geographical Institute in Gotha, where an unlimited field was offered to his restless activity. In the same year he commenced the publication of the *Mittheilungen*, the successor of the *Geographisches Jahrbuch*, started by Berghaus. Under his careful editorship this periodical has become almost indispensable to those desiring to keep *au courant* with the progress of geographical discovery. No small portion of its rich and varied contents, as well as of its finely-executed maps, were due directly to Petermann. As cartographer Petermann was unwearied, and a constant succession of admirable maps have been executed by him during the past twenty-five years. Among these we might mention especially a great part of the magnificent collection forming Stieler's "Grosser Atlas," completed two years since; the map of the United States (1875), regarded by the government officials of that country as the most perfect extant; the maps accompanying the African travels of Barth and Rohlfs; and the lately-issued charts of the Arctic regions and the Turkish empire.

It is, however, chiefly by his criticism, his advice, and his enthusiastic, unwearied support of all attempts at geographical discovery, that Dr. Petermann has rendered his greatest services. Standing practically at the head of European geographers, the depositary of all that was being attempted or had been executed, esteemed and regarded by the authorities of the leading nations, he has enabled, by his personal efforts and wide-spread influence, many of our prominent explorers to find the necessary sinews of war, and successfully realise their plans of travel. Two great departments of geographical exploration have specially possessed his sympathies. The first was that of Arctic exploration, and dates from the interest excited during his residence in England, by the expeditions sent forth in search of Franklin. It was almost entirely due to his exertions that the German government equipped the successive expeditions of Werner (1865), Koldewey (1868), and Koldewey and Hegemann (1869), all of which aimed at the exploration of the east coast of Greenland—Petermann's favourite route for approaching the Pole. His advice was likewise of material assistance in the preparations for the late Austrian, English, Dutch, and Swedish expeditions, and few voyages to the icy regions have been undertaken of late years without consultation with Petermann on the general plan.

A still deeper interest was shown by him in the matter of African travel. The important expedition of Barth, Overweg, and Vogel, in 1849, was instituted by the Prussian Government, chiefly on the recommendations of Bunsen and Petermann. Heuglin's and Munzinger's expedition in 1861 was also due entirely to his instigation. Rohlfs' journey in Morocco, as well as Mauch's expedition in South Africa, would have been impossible without his assistance, and his name is closely associated with the important expeditions of Schweinfurth and Nachtigal.

The numerous contributions of Petermann to geographical literature are contained in the *Mittheilungen*, with the exception of those published in English. Of late years he has occupied the chair of geography at the Gotha Polytechnic.

As we have said, the *Geographische Mittheilungen* has become indispensable to all who wish to keep pace with the progress of geography; and from its pages a fair idea may be obtained of the all-comprehensiveness of that department of knowledge. We are glad to learn, from a circular signed by Dr. Behm, the efficient colleague of Petermann, that this *facile princeps* of geographical

journals is to be continued. We trust its comprehensiveness, thoroughly scientific character, and general high standing will be maintained, and that it will continue a permanent monument to the genius, knowledge, and zeal of its founder.

THE NORWEGIAN NORTH ATLANTIC EXPEDITION

THE *Vöringen* left Hammerfest on July 29 on its last cruise. On the 31st, at noon, Bear Island was reached. Here the expedition was kept till August 3, the weather being too stormy to allow sea work to be done. In the night of August 1-2 a party landed on the east side of the island, where the sea was sufficiently smooth to allow a boat to land: but foggy weather interfered with any observations of importance being made. Some birds were shot and some fossils collected. In the morning hours extensive fishing operations were carried on from the deck of the ship, now anchored in some 12 fathoms. From 4 to 7 A.M. 200 large cods were hauled. From a point about midway between Bear Island and Spitzbergen we worked first up a cross section towards west-north-west, till we found 1,149 fathoms' depth on the afternoon of the 4th. From this point the course was shaped for South Cape, Spitzbergen. At noon on the 5th we made the cape, sailed round the island lying off the cape, and entered the Stor-Fjord. Here the sun was shining and the water smooth, so Capt. Wille swung the ship for deviation. The next morning we dredged on the bank lying south-east of South Cape; here the temperature was $-1^{\circ}\cdot 2$ C. at the bottom, in 140 fathoms, and zero in 120 fathoms. In the upper layers the temperature was very irregularly distributed, both increasing and decreasing with depth. We went again round the islands and to the west side of South Cape, taking here a departure for a larger cross section along the parallel of the cape towards Greenland. Having crossed the Spitzbergen bank, we sounded 523, 743, 1,017, 1,429, 1,487, and 1,686 fathoms, when we at last, on August 8, were stopped by the ice in 76° 26′ N. lat. and 0° 29′ W. long. Off the Spitzbergen bank we found 0° C. in a depth of 470 fathoms. The polar current was reached in long. 5° E. Station No. 360, where we met the ice, gave the following serial temperatures characteristic of the polar current:—Surface, $3^{\circ}\cdot 2$ C., 40 fathoms, $-1^{\circ}\cdot 3$; 70 fathoms, $-0^{\circ}\cdot 3$; 200 fathoms, $-0^{\circ}\cdot 7$; 300 fathoms, $-1^{\circ}\cdot 0$; 1,686 fathoms at bottom, $-1^{\circ}\cdot 3$. On this station we lost a trawl and 2,163 fathoms of dredge rope. The sea-bottom between Spitzbergen and Greenland was very rough; the trawl or dredge seldom came up without damage or having stones inclosed, some of which were rather heavy. We sailed, on August 9, northwards along the ice, and reached our next cross section on the 10th, lat. 77° 50′, long. 0° 9′ W. The soundings were, from west to east, 1,640, 1,686, 1,333, 1,343, 948, 110 fathoms. The polar current closed in about 4° E. long. Farther east, 0° C. was found in 300 to almost 500 fathoms. On Station No. 354, lat. 78° 1′, long. 6° 54′ E., we had the great satisfaction of verifying the Swedish sounding made in 1868 at the same place by von Otter in the *Sofia*. The Swedes found 1,350 fathoms, we found 1,343. This agreement gives me great confidence in von Otter's soundings, which were made with less perfect means than ours. The Swedish deep-sea soundings in the *Sofia* extend far westwards and northwards from Spitzbergen, and are therefore of the greatest importance. From our last cross-section we took a longitudinal section parallel to the coast of Spitzbergen. The depths reached were 421 fathoms (temperature $0^{\circ}\cdot 0$), 905 fathoms, and 459 fathoms in lat. 79° 59′, long. 5° 40′ E. There was 0° in 390 fathoms depth. There was ice floating in the surface temperature at $5^{\circ}\cdot 2$. This brought our section to a close.

It appears that here on the 80th parallel, the warm Atlantic current is still running northwards, backed up on the west coast of Spitzbergen. The polar ice, driven by northerly winds, is swimming on its back, and melted gradually off just like the end of the glaciers in the summer heat of the valleys. It was apparent that the current was rather strong towards the north, the ship's place being always, by observations and bearings, found more northerly than by dead reckoning. On the open sea it was found very difficult, not to say impossible, to determine the ship's place with the ordinary accuracy. The horizon was generally—as we observed when off the shore—lifted by a sort of mirage.

On August 15 we dropped our anchor at the Norway Islands, North Spitzbergen, where we took in some ballast. In the sound, where we were lying, the beach was formed of mere loose stones, granite, apparently burst asunder by frost. Flakes and small bergs of ice sailed through the sound with the tide and were often touching the shore, but I could not observe there any sign of the ice cutting any line or mark along the beach. From the Norway Islands we went out off Hackluyt Head, where we took a sounding, passed the Smeerenberg and the South Gat, and anchored in Magdalena Bay. The Admiralty chart of the last-named places, surveyed in 1818 by Franklin and Beechy, proved very accurate. In Magdalena Bay we found a bottom temperature of $-1^{\circ}\cdot 7$ to $-2^{\circ}\cdot 0$, in exact agreement with the results formed by M. Charles Martins in 1839 in the *La Recherche* expedition. Our last visit was in the Advent Bay ice-fjord, where Capt. Wille constructed a chart of the bay, assisted by Capt. Grieg and myself, who measured the base line, some trigonometrical angles, and took altitudes for latitude and longitude. Foggy weather prevented our visit to Bell Sound. On August 23 we left Spitzbergen, and on the 26th we anchored at Tromsö. On September 4 the *Vöringen* returned to Bergen and the expedition was closed. The three summers have yielded in all 375 sounding-stations, 113 temperature series, 44 dredgings, and 42 trawlings.

H. MOHN

THE ANCIENT CAPITAL OF ITHACA

IN a recent letter to the *Times* Dr. Schliemann describes his search for the ancient capital of the island of Ithaca. He began his researches in the valley called Polis, which is in the northern part of the island, and has generally been considered as the site of the Homeric capital of Ithaca—first, on account of its name, which is the Greek word for city; second, on account of its splendid harbour, at a distance of only two miles from a small island now called Mathitarió, which, being the only one in the strait between Ithaca and Cephalonia, has naturally always been identified with the Homeric island of Asteris, behind which the suitors of Penelope were in wait for Telemachus on his return from Pylos and Sparta ("Odyssey," iv., 844-847). As a fourth reason for the identity of Polis with the site of Ithaca's capital, he mentions an acropolis which one thinks to perceive on the very steep rock, at a height of about 400 feet, on the north side of the port. Dr. Schliemann found it to consist of a very irregular calcareous rock, which had evidently never been touched by the hands of man, and can most certainly never have served as a work of defence. There can be no doubt that the name of this valley is derived not, as has been hitherto thought, from a real city, but merely from an imaginary fortress.

Besides, this valley is the most fertile spot in Ithaca, and it can therefore never have been used for the site of a city; in fact, it never yet occurred in Greece that a city should have been built on fertile land, and least of all can such have been the case on the rocky island of Ithaca, where arable land is so exceedingly rare and precious.

The island Mathitarió Dr. Schliemann visited and carefully measured. Its length is 586 feet; its breadth varies between 108 feet and 176 feet. It cannot, on account of these small dimensions, possibly be identified with the Homeric Asteris, which, as the poet says, had two ports, each of them with two entrances.

Though for all these reasons Dr. Schliemann was perfectly convinced that no city can ever have occupied the fertile valley of Polis, yet he thought it in the interest of science to investigate the matter by actual excavations. He sunk there many shafts, but in nearly all of them he struck the natural rock in a depth of 10 to 13 feet, except in the middle of the valley, which seems to have been hollowed out to a great depth by a mountain torrent. Fragments of rudely-made black or white Greek pottery and pieces of tiles were all he found. There were only a few fragments of archaic pottery for which he could claim the sixth century B.C. Tombs are sometimes found on the neighbouring heights, but, as is proved by the pottery and coins contained in them, they are of the third, fourth, or fifth century B.C. Of the same period are also the antiquities found in a cavern to the right of the port of Polis; for an inscription found there Dr. Schliemann can with certainty claim the sixth or even the seventh century B.C. Therefore, the supposition that Polis is the site of the Homeric capital of Ithaca must now be definitely abandoned.

Dr. Schliemann afterwards carefully surveyed the remaining northern part of the island, but found nowhere the site of an ancient town, except in the environs of the small building of cyclopean masonry, usually called "School of Homer," which the owner of the property has lately converted into a small church. He refused Dr. Schliemann permission to excavate in the church, but allowed him to do so in the adjoining fields, where a number of rock-cut house foundations and remnants of cyclopean walls testified to the existence of an ancient settlement. He dug there a great many holes, but always struck the natural rock in less than 3 feet, and sometimes even in a depth of less than 12 inches; thus there can be no doubt that a town has existed here in classical times, and most probably it is the very town mentioned by Scylax Per. 34, and Ptolemæus III., 14, 13.

Dr. Schliemann proceeded thence to Mount Aetos, situated on the narrow isthmus, hardly one mile wide, which joins northern and southern Ithaca. He found everywhere the purest virgin soil, except on the very crest of the ridge, where, near the chapel of Hagios Georgios, he found a very small plain with an accumulation of artificial soil 10 feet deep. He dug there two long trenches, in one of which he brought to light a terrace-wall 7 feet high, consisting of huge polygonal blocks, well fitted together; to compare this wall to the modern terrace-walls which surround it is to compare a giant's work to a work of dwarfs. Of pottery he found there nothing but a few fragments of black Greek vases. Having here also failed in his researches he most carefully explored Mount Aetos, which rises to the height of 1,200 feet from the sea, and has on its artificially, but rudely, levelled summit a platform of triangular form, with two large cisterns and a small one, and remnants of six or seven small cyclopean buildings, which were either separate houses or—and more probably—chambers of the large cyclopean mansion which is said to have stood there, and is commonly called "the Castle of Ulysses." There can hardly be any doubt that the level summit of Mount Aetos was extended to the north and south-west by a huge cyclopean wall still existing, the space between the top and the wall being filled up with stones and *débris*. Thus the summit forms a quadrangular, even platform 166 feet 8 inches long by 127 feet 4 inches broad, so that there was on the summit ample room for a large mansion and a courtyard. To the north and south of the circuit-wall are towers of cyclopean masonry, from each of which a huge wall of immense boulders runs down. But at a certain distance these two walls begin to form a curve, and ultimately join together. Two more cyclopean walls run down from the top—the one in an easterly, the other in a south-easterly direction, and join the curve formed by the two first-named walls. Lastly, he mentions a huge circuit-wall about 50 feet below the upper circuit-wall. This wall has fallen on the west side, but is in a marvellous state of preservation on the other sides. To increase the strength of the place the foot of the rock has been cut away so as to form a perpendicular rock wall 20 ft. high. In the walls are recognisable three gates. Between all those cyclopean walls once stood a city, which may have contained 2,000 houses, either cut out in the rock or built of cyclopean masonry. Of 190 of these houses Dr. Schliemann has been able to find the ruins more or less well preserved. He measured twelve of them and found them between 21 feet and 63 feet long, and 15 feet to 20 feet broad. The usual size of the rudely cut stones is 5 feet in length, 4 feet 8 inches in breadth, and 2 feet in thickness. The size of these stones by far exceeds that of the stones in the cyclopean houses Dr. Schliemann discovered at Mycenæ and Tiryns. Some of the houses consisted of only one room, others had four or even six chambers. This cyclopean capital is unique in the world, and every admirer of Homer ought to see it.

For two weeks Dr. Schliemann excavated with thirty workmen in those cyclopean buildings; but fragments of pottery, which has no resemblance to any of the Mycenæan pottery, but is much like that from the two most ancient cities at Troy; fragments of most curious tiles with impressed ornaments; also two with a sort of written characters which he has not yet had time to copy; further, the fragments of a most curious handmill—wer the only result of all his labour.

Dr. Schliemann has also commenced excavating the stalactite grotto near the little port of Dexia, which is generally identified with the port of Phorkys, where Ulysses was landed by the Phæacians, the grotto being rightly considered to be identical with the Homeric grotto of the Nymphs, in which Ulysses, assisted by Minerva, hid his treasures. But having opened a trench just before the little altar, down to the rock, without even finding a potsherd, he abandoned this ungrateful excavation. The grotto is very spacious, and it exactly answers the description of Homer, who says "that it has two entrances, one on its north side for men, and one on its south side for the immortal gods, for no man can enter by the divine door." All this is true, but by the entrance for the gods he means the artificially cut hole in the vault of the grotto, which must have served as a chimney to lead off the smoke of the sacrificial fires. From this chimney to the bottom of the grotto is 56 feet, and, of course, no man can enter by this way. From the vault of the grotto hang innumerable stalactites, which have given to Homer the idea of the stone urns and amphoras, and the stone frames and looms on which the Nymphs weave purple-coloured mantles and veils. Dr. Schliemann most carefully explored the whole southern portion of Ithaca. The town of Vathy, the present capital of Ithaca, is not yet a hundred years old, and the complete absence of ancient potsherds on the flat soil seems to prove that there has been no city or village on the site in antiquity. Before Vathy was founded the city was on a rocky height about one mile further south. On the site of the old town he found but a very small accumulation of *débris*, and no trace of ancient pottery.

Near the south-east extremity of the island, about 4½ miles from Vathy, are a number of stable-like rooms, averaging 25 feet in length and 10 feet in breadth, partly rock cut, partly formed by cyclopean walls of very huge stones, in which Homer must have seen the twelve swine stables built by the divine swine-herd Eumæus. To the east of these stables and just in front of them, thousands

of very common but most ancient potsherds indicate the existence of an ancient rustic habitation, which Homer appears to have described to us as the house and station of Eumæus. This is the more probable as at a very short distance to east of this site, and near the sea, is a white cliff with a perpendicular descent of 100 feet which until now is called Korax—*i.e.*, the Raven Rock, to which Homer refers when he represents Ulysses as challenging Eumæus "to precipitate him from the great rock" if he finds that he is telling lies (Od. xiv. 398). Below the Korax, in a recess, is natural and always plentiful pure water, which the tradition identifies with Homer's fountain of Arethusa, from which Eumæus's swine were watered. Dr. Schliemann excavated as well in the stables as in front of them on the site of the rustic habitation; the stable he found filled with stones, but on the site of the house he struck the rock in a depth of one foot, and found there fragments of very interesting, most ancient, unpainted pottery, also of pottery with red bands, and masses of broken tiles.

Dr. Schliemann states that Ithaca is, like Utica, a Phœnician word, and means "colony," and that the type of the Ithacans is decidedly Phœnician. According to Homer Laertes's grandfather was Poseidon, and Mr. Gladstone is therefore perfectly right that the descent from Poseidon always means "descent from the Phœnicians."

Dr. Schliemann has obtained a new firman for Troy. He left Athens on September 18 for the Troad to continue his long interrupted excavation of Troy. His first work will be to bring to light the whole of the mansion immediately to the north and north-west of the gate, which seems to belong to the ancient city's chief or king.

ARE THE "ELEMENTS" ELEMENTARY?[1]

THE problem set before us by the words which I have chosen as a heading for this article is a vast one; unfortunately the data upon which an answer must be founded are in themselves vague and meagre. It is useless attempting to draw an exact conclusion from inexact data. If the degree of probability which attaches itself to the data is small, the probability of the conclusion being true must be yet smaller.

In the times of the ancients men do not appear to have attached any very definite idea to the word "Element." An element was a something, a material or an imaginary something—it did not very much matter which—a something which one might suppose, if one were so minded, to form a sub-stratum upon which other, apparently more complex, things rested. Fire was an element; it was supposed to enter into the constitution of matter of many kinds. Some people said they believed that fire and water formed earth of different kinds; others averred that air and water were the foundations of all things. But it was perfectly legitimate for a third person to tell the two former that they were completely in error, that *really* sulphur and salt were the primary elements, and that from these all other forms of matter arose.

No exact data concerning the possibility of transforming earth into air, or water into fire, or salt into sulphur were forthcoming. Men did not generally trouble themselves with investigations into the actual properties of the so-called elements. Everything was founded on supposition; the human mind was superior to nature, and could project itself upon nature and explain nature.

Such a method could lead to no true knowledge of natural phenomena. To-day we have altered our method of investigation. Nature presents us with a mass of materials; most of these we can decompose into two or more forms of matter, but some of these resist all efforts hitherto made to effect their analysis. The latter we call elements, the former compounds. Our knowledge is imperfect; we acknowledge the imperfection, but attempt to make the knowledge *exact so far as it goes*. Whether the so-called elements are or are not capable of further subdivision is an open question. Whatever answer this question may finally receive, the superstructure of chemical science will remain unshaken. We may find it necessary to alter the form of many statements; the facts and, I am persuaded, many of the theories, will remain.

An element is then a substance which has hitherto yielded no simpler form of matter than itself. We make the hypothesis that matter is built up or compounded of those substances which we call elements. But this is of course only an hypothesis. So long as we accept it as such it is of the utmost service to us; whenever we erect it into a dogma it ceases to become an aid to the investigation of nature, and begins to exercise a tyranny over us.

An amusing and instructive instance of the narrowing and deadening effect of accepting an hypothesis dogmatically is narrated in Prof. Bryce's recently-published book on Trans-Caucasia. Prof. Bryce accomplished the ascent of Mount Ararat: tradition says that no one has ever been to the summit of this mountain; the inhabitants of the neighbouring country have formed this saying into a dogma which teaches that no one can ascend to the top of Ararat. When Prof. Bryce told the Archimandrite of the district that he had been to the summit the old man only smiled a sweet, sad, pitying smile, and said it was impossible.

The more modern history of the chemical elements warns us against dogmatising concerning the nature of these bodies. Potash and soda were classed among the elements until the year 1807. Water was for ages regarded as elementary; Cavendish first taught us that the long-cherished tradition was false.

The problem of the nature of the elements is one which requires the use of the imagination; it is a problem in endeavouring to solve which we are very ready to give the reins to this faculty, or rather to allow the lower power of fancy to usurp the place of the more divine imagination—and thus we run riot. The naturalist who approaches the investigation presented by the chemical elements had need to learn the scientific use of the imagination.

Many years ago an hypothesis was started by Prout to the effect that the elements are all compounds of hydrogen, that hydrogen is the primary form of matter, and that the molecule of each element is composed of a varying number of atoms of hydrogen. If this hypothesis were correct the combining or atomic weights of the elements would be simple multiples of the combining or atomic weight of hydrogen, *i.e.*, multiples of 1. The experiments of Dumas lent support to the hypothesis of Prout, but the later and more exact researches of Stas negatived the idea.

Stas showed, in a wonderful series of investigations, that the atomic weights of the elements are not simple multiples of 1, nor of $\frac{1}{2}$, as Dumas had supposed, but that they are fractional numbers. Stas further showed that the same number, as representing the atomic weight of a given element, is obtained by different processes of investigation.

But may not Prout's hypothesis have some truth underlying it? Are the elements really elementary? Stas's researches do not answer this question. We may put the general question in two forms. Are the elements compounds, in varying proportions, of a few simple bodies? or, Are the elements compounds, in varying proportions, of *one* primary form of matter? Let us look at these questions in succession—and first we may frame the hypothesis that the elements are compounds of a few simple bodies.

In order to learn what are the general properties exhibited by a series of bodies all of which are compounds,

[1] A paper read before the Owens College Chemical Society.

in varying proportions, of a few simple bodies, let us consider one of the homologous series of hydrocarbons; say the marsh-gas series. CH_4, C_2H_6, C_3H_8, C_4H_{10}, C_5H_{12}, C_6H_{14}, &c., &c.; generally C_nH_{2n+2}. The members of this series are all compounds, in varying proportions, of carbon and hydrogen; each differs from the preceding by an increment of CH_2. The difference between the molecular weights (the weights of two vols.) of the members of the series is 14. The physical properties of the series show a gradation from the first upwards. The first is gaseous at ordinary temperatures: as we ascend the series we have liquids of gradually-diminishing liquidity, then solids, the melting-points of which gradually increase. The chemical properties of the series, so far as these have been investigated, also exhibit a regular gradation. If we divide the specific heats of these compounds by their molecular weights, we do not obtain the same number for each—in other words, the molecular heat of the members of this series varies: this is a point of some importance. The specific volumes —products obtained by dividing molecular weights by specific gravities determined at that point at which each vapour exerts the same tension, that is, at the boiling points of the liquids—of the members of this series differ by 22. A simple relation of some kind most probably exists between the densities of the members of this series, between the actions exerted by these bodies on light, &c., &c.

Take now a group of allied elements:—Oxygen (16), sulphur (32), selenium (79·5), tellurium (128). The atomic weights—or the molecular weights, whichever form is preferred—of the higher members of the series are multiples of the atomic weight of the first: $16 \times 2 = 32$, $16 \times 5 = 80$; $16 \times 8 = 128$. Oxygen is a gas, except under conditions of great pressure; sulphur melts at about 115°, selenium at about 100°, but a modification at 215°; tellurium at 450° C. There is a gradation in the general chemical nature of the four elements. There is a simple relation between the specific volumes of these four elements in the solid state; this relation is expressed by the numbers 1 : 3 : 3 : 4. The atomic heats of the three last-named substances are the same; the atomic heat of solid oxygen, as deduced from observations carried out on compounds, is rather less than the number representing the atomic heats of sulphur, selenium, and tellurium. I might adduce other series of elements, let one suffice:—

	Lithium.	Sodium.	Potassium.	Potassium.	Rubidium.	Cæsium.
Atomic weight =	7	23	39·1	39·1	85·4	133
Mean ,, =	—	23·05	—	—	86·05	—

The atomic weight of the second member of each sub-section of this series is almost exactly the mean of the atomic weights of the first and third; in each case the number representing the atomic weight of the middle element is a very little less than the mean of the atomic weights of the elements at the extremes of the series. The specific volumes of the metals lithium, sodium, potassium, and rubidium (the specific gravity of cæsium is unknown) are respectively, 11·9, 23·7, 45·1, and 56·2; these numbers are nearly in the proportion of 1 : 2 : 4 : 5; there is a regular gradation, therefore, in the specific volumes of the members of the present series. The physical and chemical properties of the series show gradations, which, so far as they have been examined, appear capable of tolerably simple generalisation. The atomic heat of the five metals is represented by a (practically) constant number. Now most of these facts are quite in keeping with the hypothesis that the elements which I have noticed are compounds of simpler forms; the properties of the compounds of the homologous series, C_nH_{2n+2}, are in very many respects analogous with the properties of the two series of so-called elements to which I have drawn attention. There is, however, one important difference between a certain physical property of the homologous series and the same property as exhibited in the elementary series—the atomic or molecular heats of the elements are, with few exceptions, the same; the numbers expressing the molecular heats of the members of the homologous hydrocarbon series are multiples of each other.

"It seems a general law," says Berthelot, "that the molecular heats of polymerised radicles are multiples of each other, whereas the molecular heats of the elements are, with very few exceptions, identical." The apparent exceptions among the elements, we have good reason to believe, will be found to obey the rule when more exact investigations have been carried out. This difference between the molecular heats of the elements and the molecular heats of series of homologous hydrocarbons lessens the probability of the elements being really compounds, in varying proportions, of a few simple bodies; or at any rate, it leads us to believe that, as Berthelot says, the phenomena attending the decomposition of the elements—supposing them to be really compounds—must be different from the phenomena attending the decomposition of those bodies which we know to be compounds. Nevertheless, I think that too much weight may be attached to this fact of differences in molecular heats. We do know of many compound gases, gases in the formation of which a very considerable amount of condensation occurs, but which have almost identical molecular heats. We have whole series of similarly constituted groups of compounds having the same molecular heats, *i.e.*, provided we accept the ordinary formulæ for solid compounds as molecular formulæ. So that the mere fact that the elements have the same molecular heats need not, I think, be a bar in the way of regarding these bodies as compounds, provided we have other evidence pointing in that direction.

But I must now briefly consider the second form under which the general question of the nature of the elements presents itself, viz., are the elements compounds, in varying proportions, of *one* primary form of matter? As a matter of fact we know of compounds in varying proportions of the same elements; we know also of compounds in varying proportions of one and the same element. The facts which have been amassed concerning allotropy and isomerism must be of service in any attempt which may be made to answer the question we are now to consider.

It is generally possible to trace a simple relation between the specific volumes $\left(\frac{\text{mol. wt.}}{\text{Sp. Gr.}}\right)$ of the various members of a group of elements; we have seen that such a relation exists in the two groups already considered, viz., the oxygen group and the potassium group. But it is stated by F. W. Clarke, of Cincinnati, who has partially investigated this subject of specific volumes, that so far as experiment has gone, no simple relation can be traced between the specific volumes of allotropic modifications of one and the same element. This statement appears to me to assume an amount of knowledge which we really do not possess. Clarke finds for the specific volume of ordinary sulphur the numbers 10·4 and 15·6; for prismatic sulphur he finds a number varying from 16·3 to 16·7; there is no simple relation between this number and either of the former. But he has assumed that the atomic weight of each allotrope is the same, and we have no data warranting such an assumption; the knowledge which we do possess points rather to an opposite conclusion. I think I am right in saying that in the case of oxygen and ozone we do possess some accurate knowledge of the (relative) molecular weights of two allotropes; these molecular weights are different; hence, probably, the atomic weights of the allotropes are also different.

M. M. PATTISON MUIR

(*To be continued.*)

ON THE NATURE OF VIBRATORY MOTIONS[1]

II.

Blackburn's Double Pendulum.

EXPERIMENT 7.—Let us return to our sand-pendulum. We have examined the vibrations of a single pendulum, let us now examine the vibrations of a double pendulum, giving two vibrations at once. The little copper ring *r*, in Fig. 7, on the cord of our pendulum, will slip up and down, and by moving it in either direction we can combine two pendulums in one. Slide it one quarter way up the cord, and the double cord will be drawn together below the ring. Now, if we pull the bob to the right or left, we can make it swing from the copper ring just as if this point were a new place of support for a new pendulum. As it swings, you observe that the two cords above the ring are at rest. But the upper pendulum can also be made to swing forward and backward, and then we shall have two pendulums combined. Let us try this and see what will be the result.

Just here we shall find it more convenient to use the metric measure, as it is much more simple and easy to

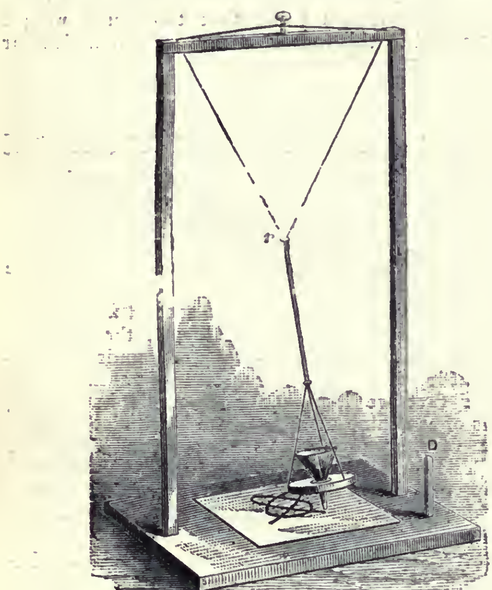


FIG. 7.

remember than the common measure of feet and inches. If you have no metric measure you had best buy one, or make one. Get a wooden rod just $39\frac{37}{100}$ inches long, and divide this length into 100 parts. To assist you in this you may remember that 1 inch is equal to $25\frac{4}{10}$ millimetres. Ten millimetres make a centimetre, and 100 centimetres make a metre.

Now slide the ring *r*, Fig. 7, up the cords till it is 25 centimetres from the middle of the thickness of the bob. Then make it exactly 100 centimetres from the under side of the cross-bar to the middle of the thickness of the bob, by turning the violin-key on the top of the apparatus.

At D, Fig. 7, is a small post. This post is set up anywhere on a line drawn from the centre of the platform, and making an angle of 45° with a line drawn from one upright to the other. Fasten a bit of thread to the string on the bob that is nearest to the post, and draw the bob toward the post and fasten it there. When the bob is perfectly still fill the funnel with sand, and then hold a lighted match under the thread. The thread will burn, and the bob will start off on its journey. Now, in place of swinging in a straight line, it follows a curve, and the sand traces this figure over and over.

FIG. 8.

Here we have a most singular result, and we may well pause and study it out. You can readily see that we have here two pendulums. One-quarter of the pendulum swings from the copper ring, and, at the same time, the whole pendulum swings from the cross-bar. The bob cannot move in two directions at the same time, so it makes a compromise and follows a new path that is made up of the two directions.

The most important fact that has been discovered in relation to the movements of vibrating pendulums is that the times of their vibrations vary as the square roots of their lengths. The short pendulum above the ring is 25 centimetres long, or one-quarter of the length of the longer pendulum, and, according to this rule, it moves twice as fast. The two pendulums swing, one 25 centimetres and the other 100 centimetres long, yet one really moves twice as fast as the other. While the long pendulum is making one vibration the short one makes two. The times of their vibrations, therefore, stand as 1 is to 2, or, expressed in another way, 1 : 2.

Experiment 8.—Let us try other proportions and see what the double pendulum will trace. Suppose we wish one pendulum to make 2 vibrations while the other makes 3. Still keeping the middle of the bob at 100 centimetres from the cross-bar, let us see where the ring must be placed. The square of 2 is 4, and the square of 3 is 9. Hence the two pendulums of the double pendulum must have lengths as 4 is to 9. But the longer pendulum is always 1,000 millimetres. Hence the shorter pendulum will be found by the proportion 9 : 4 : : 1,000 : 444·4 millimetres. Therefore we must slide the ring up the cord till it is 444·4 millimetres above the middle of the thickness of the bob.

Fasten the bob to the post as before, fill it with sand, and burn the thread, and the swinging bob will make this singular figure (Fig. 9).

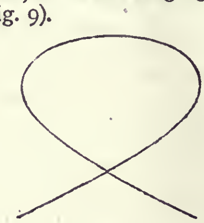


FIG. 9.

Experiment 9.—From these directions you can go on and try all the simple ratios, such as 3 : 4, 4 : 5, 5 : 6, 6 : 7, 7 : 8, and 8 : 9. In each case raise the two figures to their squares, then multiply the larger number by 1,000, and divide the product by the smaller number; the quotient will give you the length of the smaller pendulum in millimetres. Thus the length for rates of vibration, as 3 is to 4, is found as follows: $3 \times 3 = 9$, $4 \times 4 = 16$, and $\frac{9 \times 1{,}000}{16} = 562{\cdot}5$ millimetres.

The table (Fig. 10) gives, in the first and second columns, the rates of vibration, and in the third and

[1] From a forthcoming work on "Sound: a Series of Simple, Entertaining, and Inexpensive Experiments in the Phenomena of Sound, for the Use of Students of every Age." By Alfred Marshall Mayer, Professor of Physics in the Stevens Institute of Technology. Communicated by the Author. (Continued from p. 574)

fourth columns the corresponding lengths of the longer and shorter pendulums. Opposite these lengths are the figures which these double pendulums trace. In the sixth column are the names of the musical intervals formed by two notes, which are made by numbers of sonorous vibrations, bearing to each other the ratios given in the first and second columns.

	Mm.	Mm.	
1 : 2 =	1,000 :	250·0.........	Octave.
2 : 3 =	1,000 :	444·4.........	Fifth.
3 : 4 =	1,000 :	562·5.........	Fourth.
4 : 5 =	1,000 :	640·0.......	Major Third.
5 : 6 =	1,000 :	694·4.........	Minor Third.
6 : 7 =	1,000 :	734·6.........	Sub-Minor Third.
7 : 8 =	1,000 :	766·6.........	Super Second.
8 : 9 =	1,000 :	790·1.........	Second.

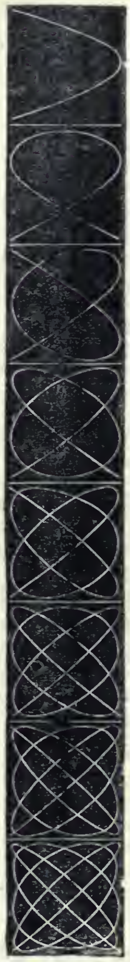


Fig. 10.

Prof. Kundt's Experiment, made with a Whistle and a Lamp Chimney, showing that, as in Wind Instruments, a vibrating Column of Air may originate Sonorous Vibrations.

Experiment 10.—The chimneys of student-lamps have a fashion of breaking just at the thin, narrow part near the bottom. Such a broken chimney is very useful in our experiments. At A, in Fig. 11, is such a broken chimney, closed at the broken end with wax. A cork is fitted to the other end of the chimney, and has a hole bored through its centre. In this hole is inserted part of a common wooden whistle. At B is an exact representation of such a whistle, and the cross-line at C shows where it is to be cut in two. Only the upper part is used, and this is tightly fitted into the cork.

Inside the tube is a small quantity of very fine precipitated silica, probably the lightest powder known. Hold the tube in a horizontal position and blow the whistle. The silica powder springs up into groups of thin vertical plates, separated by spots of powder at rest, as in the figure. This is a very beautiful and striking experiment.

Experiment 11.—The following experiment shows that the sound is caused by the vibrations of the column of air in the tube and whistle, and not by the vibrations of these solid bodies. Grasp the tube and whistle tightly in the hands. These bodies are thus prevented from vibrating, yet the sound remains the same.

The breath driven through the mouth of the whistle strikes on the sharp edge of the opening at the side of the whistle, and sets up a flutter or vibration of air. The air within the glass tube now takes part in the vibrations, the light silica powder vibrates with it, and makes the vibrations visible.

To exhibit this experiment before a number of people, lay the tube carefully on the water-lantern before the heliostat, and throw a projection of the tube and the powder on the screen. When the whistle is sounded, all in the room can see the fine powder leaping up in the tube into thin, upright plates.

Experiment 12.—Mr. Geyer has made the following

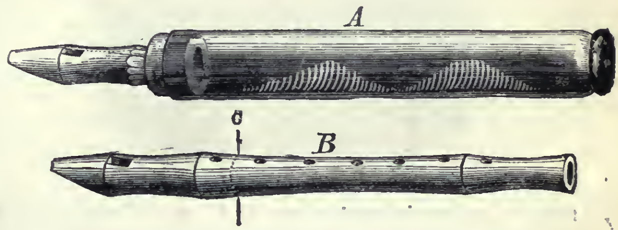


Fig. 11.

pleasing modification of this experiment:—Take a glass tube about 2 feet (61 centimetres) long and ¾ inch (19 millimetres) diameter. One end of this tube is stopped with a cork; then some silica is poured into it. The other end is placed in the mouth. Singing into the tube, a note is soon struck which causes the silica to raise itself in groups of vertical plates, separated by places where the powder is at rest, the number of these groups and their positions in the tube changing with the note sung.

We have now seen how solids, like steel or brass, may vibrate and give a sound. We have heard a musical sound from vibrating water, and these last experiments prove that a gas, like air, may also vibrate and give a sound. In the next chapter you will find experiments which show how these vibrations move through solids, through liquids, and through the air.

On the Interference of Sonorous Vibrations and on the Beats of Sound.

Experiment 13.—Cut out two small triangles of copper foil or tinsel, of the same size, and with wax fasten one on the end of each of the prongs of a tuning-fork. Put the fork in the wooden block and set up the guide. Prepare a strip of smoked glass, and then make the fork vibrate and slide the glass under it, and get two traces, one from each prong.

Holding the glass up to the light you will see the double trace, as shown in Fig. 12. You observe that the

Fig. 12.

wavy lines move apart and then draw together. This shows us that the two prongs, in vibrating, do not move in the same direction at the same time, but always in opposite directions. They swing toward each other, then away from each other.

Experiment 14.—What is the effect of this movement

of the prongs of the fork on the air? A simple experiment will answer this question.

Place three lighted candles on the table at A, B, and C (Fig. 13). Hold the hands upright, with the space between the palms opposite A, while the backs of the hands face the candles B and C. Now move the hands near each other, then separate them, and make these motions steadily and not too quickly. You thus repeat the motions of the prongs of the fork. While vibrating the hands observe attentively the flames of the candles. When the hands are coming nearer each other, the air is

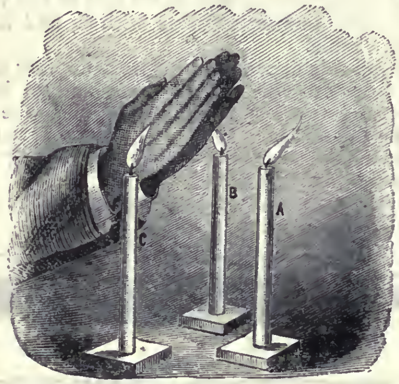


FIG. 13.

forced out from between them, and a puff of air is driven against the flame A, as is shown by its bending away from the hands. But, during the above movement, the backs of the hands have drawn the flame toward them, as shown in Fig. 13. When the hands are separating, the air rushes in between them, and the flame A is drawn toward the hands by this motion of the air, while at the same time the flames at B and C are driven away from the backs of the hands. From this experiment it is seen that the space between the prongs and the faces of the prongs

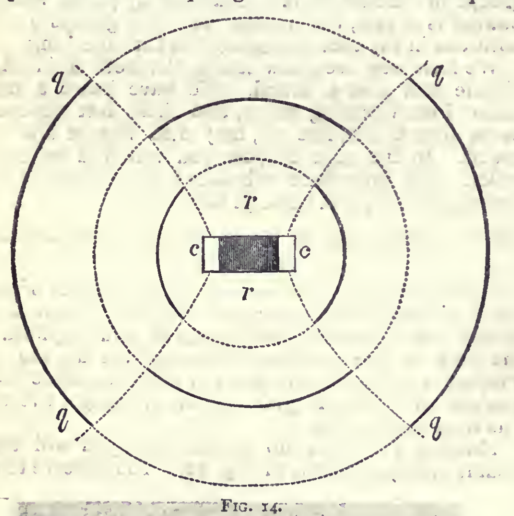


FIG. 14.

of a fork are, at the same instant, always acting oppositely on the air.

This will be made clearer by the study of the diagram, Fig. 14.

This figure supposes the student looking down on the tops of the prongs of the fork. Imagine the prongs swinging away from each other in their vibration. Then the action of the faces c and c on the air is to condense it, and this condensation tends to spread all around the fork. But, by the same movement, the space $r\,r$ between the prongs is enlarged, and hence a rarefaction is made there. This rarefaction also spreads all around the fork. But, as the condensations produced at c and c and the rarefaction at r and r spread with the same velocity, it follows that they must meet along the dotted lines q, q, q, q, drawn from the edges of the fork outwards. The black $\frac{1}{4}$-circle lines around the fork in Fig. 14 represent the middle of the condensed shells of air, while the dotted $\frac{1}{4}$-circle lines stand for the middle of the rarefied shells of air.

Now what must happen along these dotted lines, or, rather, surfaces? Evidently there is a struggle here between the condensations and the rarefactions. The former tend to make the molecules of air go nearer together, the latter try to separate them; but, as these actions are equal, and as the air is pulled in opposite directions at the same time, it remains at rest—does not vibrate. Therefore, along the surfaces q, q, q, q, there is silence. When the prongs vibrate toward each other they make the reverse actions on the air; that is, rarefactions are now sent out from c and c, while condensations are sent from r and r, but the same effect of silence along q, q, q, q is produced.

Experiment 15.—That this is so is readily proved by the following simple experiment:—Vibrate the fork and hold it upright near the ear. Now slowly turn it round. During one revolution of the fork on its foot you will perceive that the sound goes through four changes. Four times it was loud, and four times it was almost, if not quite gone. Twirl the fork before the ear of a companion; he will tell you when it makes the loudest sound and when it becomes silent. You will find that when it is loudest the faces c, c of the prongs, or the spaces r, r between them, are facing his ear; and when he tells you that there is silence you will find that the edges of the fork, that is, the planes q, q, q, q, are toward his ear.

(*To be continued.*)

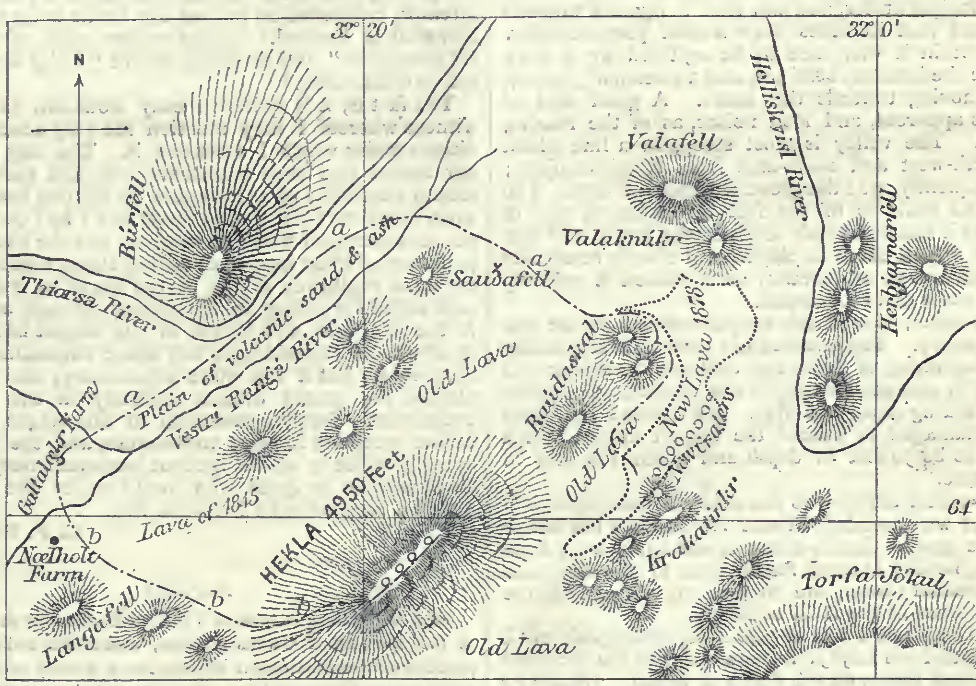
ON AN ASCENT OF MOUNT HEKLA, AND ON THE ERUPTION OF FEBRUARY 27, 1878

ON February 27 last severe earthquakes were felt throughout the south-west portion of Iceland, particularly in the districts of Land, Rangarollir, Hreppa, and Fljotschlith, which are situated immediately to the south and south-west of Mount Hekla. Between 8 and 9 P.M. an intense illumination of the sky, at first believed to be actual fire, was seen to the south-east. This was found to be due to the reflection by clouds of the light emitted by molten lava within a subsidiary crater, or *bocca del fuoco*, as the Italians would call it, of Hekla. On the following day dense columns of smoke ascended from the crater, and quantities of volcanic ashes fell in the districts of Hreppa and Biskupstundur. The light was seen at Reykjavik, nearly seventy miles distant, and there appeared to be two vents of fire.

One month after the eruption Prof. Tómas Hallgrimson visited the district and endeavoured to discover the exact position of the new crater. He found it in the Raúdaskal Valley, about four miles to the north-east of Hekla, and in connection with one of its outlying spurs. The chief crater was observed to be near the northern base of Krakatinkr, and a good deal of new lava was heaped around it. Herr Nielsens, a merchant of Eyrabakki, on the southern coast of Iceland, visited the scene of the eruption about the same time, and by ascending Krakatinkr he was able to look down into the new crater. He also determined its position, and traced the course of the new lava streams. The map which is here reproduced (for which I am indebted to Sjera Gudmundr Jonsson, the priest of Stóruvellir, a hamlet near to Hekla) is a copy of Nielsens' sketch made on the spot.

Upwards of a month ago (August 21) I visited the scene

of the eruption, in company with three friends. Two guides accompanied us, and we took a relay of ponies in case of accident or fatigue. We left the Galtalœkr farm (see the map, route *a a*) at 7.30 A.M., crossed a small streamlet, and soon came upon a growth of dwarf birch-trees from one to two feet in height. Here we put up a covey of very tame ptarmigan. On emerging from the underwood we entered a long level plain of volcanic sand and ash, extending between the river Thiorsa and the smaller stream called Vestri-Rángá. We cantered over the plain for about ten miles, turning aside for a moment to look at the falls of the Thiorsa, at the southern base of Búrfell. The substance of the plain is black volcanic sand and ash, with occasional white, grey, and red patches of pumice. At 9.15 A.M. we found ourselves near the termination of the Vestri-Rángá, and, bearing to the south-east, we forded the stream, and almost immediately afterwards came upon fields of volcanic ash and lava. The ground now became very rough, masses of lava were strewn in all directions, and there was of course no track. We passed several cones of ashes emitted by extinct craters, and presently saw in front of us Raúdaskal—the red mountain—a subsidiary cone probably thrown up in 1554. We wound along the Raudaskal valley, which is between six and seven miles in length and nearly two miles broad, passing by the base of several large cinder-cones. The new lava of 1878 was soon sighted, and we picked our way for about three miles between the edge of the field and the sloping sides of the cinder cones. By the time we had ridden some twenty-three miles from Galtalœkr we found ourselves at the edge of a field of old lava, contiguous to the lava of 1878, and in sight of the largest of the new craters. The summit of Hekla was on our right, about four miles distant, and Krakatinkr was nearly facing us to the south. Here we left the ponies, tied head and tail together, to prevent straying during our absence. The only possible way to get nearer to the new crater was to clamber over the old



Map of Mount Hekla showing position of the new craters. Route *a a*, from Galtalœkr to the new craters. Route *b b*, from Galtalœkr to the summi Hekla. The dotted line between Valaknukr and Krakatinkr shows the position and extent of the lava field of 1878.

Scale $\frac{1}{240,000}$. ¼ ½ 1 × 2 Miles Danish.

bed of lava, which, although very rough and jagged, was not difficult to cross, on account of its covering of moss. At the edge of this bed we came upon a small tract of old snow, covered with volcanic ash to the depth of about eight inches. Then we reached the new lava and saw the largest cone of the new craters immediately in front of us, and about half-a-mile distant. The new lava was more rough and cindery, at least at the surface, than any I have ever seen around Etna or Vesuvius. Its surface was extremely jagged, and it was broken into ridges, and crests, and sharp pinnacles, which yielded uncertain support to the foot or hand. Hence it was both difficult and dangerous to cross, and we could only climb over a few dozen yards of it. Standing upon its surface we could see the cone of the largest of the new craters emitting small quantities of vapour and incrusted with red and yellow substances. The lava field extended both to our right and left, and in some places it was giving off vapour. The edge of the field along which we travelled was about twenty feet in depth, but in some places it was piled to a much greater height. The lava was very close and compact some distance from the exterior; it was cracked and split in all directions. White incrustations of salt (*not* chloride of ammonium, which is common in some lavas) were often noticed at the points of fracture; also brilliant red and yellow incrustations, which are often mistaken for sulphur, but which in the case of this lava, at least, I have proved to be *sesquichloride of iron.* In some parts of the lava these incrustations covered many square yards, while fumes of hydrochloric acid were freely emitted in their vicinity. The sesquichloride when removed from the lava rapidly absorbed moisture from the air and formed an intensely acid solution. We left the neighbourhood of the new crater at 2.30 P.M., rested at 4 P.M. on the banks of the Vestri-Rángá, and reached Galtalœkr at 6.15 P.M., having left it eleven hours before, and travelled over nearly fifty miles of volcanic country.

Two short accounts of the eruption have been published in Icelandic, the one by Prof. Hallgrimson, the other by Herr Nielsens. These have been translated for me by Herr Matthias Jockumsson, of Reykjavik, to whom I beg to express my acknowledgments. According to Hallgrimson there are fourteen small craters situated in a line passing from west-south-west to east-north-east, which line, if prolonged, would pass through the centre of the craters on the summit of Hekla. The distance between the extreme craters of February 27 is about 6,000 feet. The lava flowed more than a mile to the south of the largest of the new craters, but the main stream flowed to the north and north-east, and nearly reached the Mountain Valaknúkr.

Nielsens approached the scene of the eruption, a month after its commencement, from the north-west, and passing between Valafell and Valaknukr, he went due south to Krakatinkr, along the eastern edge of the new lava. From the summit of Krakatinkr he looked down into the principal crater, and saw that it opened towards the east, and that its sides were almost perpendicular. The lava within it was seen to be agitated by a wavy motion like the billows of the sea, and a stream of glowing lava was moving towards the south. A good deal of vapour was apparent, and loud noises, as of the roaring of the sea. The valley is most steep, in a line which passes north-west and south-east from Raudfossafyall; here, consequently, was the greatest depth of lava. The stream varies from 10 to 100 feet in thickness, and in some places it has burrowed beneath the snow and ice. The lava is almost black in colour, and closely resembles the old Hekla lavas in texture; as it cooled it split up with great noise and commotion.

Nielsens again visited the neighbourhood of the new crater on June 9. The lava had not increased in quantity since the beginning of April, but columns of vapour still arose from it, and small quantities of ashes and of pumice had fallen during April and May. He crossed the warm lava and managed to ascend the largest crater, which was found to be 90 feet in depth and about 100 feet in circumference.

Two days after our visit to the scene of the eruption of February 27 we ascended Mount Hekla from the south-west. The general details of the ascent I now copy from my journal, hoping at some future time to produce them in a less desultory form, and to give an account of the analysis of some of the lavas of Hekla.

We left Galtalœkr Farm at 9.40 A.M., taking with us G. Zöega, a guide from Reykjavik, together with the occupant of the Nœfholt Farm, on the flanks of Hekla. (Route *b b* on the map). Having crossed the Vestri-Rángá, we proceeded nearly due south, passed the Nœfholt Farm, near which we came upon the lava of 1845; and then passed over some very rough lava-strewn ground covered with volcanic ash, which concealed holes into which the ponies sometimes stumbled and fell. The lava of 1845 is covered with the same moss which we noticed on the old lava near the crater of 1878, which causes it to look much older. When the ascent became greater, we left the ponies. Distance to foot of steep incline about seven miles from Galtalœkr. Followed the southern boundary of the lava-field of 1845, until we reached a steep incline which we ascended. Crossed a small portion of the old lava field, then a tract covered with volcanic ashes, and finally found ourselves at the bottom of a steep slope covered with snow, beneath which water was heard rushing downwards. The ascent of this slope without alpenstocks was not easy. Several other snow slopes were crossed, and we then found ourselves near the crater of 1845—the most westerly of the four craters on the summit of Hekla. Above this we saw a crater with a red smoking mound within it; then in succession the third and fourth craters, and beyond the most easterly crater a nearly level snow-covered waste full of lava blocks. From the most easterly extremity of this, which was reached at 2.30 P.M., we looked down upon the principal crater of 1878. The summit of Hekla is covered with much ash, sand, and red pumice, together with lavas of every degree of compactness, from the most vesicular up to obsidian. The descent over the snow-slopes was troublesome, on account of their steepness and the slipperiness of the snow, but we regained the spot at which we had left the ponies at 5 P.M.

Hekla was ascended for the first time in the year 1770. The difficulties of the ascent have been much exaggerated subsequently. Many travellers have made no attempt to scale the mountain. Even the "American in Iceland," who was in the neighbourhood of Hekla only three years ago, asserts that "the obstacles, real and imaginary, stated to be in the way, such as tortuous and difficult paths, swollen rivers, depth of snow, treacherous bogs, and the evident indisposition of the guides, perhaps from superstitious fears, to make the attempt, compelled us to turn our backs upon this snowy monarch of Iceland." Other writers speak of the "fearful precipices" and of places where "a slip would be to roll to destruction."

The fact is, it is quite an easy mountain to climb, in witness whereof I may mention the very abnormal conditions under which I ascended it. The day preceding our ascent had been extremely wet, and there was at least a possibility of a downpour on the day itself; moreover, there were streams to ford, and I had consequently put on waterproof wading stockings, and the usual accompaniment of thick iron-clad wading clogs, which, although the most cumbrous things in the world to walk in, are well suited for riding in the humid climate of Iceland. If the steepness and length of the ascent had required it, of course I should have left these encumbrances with the ponies, but it was quite unnecessary, and I accomplished the ascent without difficulty in this unwieldy equipment. Surely a mountain of this height was never before ascended in clogs and waders, and the fact of its being possible to do so without inconvenience, is a sufficient answer to those who would make the ascent of Hekla a dangerous and difficult feat.

G. F. RODWELL

NOTES

THE moderation in tone of Viscount Cardwell's short address at the opening of Owens College, makes his estimate of the position of that institution all the more worthy of acceptance. He is inclined to regard it as the most singular acquisition to the academical strength of the county since the middle ages; and we venture to think that one chief reason why it occupies so distinguished a position is the completeness, the all-roundness of the education which it furnishes. There, science and letters are on an equal footing, and it will only be when this is the case in all our educational institutions that the educational apparatus of the country will be thoroughly efficient. The fact that Owens College now administers funds exceeding 400,000*l*., all obtained from private sources, is one proof that it fills a great want. We have on previous occasions spoken of its claims to be erected into a university. The tone of Lord Carnarvon's address at Saltaire, was in effect similar to that of Lord Cardwell; education is incomplete unless all our faculties have equally fair play.

ACCORDING to the Naples correspondent of the *Times*, Prof. Palmieri does not think there is any likelihood of a violent eruption of Vesuvius. The mountain has been in an eruptive state for two years; the cup is full, and must run over, but will probably do so quietly. The state of Vesuvius gives some interest to the project of the construction of a railway intended to conduct travellers from Naples to the margin of the crater. The scheme, proposed by a Neapolitan banker, has just been adopted.

According to the *Italie*, the waggons will be dragged by a rope. The length of the way will be only 840 metres, and the altitude reached 490 metres above the level of the sea. Each waggon will have only four seats, and four waggons, carrying sixteen passengers, will go up at the same time that four others loaded with sixteen persons will come down. Each waggon will be supplied with a patent brake for stopping instantly if the rope breaks.

THE death is announced, at Milan, of Signor Giulio Curioni, a distinguished chemist, metallurgist, and geologist, in his 82nd year.

MR. EDISON, the *Polytechnic Review* states, has made some experiments with a view to accomplishing the reading of the phonograph record by sight; but although a fundamental form exists for each articulated sound, he finds it very difficult to free the record from what might be called accidental influences; for so sensitive are these markings that the same sound uttered by different persons, or the manner in which it is spoken, the distance of the mouth from the instrument, the force with which it is spoken, or the speed with which the barrel is rotated, gives a different form of indentation. Vowel sounds appear to be but little affected by these variations as compared with consonants.

ABOUT 14,000 telephones, we learn from the *Polytechnic Review*, had been introduced into the United States up to the close of the year 1877, and the manufacturers are receiving orders at the rate of about a thousand a month.

WE learn from the Report of the British Association Committee on Erratic Blocks, presented by Rev. H. W. Crosskey, that the Committee were supplied with notes on boulders near Kendal by Mr. J. R. Dakyng. The most remarkable are those of the granite of Wastdale Crag, near Shap Wells, the distribution of which seems to show that they must have travelled over the high ground south of the granitic area, and not have followed the course of the present drainage. The general due south course of the boulders is shown by their distribution south of Kendal. They have been traced as far south as Milnthrops and occupy a narrow band of country whose long axis point, directly to the granite of Shap Fells. The most westerly are some near Hincaster, and a line drawn from the most westerly outcrop of granite on Shap Fells to these boulders bears south by west. The most easterly in this neighbourhood is in a field near Windy Hill, about two miles south-east of Kendal railway station, but one has been found high up on the side of Grayrigg Fell. Boulders of the dark compact altered rock that surrounds the granitic area are generally found along with the granite boulders. When the localities where granite boulders occur are marked on a map, the steady lineal north and south direction of their course is very striking. Boulders of the ordinary volcanic rocks of the Lake Mountains indicate other directions for the ice-flows. One of these may be seen two and a half miles out of Kendal, and east of the line of granite boulders. As the granitic area of Shap Fells is at the extreme east end of the volcanic rocks, this boulder must have crossed the line of flow along which the granite boulders travelled. Some new facts were reported by Mr. MacIntosh relative to the derivation of boulders already recorded, the existence of several large boulders previously unnoted, and by the extent to which Ireland has sent erratics into England. The report further contained descriptions of the position and character of many large boulders and groups of boulders in Leicestershire and Staffordshire.

IT is announced that a second session of the International Congress of Ethnographical Science which met under the patronage of the French government in July last at the Palace of the Trocadéro, will be held on October 10 next at the Palace of the Tuileries. This session is to be held in order that those who were unable to attend in Paris in July last may now have an opportunity of being present at the congress. In the course of a few days a programme will be published of the questions to which special attention is to be called. The committee of the Ethnographical Society of Paris which is organising the congress are especially desirous that England should be well represented on the occasion.

IN a recent issue of the *Indian Tea Gazette* we find some interesting notes of an attempted journey towards China, overland from Assam, extracted from the journal of Mr. C. H. Lepper, who, attracted to the subject, no doubt, by his previous residence in China and Japan, has always kept in view, since his arrival in Assam, the idea of taking advantage of the magnificent Brahmapootra as a road to within about 200 miles of the frontier of the province of Yünnan. His residence being only sixteen miles from Suddya, the furthest outpost on the north-east of India, he has had constant opportunities of making inquiries from the intervening peoples in regard to the distance, state of the tribes, &c., between our frontier and China. These proved so satisfactory in their results that early in 1876 he was induced to make an attempt to advance as far as possible in the direction of China with the object of obtaining more precise and practical information. Before the party left Suddya, the *mehla* or fair for the hill tribes had just been held, at which Chow Mang Ti, a chief of the Kamptis, acts as interpreter for that tribe and the Singfoos, who assemble there in considerable numbers. His brother, Chow Kun, accompanied Mr. Lepper as head of the Kampti guides, and in the course of conversation Chow Mang Ti stated that there would be no difficulty in getting through the intervening country to the Chinese border. Passing up the Brahmapootra, the party entered the Tenga Pani River through the Noa Dehing. They found the first three or four miles had a sandy bottom, with great quantities of the silkworm-feeding *Soom* tree on the banks, on which the forest soon became dense and the scenery beautiful. Near the first rapid is a pretty islet, named Shikar Mazeli, which, when seen from a point a little further up the stream, Mr. Lepper describes as "perfectly enchanting." After passing the village of Juna or Chuna, Mr. Lepper says "we soon came to a bend, having a very high forest facing us on a hill, containing banyan, valuable saul trees in great quantities, also uriam and soom trees; we noticed, too, the Bazal bamboo appeared again, bamboos up to within a short distance of this hill not having been seen on the river. The 'surat,' a horrid leaf, which, if touched, causes intense pain for days, is prevalent, also a kind of wild chestnut. Plantains, *toku pat*, *tora pat*, tree ferns, orchids, and other parasites, all help together with graceful patches of cane to make the picture perfect." Owing to numerous rapids progress was slow, and it was late on the fourth day when the party reached Shangkam, where they were kindly received by the Kamptis. After they had waited some time, Chow Mang Ti arrived from Suddya, and to their great disappointment assured them that it would be useless to attempt to proceed towards China at that time of the year, as the rainy season had set in. He, however, gave some information on points connected with the route to China. "It takes eight days," he said, "to reach Hobong, between which place and this you could get no supplies, and it would take you about sixteen days to get there. Hobong is a large town. Then it takes us eight days more to reach the Irrawaddy River, and would take you at least sixteen or eighteen days. It is between Hobong and the Irrawaddy that the ascent I told you of has to be made. A little past Hobong are the two hills of almost pure silver; . . . there is a great quantity of gold there too, which gives that little colony of ours the name of Kamp Thi, meaning gold country, by which name you call all of us, who are really Shans from Shan, a country lying nearer China,

having a strip of Singfoo country lying between us and our colony of Kamp Thi. The Singfoos, again, lie between our Shan country and China. This Hobong route is better than the Hukong route, which leads to Burmah, as the Noa Dehing is not navigable now." As it was impossible to prosecute his projected journey without Chow Mang Ti's assistance, Mr. Lepper deemed it prudent to acknowledge the weight of the arguments which he advanced against making the attempt at that season, the more so as he was aware that the Government of India objected to Europeans crossing into the territories of the hill-tribes, and he had only obtained permission to cross the "inner line" on the ground that he desired to visit the Brahma Khund or Sacred Pool of Brahma. He accordingly descended the Tenga Pani River, and then proceeding up the Brahmapootra, visited the Sacred Pool, a spot which few living Europeans have ever reached.

FROM time to time we have given various examples of the progressive tendencies of the Japanese in many directions, and we are glad to be able to supplement them with the information that they are turning their attention to the dredging of their harbours. We learn from a Japanese contemporary that the ports of Niigata, Ishinomaki, and others are to be dredged and improved in such a way as to be most convenient for shipping. The Bay of Hachiro, in the province of Ugo, is to be dredged, at the estimated cost of a million *yen*. Mr. John Perry, engineer in the employ of the Public Works Department, and a number of students of the Survey Department were sent there in April in order to survey the bay, and their inspection will probably be completed in October. This work is the more important as there is no safe shelter for vessels in stormy weather between Hakodate and Tsuruga.

MR. EDWARD STANFORD has the following books and maps preparing for publication:—A fifth edition, greatly enlarged, of "The Physical Geology and Geography of Great Britain," by Alexander C. Ramsay, LL.D., F.R.S., Director-General of the Geological Surveys of the United Kingdom, with Geological Map printed in Colours, and numerous Illustrations; "Notes of a Tour in America from August 7 to November 17, 1877," by H. Hussey Vivian, M.P., F.G.S., with Map; "The Fairy-land of Science: Chapters for Children," by Arabella B. Buckley, Illustrated; "Karamania; or, Life in Asiatic Turkey: a Journal of Travel in Cilicia (Pedias and Trachæa), Isauria, and Parts of Lycaonia and Cappadocia," by the Rev. E. J. Davis, M.A., English Episcopal Chaplain, Alexandria, Illustrated; "Manual of Physical, Historical, and Political Geography for Schools," by Keith Johnston, F.R.G.S., with numerous Maps and Illustrations; "Stanford's Compendium of Geography and Travel—Europe," Edited and Extended by Prof. A. Ramsay, F.R.S.; "Australasia," Edited and Extended by A. R. Wallace, F.R.G.S.; also in preparation, uniform with the last, "Asia," Edited and Extended by Col. Henry Yule, C.B., F.R.G.S., "North America," Edited and Extended by Prof. F. V. Hayden, of the United States Geological Survey; "Elementary Physics for Middle-Class Schools, with Experiments and Illustrations, to which are added Examination Questions on each Section," by John A. Bower, Science Master, Middle Class School, Cowper Street, London. Among Maps Mr. Stanford will publish an "Orographical Map of Asia," for Use in Schools and Colleges," Edited by Prof. Alexander C. Ramsay, F.R.S.; "Library Map of Japan," Compiled by E. Knipping; size, 4 feet 6 inches by 5 feet 6 inches; scale 17 miles to an inch; this is an entirely new and original map, compiled from the various large divisional Maps prepared by the Japanese, and corrected and extended from journeys made for the purpose by the author; Supplementary maps show the railways, chief roads, telegraphs, and lighthouses, and the new administrative divisions introduced in 1876; "Library Map of Africa," new edition, with the Results of all Recent Explorations carefully laid down; "School Map of Africa;" new edition; size, 50 inches by 58; scale 118 miles to an inch; "Library Map of South America;" new edition; "Library Map of Australia," in nine sheets; constructed from the most recent Official Documents furnished by the Surveyors-General, showing the Details of Recent Explorations, and including a supplementary Map of Tasmania; on the same scale; "General Map of Australia," with all the recent explorations, the roads, railways; and a "School Map of New Zealand."

MESSRS. E. AND F. N. SPON will publish during the forthcoming season:—"The Chemist's Pocket-Book," by Thos. Bayley; "The Power and Speed of Steam Vessels Calculated by Rules adapted for Vessels of all Types," by W. Bury, Mem. Inst. M.E.; "A Treatise on Bridge and Roof Construction," by Karl von Ritter, translated from the German by Lieut. Sankey, R.E.; "Applied Mechanics," by Prof. Calcott Reilly, M. Inst. C.E.; "Graphic Arithmetic," by G. S. Clarke, Instructor in Geometrical Drawing, Royal Indian Engineering College, Cooper's Hill; and a "Supplement to Spon's Dictionary of Engineering." M. Ch. du Moncel has published in the "Bibliothèque des Merveilles" a volume on "The Telephone, Microphone, and Phonograph," in which he describes in a popular form all the discoveries which for the last two years have astonished the world.

MESSRS. WILLIAMS AND NORGATE have sent us "Karl Ernst von Baer; eine biographische Skizze," by Dr. Ludwig Stieda, and "Karl Freidrich Gauss, Zwölf Kapitel aus seinem Leben," by Ludwig Hänselmann. Messrs. Longmans and Co. have published a fourth edition of Prof. Fleeming Jenkin's text-book on "Electricity and Magnetism."

FROM America we have received the "Bibliography of North American Palæontology," by Dr. White and Prof. Alleyne Nicholson (Washington Government Printing Office), and "Annals of the Astronomical Observatory of Harvard College," vol. iv. part 2 (Cambridge, U.S.: Wilson and Son). From Australia we have Decade 5 of the "Prodromus of the Palæontology of Victoria," by Mr. F. McCoy, of the Victoria Geological Survey; "Meteorological Observations made at Adelaide Observatory during 1878," by Mr. Charles Todd; "Mineral Statistics of Victoria for 1877," and "Report of the Mining Surveyors for Victoria for Quarter ended March 31, 1878."

THE Chilian government has now established a meteorological service in all the ports that are connected by telegraph, and the daily observations are published in the government gazette at Santiago.

AN earthquake was felt at Pic-du-Midi Observatory on Wednesday, September 25, at 8h. 53m. in the morning. The snow was falling heavily. Heavy falls of snow have also been recorded in the Oberland, and communications with St. Gothard have been interrupted. Shortly after midnight on Friday the shock of an earthquake was felt at Osterath, in Germany.

FROM the 16th to the 18th of August the city of Cartago in Costa Rica, in Central America, was visited by five distinct shocks of earthquake. No particular damage was caused. What is to be noticed is the meteorological result: that the weather was changed, being attended by heavy showers that flooded the rivers. In all earthquake countries it is believed the weather is affected by such phenomena, and it is desirable that observations should be recorded, showing the influence of earthquakes on weather. It has been alleged that the Comrie earthquakes in Perthshire came on after rains and floods of the river.

PROF. KARL ARENDTS, of Munich, has been nominated corresponding member of the Lisbon Geographical Society, in acknowledgment of his merits in geography. Prof. Arendts is the editor of a new geographical serial, entitled "Deutsche Rundschau für Geographie und Statistik," published in monthly parts by Hartleben, of Vienna.

"VIH CHE SIN LOO" (magazine for the propagation of knowledge) is the title of a new monthly publication in the Chinese language, edited by Dr. Edkins, of Pekin.

SECCHI'S "Meteorologica Romana" has just been published by the Italian government. The last revision of proofs was made by Secchi's friend, Sig. Ferrari, death having prevented the great astronomer himself from finishing this last part of his work on the meteorology and climatology of the Eternal City.

THE twenty-third meeting of German and Austrian apiculturists took place at Greifswald on September 11-13, and was attended by over 700 persons.

THE International Agreement regarding the steps to be taken for protecting the wine-growing districts against phylloxera, was signed at Berne on September 17.

RECENT borings made in different parts of North Germany have proved beyond denial that the assertion made by several eminent geologists, that a mighty deposit of salt stretches from the Lüneburger Heide to the coast of the Baltic, is perfectly correct. The deposit begins near Lüneburg, passes underneath the Elbe, and extends right across the Grand Duchy of Mecklenburg. Another branch goes in the direction of the Duchy of Holstein, *viâ* Legeberg to Elmshorn and Heide. Borings made at Lübtheen, near Hagenow, by order of the Mecklenburg government, have now reached a depth of 456 metres, and the thickness of the deposit of salt now reaches 130 metres; the basis, however, is not yet reached.

THE *Palæontologist* is the name of a new journal of small size published in Cincinnati, devoted to the subject indicated by its title. It will be published from time to time as matter accumulates.

AMONG the many rare shells lately belonging to Dr. Marie, of New Caledonia, is a new Murex, which will be described by Mr. Bryce Wright in the next number of the *Proceedings* of the Malacological Society of Belgium, under the name of *Murex huttonæ*.

STUDENTS of physics will be glad to learn that Gauthier-Villars has just issued a complete collection of the papers of the lamented Léon Foucault. They are preceded by a notice from the pen of M. Lissajous.

RECENT numbers of *Globus* contains an account, by Dr. Schröder, of his second visit to Cyprus in 1873.

HERR GERHARD ROHLFS will start for a new exploring tour to Africa in the first week of October. He will be absent some eighteen months, and he begins his journey *viâ* Tripolis and Wadai.

IN the Barmsee, a small lake situated in the Bavarian mountains, near the villages of Mittenwald and Krün, not far from the Austrian frontier, numerous piles, dating from pre-historic lake-dwellings, have just been discovered by Herr Zapf. The massive piles are standing upright in the lake; some of them still show incisions and spikes on their surface, indicating the spots where they were united, or where planks rested upon them. They stand in rows parallel to the southern shore of the lake. Other rows run in a northerly direction, but in the latter the piles are smaller and stand close side by side, forming a sort of palisade. None of the piles now reach the surface of the water. The total length of these pile-dwellings is about 200 metres.

THE *Precurseur d'Anvers* publishes a letter from Zanzibar stating that the Belgian expedition had reached a place situated some distance in the interior. Owing to a misunderstanding, they were compelled to engage in conflict with the natives, and eventually had to seek refuge in flight. It is not yet known whether they succeeded in escaping or were murdered.

AN American Microscopical Congress met at Indianapolis on August 14–17, under the presidency of Dr. R. H. Ward. Nineteen papers in various departments of microscopical research were read. The congress resolved itself into a permanent organisation under the name of the American Society of Microscopists.

THE Christchurch (N.Z.) *Press* contains a description of a very complete physical, chemical, and metallurgical laboratory which has been established in connection with Canterbury College. It is well provided with the best apparatus, and is well fitted both for original research and for student work.

WE have received a neat and attractive little "Guide to the Upper Engadine," by Dr. J. Pernesch, published by Orell Füssli and Co., of Zurich. It contains useful and well-arranged information, with a small map and twenty-one well-executed illustrations.

WE have received the July number of the *Anales* of the Argentine Scientific Society, which, besides an account of the proceedings of the society, contains a narrative of journeys in Patagonia, by Ramon Lista, papers on the composition of the water of the Uruguay, by Mr. J. J. Kyle, on the Hemiptera Argentina, by Carlos Bery, and on the flora of Paraguay, by Domingo Parodi.

WE are asked to publish the following statement:—During the recent opening of the Grosvenor Gallery on Sunday, nearly 6,000 persons availed themselves of the privilege of visiting the exhibition. This success has encouraged the committee of the Sunday Society to arrange for the exhibition of a series of water colour drawings of Swiss life and scenery, sketched in Switzerland by Mr. William L. Thomas. The exhibition will be opened on the first three Sundays in October, from 2 to 5 P.M., at 33, New Bridge Street, Blackfriars, E.C. (the late offices of the London School Board), and the public will have free access to it by means of tickets, which will only be issued to those applying by letter, and sending stamped and addressed envelope to the Honorary Secretary, 19, Charing Cross, S.W. The Committee of the Sunday Society intend to continue these Sunday Art Exhibitions from time to time, until the Society's object is accomplished, and the public Museums, Art Galleries, and Libraries, are all opened on the people's weekly leisure day. The Society will be glad to receive pecuniary and other assistance to enable the Committee effectively to carry on these exhibitions.

THE separation of the Central Bureau of Meteorology of the French Government is an accomplished fact, and the officers will be located in an hotel in the Rue de Grenelle, St. Germain, where no readings can be taken, the inspection of the sky being as difficult as from the British Office in Victoria Street. These peculiarities have raised much sharp criticism, and it is not certain that this solution will be accepted by the French Legislative Assembly. It has been suggested that the Bureau should be located at the Trocadéro, or on the top of the Arc de Triomphe, where there are magnificent rooms in which offices could be established.

THE additions to the Zoological Society's Gardens during the past week include a Bonnet Monkey (*Macacus radiatus*) from India, presented by Mr. Frederick Carter; two White-crested Touracous (*Corythaix albocristata*) from South Africa, presented by Mr. W. Wormald; a Green Turtle (*Chelone viridis*) from the Island of Ascension, presented by Capt. J. Smith; a Tabuan

Parrakeet (*Pyrrhulopsis tabuan*), a Masked Parrakeet (*Pyrrhulopsis personata*) from the Fiji Islands, purchased; a Chestnut-backed Weaver-bird (*Hyphantornis castaneo-fusca*), four Rufous-necked Weaver-birds (*Hyphantornis textor*), two Grenadier Weaver-birds (*Euplectes oryx*), a Barbary Turtledove (*Turtur risorius*), a Vinaceous Turtledove (*Turtur vinaceus*) from West Africa, a Turquosine Parrakeet (*Euphema pulchella*) from New South Wales, two Undulated Grass Parrakeets (*Melopsittacus undulata*), seven Crested Ground Parrakeets (*Calopsitta novæ-hollandiæ*) from Australia, deposited; eight Mocassin Snakes (*Tropidonotus fasciatus*), born in the Gardens.

THE FIGURE AND SIZE OF THE EARTH[1]

III.

DURING the years 1816-52 a Russo-Scandinavian degree-measurement was carried out by Struve and Gen. Tenner, of unusual length and wonderful accuracy. It extended from Hammerfest in the north (70° 40′) to Ismail in the south (45° 20′), a length of 25° 20′. From this it followed that for latitude 56° 3′ 56″, the degree length is 57,137 toises, and in this was included a Swedish degree-measurement which gave 57,209 toises as the length of a degree in lat. 66° 20′ 12″. At this time extensions of earlier operations were undertaken in other countries; as in England with the result of $\frac{1}{280}$ for the earth's oblateness. Everest extended Lambton's East Indian degree-measurement, which at present has a length of over twenty-one degrees. Also a longitude measurement begun at an earlier period in Central Europe was recommenced and reaches now from Brest, Paris, Strassburg, Munich, to Vienna. With the assistance of the new data the amount of the earth's oblateness was again investigated, and now that there was no doubt of the accuracy of the measurements, it was shown distinctly that the earth was not an entirely regular elliptical spheroid, that the flattening did not pass regularly over the earth's surface. The theory was next propounded that the earth was an ellipsoid of three axes, but the proposition was not fully supported by the measurements.[2] A newly elaborated mathematical method, by which from the existing measurements, the figure of the earth could be obtained, and by which the remaining errors could be reduced to a minimum, was now applied by the great Bessel to the data before him, and according to these principles and on the basis of the best measurements, he obtained dimensions of the earth which still form the ground of all astronomical and geodetical calculations, and which are as follows:—He found for the equatorial radius of the earth 327207714 toises = 6377400 metres; for the polar radius 326113933 toises = 6356080 metres; and for the length of the earth's quadrant 10000855765 metres; while he gave the earth's oblateness as $\frac{1}{298 \cdot 153}$. He also deduced formulæ giving the length of a degree or of a parallel of latitude for any part of the earth; these formulæ will be found in most geodetic handbooks. New values for the dimensions of the earth were, ten years ago, deduced by Leverrier with the assistance of other measurements, but they differ very little from those of Bessel, and in almost all scientific works Bessel's constants are adopted. Thus, about the middle of the present century the solution of the original problem has been attained. And yet since that time there has unmistakably been an altogether unusual progress in all departments concerned in the solution of the problem. First of all, mechanical precision has in the last decades attained such perfection, that now astronomical observations, and especially the geographical determination of places, can be made with considerably greater accuracy than was possible a few decades ago. Moreover, these improvements have been accompanied with extraordinarily suitable new methods. The telegraphic determination of longitude, a method for the determination of the lengths of parallels, has above all, especially in the last ten years, attained such perfection, theoretically and practically, that the measurements carried out on this method are most accurate, and we may justly expect in the future most brilliant results from it. Altogether the problem is near a more substantial solution now than it ever was before, as there are so many well-equipped observatories at work, bound together by a network of the most accurate determination of longitude and latitude, extended over the surface of the earth, and the completion of which is yet going on. Of the greatest importance also in this connection is the more recent theory of measurement, according to which the points to be connected need not lie in one and the same meridian or parallel, but, on the contrary, are connected by an arc of a great circle lying in any direction—the geodetic line.

Thus have we seen how from the remotest times, among all the nations of the earth not only has the wish to obtain a knowledge of the form and size of our dwelling-place prevailed, but also the most earnest activity in attaining this knowledge of nature; how this has in an increasing degree gone hand in hand with the progress of the mathematical and physical sciences, having at last reached a worthy solution in our time; and we have the best reasons for believing that, closely following the progress of the sciences, our knowledge in this connection will soon reach the highest degree of certainty.

[1] Continued from p. 580.

[2] This is scarcely correct: the figure of three unequal axes agrees better with the observations than does the spheroid of revolution. But there is a necessity for this, and the ellipsoidal figure cannot be regarded as established.

Remarks on Karl Maria Friederici's Paper, by Col. Clarke, R.E.

One feature of modern geodesy that has been left unnoticed by the writer of the foregoing paper is the application of the theory of probabilities, under the form of the "method of least squares," to the reduction of the observations. In any network of triangulation if it were required to obtain the distance apart of two of the extreme points, we should obtain varying results according to the set of triangles used in the calculation, and the question arises, What is the degree of credibility to be given to each result, or what the "probable error" of each? This implies, of course, that there is a superfluity of observations, that is, more than are absolutely necessary to fix all the points. If there were no superfluous observations there could be but one result obtained—an advantage in one respect (in saving calculation)—but then we should be wholly at sea as to the degree of trust to be placed in this one result. The theory of probabilities teaches us how to treat superfluous observations, and practically it leads to the calculation of a system of corrections to all the observations in order to bring them into harmony. Now there could be found an infinite variety of systems of corrections that would harmonise the observations, but the particular system we are led to by the method of least squares has this feature, that the sum of the squares of the corrections is an absolute minimum. In plain words, the triangulation is harmonised with the least possible alteration of the original observations as a whole.

These calculations, however, involve an enormous amount of labour, and one is compelled sometimes to relax the rigour of the theory, and accept some slight modifications. For example, in the triangulation of Great Britain and Ireland this method required the solution of an equation of 920 unknown quantities; this hopeless task was evaded by breaking up the triangulation into some fifteen or more parts.

The principal triangulation of India—unlike that which uniformly covers the face of this country—consists of chains of triangles running in a meridian direction crossed by several other chains perpendicular to them; as the longitudinal series from Kurrachee to Calcutta, and that from Bombay to Vizagapatam, the north-east longitudinal series, and others. The reduction of these chains of triangles by the method of least squares is a vast labour not yet completed.

The enormous mass of the Himalayan Mountains causes, or rather might theoretically be supposed to cause, an immense disturbance of the direction of gravity at the stations of the Indian arc amounting at the northern stations to 30″, with smaller amounts as the distances of stations southwards increases. Now it is very singular that, except at the very foot of the mountains, these discrepancies do not actually appear (to anything like that amount) in the arc itself or when it is used in the problem of the figure of the earth. This very remarkable circumstance has led to the hypothesis that the attraction of the superincumbent visible mass must be compensated for by a diminution of density in the strata underlying the mountains. Pendulum observations made for the purpose of elucidating this point have shown conclusively that the density of these underlying strata *is* actually less there than elsewhere. A compensation therefore takes place—but in a general way only; it cannot be shown that it is a mathematically exact compensation, consequently there is some residual doubt as to what really are the deflections of the vertical at the astronomical stations of the Indian arc.

We may supplement Karl Maria Friederici's account of how to calculate a meridian arc by a description of the method fol-

lowed in calculating a portion of the English arc where proceeding from the Yorkshire coast between Redcar and Whitby (the survey station is Easington) it crosses the sea in a line to Saxaford in Shetland. At Easington astronomical observations gave the azimuth of Cheviot as $38° 48' 58''\cdot68$. This determines the meridian; and the triangulation gives the relative distances and azimuths at the following succession of stations (selected for this calculation):—Cheviot; Mount Battock, on the Grampians between Aberdeen and Balmoral; Scarabin in Caithness, Fitty Hill in the Orkneys; Foula, a precipitous island between Orkney and Shetland; and Yell, a station in Shetland (not quite so far north as Saxaford (see Fig. 3).

On the meridian line defined above, take a point, A, whose distance from Easington is equal to the side "Easington to Cheviot." Join A with Cheviot and Mount Battock, and it is evident that we can determine the distance A to Mount Battock and the angle at A between Mount Battock and the north meridian. Next take the point D at a distance from A equal to the side "A to Mount Battock." Join D with Mount Battock and Scarabin, then we can determine the side "D to Scarabin" and its inclination to the meridian at D. Next take a point G (still in the same meridian) at a distance from D equal to the side "D to Scarabin," and join G with Scarabin, Fitty Hill, and Foula. Next take H at a distance from G equal to the side "G to Foula," and join H with Foula, Yell, and Saxaford. From Saxaford drop a perpendicular on the meridian meeting it in S; then we have the distance Easington to S, and the length of this perpendicular, which is about 200 feet. In order to verify this result a different set of stations were chosen for a second calculation conducted in a similar manner; the distance Easington to S by the first calculation was 2288427·29 feet; by the second it was 2288427·38 feet.

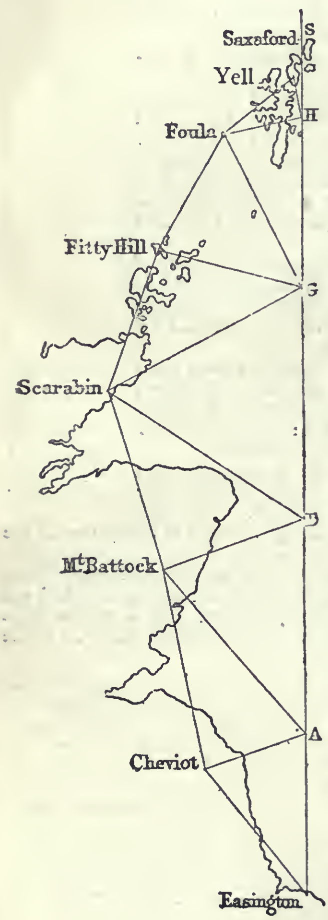


Fig. 3.

The volume of the Ordnance Survey entitled "Account of the Principal Triangulation of Great Britain and Ireland," by Capt. A. R. Clarke, R.E., contains (pp. 733–778) an elaborate calculation of the figure of the earth based upon the English and French arcs of eleven and twelve degrees respectively, the Russian and Indian of twenty-five degrees and twenty-one respectively, the first Indian arc, the Danish and the Prussian arcs of about a degree and a half each, the Peruvian arc of three degrees, and the Hanoverian arc of two degrees—a total length of eighty degrees within a few minutes. The figure is first investigated without restricting it to the elliptic form, but assuming that the radius of curvature of the meridian is expressed by the formula $\rho = A + 2B\cos 2\phi + 2C\cos 4\phi$ (the ellipse is a particular case of this curve, *i.e.*, if $5B^2 - 6AC = 0$). The resulting semi-axes are $a = 20927197$ ft., $b = 20855493$ ft., $a : b = 291\cdot9 : 290\cdot9$, and the meridian curve is more protuberant than an ellipse of the same axes by the quantity $\delta r = (177\cdot5 \pm 70\cdot9)\sin^2 2\phi$. But when the curve is restricted to the form of an ellipse the semi-axes are found to be 20926348 and 20855233 with the ratio of 294·26 : 293·26. The probable errors of the semi-axes so determined are ± 186 and ± 239 respectively.

Subsequently to the publication of the volume containing these investigations, an extensive series of comparisons of standards—the geodetic standards—of length of the various countries in which meridian arcs have been measured, was made at the Ordnance Survey Office, Southampton. In order that these most important comparisons might be made with the utmost attainable accuracy, a building was erected especially for this purpose. The comparison-room—itself double-walled—is surrounded and wholly inclosed within an outer building, and it is partly sunk below the level of the ground. The diurnal change of temperature is not perceptible in this room, consequently comparisons are made under extremely favourable circumstances. As a rule, each pair of standards was compared in summer at about the temperatures of 62°, and in winter at about 32° or 42°, but always at the natural temperature of the room, artificial temperatures being wholly excluded. To give an idea of the precision attained in these comparisons, the probable error of the length of the Ordnance Standard ten-feet bar in terms of the national yard is four ten-millionths of a yard; that of the Indian steel standard (a ten-feet bar) is less than three ten-millionths of a yard; that of the Russian standard, a bar of more than twelve feet long (two toises) is six ten-millionths. The importance of these results, connecting the largest arcs, upon which the figure of the earth must depend, can hardly be overestimated. The length of the "Toise of Peru" obtained through three entirely independent sources, viz., the Russian, the Prussian, and the Belgian toises, is 6·39453348 feet, from which the greatest divergence of the three separate results is only half a millionth of a toise; this corresponds to ten feet in the earth's radius. The length of the "metre" deduced from the above by means of its defining ratio (443296 : 864000) is 3·28086933 feet.

One of the most difficult of the determinations of length made in connection with this series was the measuring the distance between the knife edges of Kater's Reversible Pendulum. In this operation the flexibility of the bar was in the preliminary comparisons a source of much error. This, however, with some other troublesome matters, was successfully overcome, and the length was determined with a probable error of less than one five-millionth part of the length. The length of the pendulum was obtained by comparing it with the metre, and the details of the measurements will be published in the "Account of Pendulum Observations in India," by Col. Walker, R.E., F.R.S., Superintendent of the Great Trigonometrical Survey and Surveyor-General of India.

These results of the comparisons of standards of the different countries concerned, modify the numbers given above for the semi-axes of the earth. After making the necessary corrections, the semi-axes of the elliptic meridian are 20926062 feet and 20855121 feet, or 6378206·4 metres and 6356583·8 metres. The ratio of the axes 293·98 : 294·98. See the Ordnance Survey volume, entitled "Comparisons of Standards of Length," page 287. In the same volume will be found an investigation of the figure of the earth, supposing it to be an ellipsoid of three unequal axes.

But these results are now again superseded by a more recent determination published in the *Philosophical Magazine* for August. It appears that during the last few years the Surveyor-General of India, Col. Walker, C.B., F.R.S., Royal Engineers, has been not only measuring new arcs in India, both of latitude and of longitude, but has revised the southern portion of the Indian arc as measured originally by Col. Lambton. On this chain of triangles considerable doubt rested as to what was the unit of length used in the measure; a complete remeasurement according to modern methods has set this question at rest. A complete meridian chain of triangles has been carried from Mangalore on the west coast in latitude 12° 52′ and longitude 75° E., to a point in latitude 32°; thus the whole Indian arc is now 24° in length. Eleven differences of longitude have been determined by electro-telegraphy between the stations Mangalore, Bombay, Vizagapatam, Madras, Bangalore, Hydrabad, and Bellary; it is almost unnecessary to add that in these operations no refinements of modern science have been overlooked. As the difference of longitudes of Bombay and Vizagapatam is 10° 28′ and the geodetic connections of the above seven stations is completed (liable, however, to some small future corrections resulting from the least square calculations) there is thus presented a large addition to the data of the problem of the figure of the earth. Taking the English and French conjoined arc of 22° with fifteen astronomical stations; the Russian arc of 25° with its thirteen stations; the Indian meridian arc of 24° with

fourteen stations (this is not a fourth part of the number available), and the Indian longitude stations specified above, fifty-six equations treated by least squares give the following elements of the earth's figure :—

Polar semi-axis ... ... $c = 20854895$ standard feet.
Equatorial semi-axis ... $a = 20926202$ „ „

$$c : a = 292\cdot465 : 293\cdot465.$$

But the evidence of the Indian longitude observations goes to show that the curvature of the surface of India in a direction perpendicular to the meridian is considerably less than that belonging to the spheroid just specified. Possibly this apparent result may be owing to the existence of attractions of the plumb-line seawards at the coast stations. At any rate it suggests the re-investigation of the ellipsoidal form of the earth : and the result of a formidable calculation is that the ellipsoid best representing all the observations has the following semi-axes :—

$a = 20926629$ $b = 20925105$ $c = 20854477$,

and the ellipticities of the two principal meridians

$$\frac{1}{289\cdot5} \text{ and } \frac{1}{295\cdot8}.$$

The longitude of the greater axis of the equator is 8° 15′ W. of Greenwich—a meridian passing through Ireland and Portugal, and cutting off a portion of the north-west corner of Africa ; in the opposite hemisphere this meridian cuts off the north-eastern corner of Asia and passes through the southern island of New Zealand. The meridian containing the smaller diameter of the equator passes through Ceylon on the one side of the earth and bisects North America on the other. Thus the division of the earth by the meridian plane of the greater axis of the equator corresponds very nearly with the ordinary two-circle representation of the earth, the one showing the Eastern hemisphere the other the Western.

Such is the result of the calculation, and it is a somewhat remarkable result when considered in connection with the actual physical features of the globe, and the distribution of land and sea on its surface. But too much confidence must not be placed in it ; many more measurements would be necessary to establish this figure as a reality ; as yet it is merely indicated by the existing observations, and the amount of the eccentricity of the equator shown above is really very minute.

It is to be observed—returning to the spheroidal figure and comparing this new result with that quoted above from the volume of "Comparisons of Standards"—that the effect of the new work in India has been to increase the radius of the equator by 140 feet, and to diminish the polar radius by 226 feet.

There are several short arcs on the European continent which might have been used in addition to the long arcs, but the influence of these on the result would have been almost imperceptible. The details of the American Coast Survey oblique arc are not yet published.

Notwithstanding the immense additions to geodetical measurements and to the data of the problem of the figure of the earth since Bessel's investigations (1841), it is with a good deal of truth that Karl Maria Friederici says that Bessel's results are still universally adopted by scientific men. And this must be considered a very remarkable instance of the influence of a *name*. Bessel was a splendid mathematician ; his works are characterised by great elegance ; and in this case his fame is a set-off against the increase of data subsequent to his time. But Bessel could only use one-fourth part of the present English arc (and the terrestrial measure of this arc as used by him was some 200 feet in error), and one-third of the present Russian arc. In his time the English, Russian, and Indian arcs amounted in all to less than 27° ; now they exceed 57°. Hence Bessel's figure of the earth cannot be considered anything else than obsolete, however excellent it may have been six-and-thirty years ago.

The operations which are being conducted with so much activity on the European continent and in India must shortly put us in possession of great additions to the data of the problem, especially through the agency of the electric telegraph. As a specimen of the precision now attainable in the determination of longitudes by galvanic signals, we may quote the three results obtained at different times and in different ways for the difference of longitude of Greenwich Observatory and Harvard Observatory (Cambridge, Massachusetts). They are as follows :—

	h.	m.	s.
In 1866, by Anglo-American cables	4	44	31·00
In 1870, by French cables to Duxbury	4	44	30·99
In 1872, by French cable to St. Pierre	4	44	30·96

Doubtless there is a certain amount of good fortune in this, but nevertheless the accordance is highly satisfactory.

AN EXPERIMENTAL INVESTIGATION OF THE STRUCTURE OF FLUID COLUMNS WHICH ARE AFFECTED BY SOUND

WHEN a fluid escapes from a contracted opening, it may form a column, which throughout the greater part of its length has the same sectional shape as the opening. This kind of column may be called prismatic.

It may after leaving the opening form an expansion, this expansion being succeeded by another at an angle (usually a right angle) to it ; and this latter by another, and so on. This kind of column may be called segmental (Fig. 1).

1.

An example of the first is obtained when the column proceeds from a true cylinder, truncated at right angles to its axis, and is very difficult to obtain.

A segmental column is easily obtained from the end of a partially closed glass tube.

Segmental columns are sensitive, prismatic columns not sensitive, to sound.

The character of a jet is determined by connecting with a water supply.

The apparatus, Fig. 2, is intended to show the structure of a segmental column. *a*, *b*, *c*, *d* is a piece of cork $2\frac{1}{2}$ in. × 1 in. and 1 inch thick. Pieces of glass tube 2 inches long serve as axes for the corks C C. Each cork is pierced at right angles to its axis to support a jet J. An india-rubber T-piece connects the jets with gas or water supply.

It will be seen that the arrangement permits of the jets being inclined to each other at any angle by the movement of the corks round their axes ; and a lateral adjustment may be obtained by sliding them along their axes.

To make the jets : the middle of a long piece of glass tube is contracted in the blowpipe flame ; when cold a sharp scratch is made in the middle of the contraction, and the tube broken through. Two perfectly equal jets are thus obtained.

Experiment I.—Arrange the jets at an acute angle and connect with a water supply. The segmental column will be obtained. The first (primary) expansion being at right angles to the plane of the jets.

Experiment II.—Keeping the water supply as before, increase the angle of the jets ; the segments will become more marked and the length of the entire column decreased. At a certain angle the primary expansion becomes so great that the cohesion of the

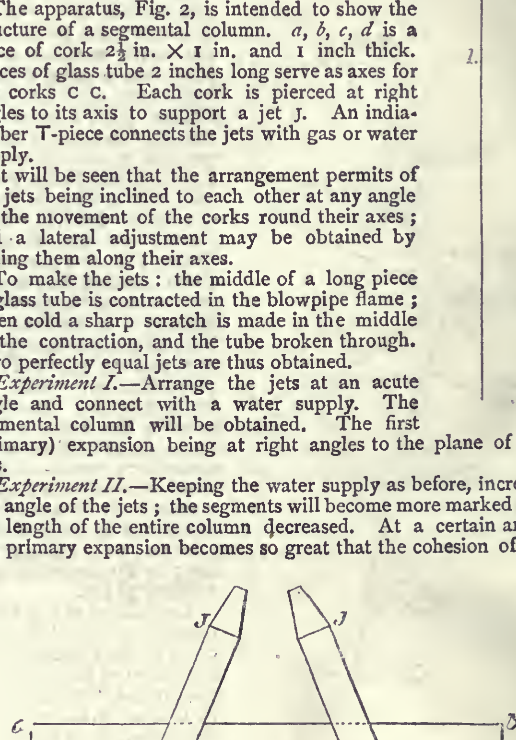


Fig. 2.

different parts will be overcome, and the column be fan-shaped.

The ordinary fish-tail burner furnishes a familiar example of this extreme segmentation.

Note.—As every degree of segmentation may be obtained with suitable inclination of the jets, it follows that a segmental column really consists of two similar ones, meeting at an angle.

Experiment III.—Arrange, as in Experiment I., but connect with a gas supply and ignite. The primary expansion will be obtained but not succeeded by any other. This is readily accounted for. The edges of the expansion, as far as the middle, are receding from each other, and there being no cohesion in gases, they continue to recede, and so the expansion is fan-shaped.

Experiment IV.—Starting with the jets at an angle of about 60° and their ends nearly in contact, lessen the angle and adjust the gas supply, that the flame may take the shape shown in Fig. 3. At a certain point increase of gas will cause the flame to emit a note (usually a very high one), and at the same time commencing somewhere about O[1] there will be an expansion at right-angles to the primary one. A horizontal section of the flame will now have a cruciform contour.


Fig. 3.

Having obtained this musical flame, withdraw the jets through their corks (keeping the same angle throughout). As the distance between them increases the note becomes of lower pitch. With careful attention a very low note may be obtained. At a certain distance the note ceases, but may be restored by additional gas pressure.

Note.—It is evident that the velocity of the gas decreases with its distance from the jet. Hence the greater the velocity the higher the note.

I have shown (NATURE, vol. xv. p. 119) that the velocity of the escaping gas is decreased by ignition, therefore if a certain flame produces a low note the same column unignited will give a higher one. This is found to be the case.

Experiment V.—Having arranged, as in the preceding experiment, reduce the gas supply till the flame ceases to sing. It will now be sensitive to a certain note. Produce this note very near the flame, the flame takes the same shape as when singing (Experiment IV.).

Experiment VI.—Produce a flame tolerably sensitive (Experiment V.). Mount a whistle so that it may revolve round the flame. In two positions, being the ends of a diameter passing through the edges of the flame, the sound will produce no effect.

Note.—It will be seen that the sonorous impulses strike the flame in such a manner that the two columns are thrown into the same phase of vibration. The maximum effect is produced at points at right-angles to the minima. The waves reaching the nearer column first will throw it into a phase differing from the distant one.

Experiment VII.—Select jets of such a size that a flame may be obtained as large as one's hand. At a pressure just below that at which it sings it will be highly resonant.[2]

A sound produced in its vicinity will cause it to resound. When the exciting note has ceased, the sound of the flame will gradually subside; at the same time giving forth higher and higher harmonics of the note.

The sounds, however, are very feeble, and can only be heard at a very short distance. A convenient arrangement for amplifying the sound, is to place the stoppered end of a large gas jar in contact with the ear, and direct the open mouth towards the flame. With a correct adjustment any note within a very wide range will excite the flame, which may emit the same, or some harmonic of it. By whistling an air with the mouth, a rather pleasing accompaniment is heard, and the extreme gravity of some of the flame-notes is certainly remarkable. I have not succeeded in augmenting the notes so as to make this a lecture experiment.

Fig. 4 consists of a cork or other suitable material about 6 in. long, and 1 in. wide and thick. The axis of a jet A is directed to just pass clear of the top of a jet B. A is supported in a cork so that it may approach or recede from B. The end of A is contracted till it will give a flame about $1\frac{1}{2}$ inch long, under the full pressure of the gas mains ($\frac{6}{10}$ in. water).

[1] This corresponds with the intersection of axes of the jets.

[2] A large gas supply is indispensable, and very careful adjustment is required to obtain the most favourable result. A pressure of $1\frac{1}{2}$ inches of water is sufficient.

When tested with water it should show very slight segmentation.

Experiment VIII.—Produce at B the smallest possible flame, and direct the full flame from A, so that the point just passes over the top of B. Extinguish A without turning off the gas. The issuing gas will form a bluish cone beyond B, the space between A and B remaining unignited.

A may now be withdrawn from B till the cone becomes unsteady. If the column has very slight segmentation the distance may be five or six inches. With most jets, however, the limit will be about three or four inches.

This column is exceedingly sensitive. The faintest sound to which it responds will cause the ignited cone to recede towards A.

The greater length, and therefore the greater velocity of the unignited column will be at once noticed (*vide* Experiment IV.)

Experiment IX.—Arrange as Experiment VIII., and remove A just so far, that the cone does not strike back to it. Now very softly produce the responding note. The column recedes and becomes ignited through the whole of its length.

Experiment X.—Replace A by a rather more segmental jet, and obtain the cone. It will be seen that when the cone recedes, it is divided at the extremity more or less perfectly, into two parts.

Experiment XI.—Replace the jet B by a disc of spongy platinum [1] about the size of a halfpenny.

The unignited column causes an annular patch of the platinum to become red hot. When the column is excited the annulus is divided into two spots. The same column, if previously ignited, will not be affected by the same sound.

This forms another proof of the excitability of the unignited column.

In Fig. 5 two equal jets inclined to each other at an acute angle, are fixed in a cork which freely slides in a tube 5 in. × $\frac{5}{8}$ in.

Experiment XII.—Using the apparatus (Fig. 5), slide the cork near the top of the tube and ignite the gas. Slowly lower the cork till a point is reached at which the flame is sensitive. Observe that the base of the quiescent flame in contact with the tube forms a sinuous line, consisting of two depressions and two crests at right angles to each other; and further, that the crests correspond in position with the edges of the primary expansion, and the depressions consequently with the sides.

Note.—When the flame is excited their relative positions are reversed.

Experiment XIII.—Use a low gas pressure (one or two tenths) with Fig. 5, and slide the cork down the tube till the base of the flame becomes unsteady. At a certain point a noise will cause the flame to move from side to side *in the same plane as the jets.* Increase of pressure accelerates the movement.

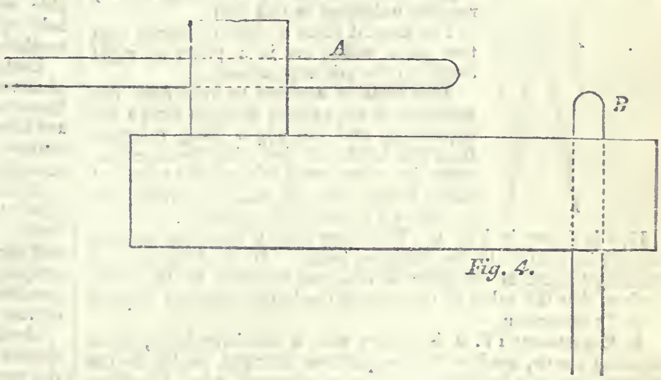


Fig. 4.

Instruments for the production of sensitive flames may be divided into two classes:—

1. Those in which the whole of the gaseous column is ignited.
2. Those in which the column is only partially ignited.

[1] A good substitute for the spongy platinum is obtained by pounding some fine asbestos till quite smooth, moistening with a tolerably strong solution of platinum tetrachloride, moulding the mass into the required shape in a piece of paper, and igniting. The paper burns off and leaves a porous fragile mass. This is alternately soaked in the platinum solution and ignited three or four times, when it becomes tolerably durable.

Class I. *Ignited Columns.*	Class II. *Partially-ignited Columns.*
a. The apparatus described by Prof. Barratt, *Phil. Mag.*, April, 1867.	*a.* Described in Nature, vol. v. p. 30.
b. Described in this paper, Fig. 2, *et seq.*[1]	*b.* Described in a former communication, December 7, 1876.
	c. Described in this paper, Fig. 4, *et seq.*

I. *a* consists of a glass tube with a tapering jet. A V-shaped cut made across the end of the jet renders it more sensitive. If such a jet be connected to a water-supply it will be found to be segmental. If it has the V-shaped groove this will be more marked. The primary expansion lies in the groove in most cases. The only use of the latter is to render the column more certainly segmental.

Fig. 5.

The column being segmental, consists (Experiment II., note) of two streams, one on each side the groove meeting at a very acute angle.

When there is no groove little irregularities in the orifice determine the segmentation of the column. The friction of the long, narrow jet and the ignition of the gas at the orifice retard the outflow, and to obtain a sufficient velocity the gas must issue under a considerable pressure[2] (one or two feet).

When excited it shortens and expands at *right angles to its primary expansion.*

I. *b* has been described (Fig. 2, *et seq.*). The jets meeting at a considerable angle, the column is flattened. It responds in the same manner as the preceding, but the primary expansion being very short, the responding expansion is usually the longer one.

II. *a* consists of a tapering jet placed a small distance below a piece of fine wire gauze. The gas is ignited above the gauze. It is very sensitive, but the intersection of the column by the gauze prevents the flame from taking any well-marked form when responding.

II. *b* is practically the same as the foregoing. The column being surrounded for a part of its length by the closed tube, remains unignited in this part.

The excited flame divides distinctly into two parts, which are in a plane at *right angles* to the primary expansion.

This division appears to arise from the tendency of the excited flame to form a flat expansion, and the edges being reflected from the inside of the tube, because the same jet when used without the tube and under a high pressure, does not divide, but produces a fan-shaped expansion.

II. *c* is shown in Fig. 4. The flame from B keeps the column beyond it heated to the ignition point. It is of interest as showing the use of the gauze in II. *a* and the tube in II. *c*, and further, that the value of the tube in the latter does not depend upon its resonance.

In Experiment IV. it is shown that a sensitive-flame, when emitting a note, sends out an expansion at right angles to the primary one, the same behaviour being observed when the flame is excited by an external sound. It therefore follows that if these actions are conversely related to each other, that a responding flame should emit a note. This will always be found to be the case; but the sound being usually very feeble, may escape observation unless some means be adopted to concentrate it (*vide* Experiment VII.).

The expansion occurs just above the intersection of the axes of the jets. Call these *a* and *b*. The two columns strive for a mastery of direction. *a* overcomes *b* and sends a tongue of flame through the primary expansion, but the partial stoppage of *b* causes an increase of pressure by which in turn it overcomes *a*, and sends a tongue of flame through the other side of the primary expansion, and so on. These movements succeeding each other with very great rapidity in a high note, and gas being highly elastic it is impossible to recognise them separately. Experiment XIII. shows how the impulses may be obtained so slowly as to be individually perceived.

When the gas pressure is so low that the column is quiescent, a sound is necessary to start the operation; and further the sound must so strike the component columns as to give one of them an advantage (*vide* Experiment VI.).

I have only referred to water columns as far as was necessary to illustrate the behaviour of gaseous ones. They should form a subject for separate consideration.

Summary.

1. A fluid column if sensitive to sound consists of two columns meeting at an angle (Experiment II. *note*).
2. The resultant of the two columns is an expansion (Experiments II. and III.).
3. A column so constituted will under favourable conditions emit a note (Experiment IV.).
4. If excited by an external sound, it takes the same form as when it spontaneously emits the sound (Experiment V.).
5. A column excited as in 3 and 4 sends out an expansion at an angle (usually a right angle) to the primary expansion.
6. The component column of a sensitive column must be at such unequal distances from the sounding body that they are not thrown into the same phase of vibration (Experiment VI. and *note*).
7. A gaseous column increases in sensitiveness with the pressure, *i.e.*, the velocity.
8. A gaseous column is lessened in velocity by ignition at its origin (Experiment VIII.). Hence—
9. A gaseous column when ignited is less sensitive than when unignited (Experiment IV.).

R. H. Ridout

[1] Lecomte has previously shown that the fish-tail burner is sensitive.
[2] By making a V-groove across the end of a partially-closed tube, this kind of jet becomes tolerably sensitive at a pressure of three inches

THE AURORA OBSERVATIONS OF THE AUSTRO-HUNGARIAN ARCTIC EXPEDITION, 1872-74, *BY CARL WEYPRECHT*

The Austro-Hungarian Arctic Expedition of 1872–74 was in many respects an unfortunate one. Not only was the first winter occupied with an unintermitted struggle with the ice, which from hour to hour threatened to crush the ship, and rendered it imperative that everything should be in constant readiness for her sudden abandonment, but in the second year this had actually to take place, and, on account of their bulk, valuable records of scientific observation were unavoidably left behind, and among these was the carefully-kept journal of northern-light observations.

Under such discouraging conditions the mass of valuable observations which Lieut. Weyprecht has succeeded in collecting from the meteorological and magnetic journals and other sources, are interesting not only on account of their many positive contributions to our knowledge, but as an example of wonderful scientific industry and devotion.

Spite of the perpetual changes of the aurora, Weyprecht considers that its appearances may be classified under five distinct forms, viz., the *arch*, the *ribbon* or *streamer*, the *rays*, *crown*, and *haze* (*Bogen*, *Band*, *Fäden*, *Krone*, and *Dunst*). His description of these forms differs in several particulars from those common in lower latitudes, so that we may be excused for noticing them at some small length.

Arches (*Bögen*, *arcs*) are of regular form; the highest point closely coincides with the magnetic meridian and the ends cut the horizon at points equi-distant from it. They usually move either northward or southward, rising from the edge of a low dark segment near the horizon, or again vanishing into it. The rim of light which edges this dark segment is probably only a low and distant bow, or possibly the combined effect of all the remoter arches which are melted into each other by distance and perspective. This is the more likely since a bow is never observed to sink wholly below the horizon, but fades into this distant rim, and, conversely, from it, arches frequently arise and separate themselves as they get higher. Not unfrequently the arches sink back to the point from whence they arose; at other times they gradually fade away as they near the zenith, or after they have passed it. Very intense displays never take the form of regular arches.

While in low latitudes the arch is one of the most frequent of auroral forms, in the polar regions this post is occupied by the *ribbon* or *streamer* (*Band*), which resembles a torn and irregular arch, and may take any position or direction, one or both the ends being usually visible above the horizon. It takes the most varied and ever-changing forms, forcibly suggesting a pennon floating on the breeze, doubling and curling on itself in graceful and translucent folds, or again appearing as if torn into shreds blown by the wind. Weyprecht is inclined to believe that the wind actually has some influence on the northern lights, and notes that, after severe and widely-extended storms, the streamers are especially ragged and broken. While in the arch the light intensity changes but slowly, and simultaneously over the whole bow, the reverse is the rule with streamers, in which it varies irregularly, lightening in one place and fading in another. The rapidity of these changes is very varied. Sometimes a band will drift slowly across the northern or southern sky, without change of form or colour, for a long time together, and then suddenly flame up with rapid movement across the zenith to form a crown, to renew its play, or to vanish on the other side. Frequently pieces break off and form new bands, which spread over the whole heavens, and then again fade down to a single band of a new form, or perhaps carry on their game until the dawn drives them from the sky. In most cases the light of the streamers has a peculiar motion, resembling waves which roll continuously from one end of the band to the other. These waves are more intense and move more rapidly in proportion to the activity of the bands themselves. The streamers vary in their appearance, sometimes seeming to consist of a uniform light-material like most of the arches, at others of closely-packed perpendicular rays, with the intervals filled with the same light-material. Between these two extremes are all intermediate gradations. When the rays are visible each brightens as the wave passes over it, but does not change its position except by a slight lengthening, which gives the hopping or dancing motion which, no doubt, is the origin of the term "merry dancers." When, on the other hand, the streamers consist of simple "light-material," the waves cause a brightening and slight undulatory motion of the edge.

Admirable illustrations of these streamers, as they are occasionally seen in lower latitudes, may be found in Prof. Piazzi Smyth's Edinburgh astronomical observations, under the name of "multiple arcs," but they are already approximating to regular arches, from which they probably differ only in distance and altitude. This work also contains most valuable plates and observations of the auroral spectrum, as compared with that of twilight and the zodiacal light. The *threads* (*Fäden*, *rayons*) are fine rays of light directed from the magnetic zenith to the horizon, but not quite reaching either. They occur sometimes singly or in bundles, sometimes pretty uniformly distributed, and are very variable in length. Their breadth rarely exceeds one minute of arc, and they are separated from each other by dark spaces. Their motion is peculiar, and seldom rapid. They lengthen and shorten upwards or downwards, giving the impression that already existing threads are lighted up or fade. They also move slowly from west to east or east to west, and not unfrequently it seems rather that the light is transferred gradually from one thread to another than that the threads themselves actually move.

The threads are evidently in intimate connection with the streamers. Often they stretch from near the magnetic zenith like a fan or a veil of gold or silver threads, of which the streamer forms the broad lower border; singularly, however, they rarely actually reach it, but seem to fade away near its edge, only to reappear with greater brightness in the streamer itself. This phenomenon is seen in the most beautiful way where two bands appear at once, one over the other, and each with its proper fringe of threads, like a silvery veil, falling in the most graceful folds. It is, however, only occasionally that threads and streamer occur in combination, and in the feebler displays it is more usual to see fans of threads, or streamers alone than both at once.

The *crown*, as is well known, is the perspective effect produced round the magnetic zenith, towards which the auroral beams, following the lines of magnetic force, seem to converge. The intensity of the appearance is very variable, and at times becomes such that thousands of short broad flashes dart at once from or towards the centre, while the whole firmament is covered with widespread rays, which lengthen and shorten with a flickering motion. Broad bright flames leap about the centre as if driven by a fiery whirlwind, and all the heavens seem in flames. In general the more brilliant the appearance, the shorter is its duration, and though it sometimes happens that masses of light move for hours about the centre, yet in such cases the brilliancy and the motion are alike feeble.

As a rule a crown is formed whenever an aurora of pronounced form passes the magnetic zenith, *i.e.*, when the beams of which it is composed are parallel with the observer's line of sight. Faint auroræ, however, especially arches with slow motion and no rays, may pass the zenith without forming a crown, and, on the other hand, a feeble crown sometimes becomes visible without the passage of any noticeable band or arch. When, as sometimes happens, the streamer is formed of uniform "light-material" without distinct rays, rays are also absent from the crown, which consists merely of lambent flames flickering round the centre, and resembling those of alcohol burning on a flat surface.

The *haze* (*Dunst*), as its name implies, resembles a faint mist, from which, by moonlight, it is scarcely distinguishable, as its light is never intense.

With regard to *colour*, Weyprecht observed that in bands the red always formed the lower and green the upper part, the middle being whitish. The haze was frequently reddish, but dull greenish seems to have been the prevailing colour, which is compared to that of the electric spark. Most unfortunately the expedition was unprovided with any spectroscope suitable for such observations, so that on this most important point we have no information to record.

The intensity of the light was such that the smallest type was frequently legible, and larger could be read easily. Weyprecht proposes to measure the intensity of the aurora by observation of the legibility of print of different sizes.

With regard to the height of the northern lights above the earth's surface, Weyprecht is strongly of the opinion that they are much lower in the arctic regions than in lower latitudes, but was not able to make any direct measurements. Their brilliancy and distinctness, and, above all, their rapid movements, give the impression of nearness; and the observations of Parry, Farquharson, and others, lead to the same conclusion. On one or two occasions the aurora appeared to be below the light cirrus clouds, which do not attain a maximum height of more than 8,000 metres; but the observation was by no means certain.

Auroræ were also repeatedly visible when the sun was so little below the horizon that the height of the direct sun-rays in the zenith was not more than from six to twelve miles. If, as has been suspected, the auroral light depends on some form of mist for its basis, this would have been rendered visible had the height been such as to bring it into the sunlight; but if, as is generally supposed, it depends on the electrification of rare and transparent gases, this would not be the case; so that it seems difficult to draw any conclusion from these facts. Admitting, however, the conclusion, which is in itself probable enough, that in high latitudes the appearances are lower than with us, it will go far to explain many of the differences which may be noted between Weyprecht's description and what we are accustomed to see. Streamers such as he describes would, if far enough off, and owing their altitude to great elevation, appear like arches, as the lower edge is at an almost uniform height, the windings would disappear with distance, and we should have the appearance of a pretty regular arch of irregular brightness and with beams shooting from it towards the zenith, while the individual threads would be, as is indeed the case, rarely or never visible.

Weyprecht repeatedly observed clouds and mist which took the same forms as aurora, and strongly resembled it; but he does not think a case is made out for any real connection between the two phenomena. The stars, however, are decidedly obscured by an intense aurora, and many observations seem to point to some such connection. Several very interesting ones are quoted by the present writer in the article "Aurora" in the last edition of the *Encyc. Brit.* Weyprecht frequently uses the phrase "light-material" in speaking of the aurora, and it is evident that there must be some material basis to the phenomenon. This, he suggests, may be the minute ice crystals, which are the cause of mock suns and moons, appearances of daily occurrence in the polar regions. It seems certainly possible that these may be projected far above the earth, or even above the atmosphere by electrical repulsion, and may serve as carriers of electrical discharges, which would at once illuminate the particles and arrange them in the lines of magnetic force. If the aurora is really, as

is commonly supposed, an electric discharge in a rarefied gas, it must admit of a very considerable range of density, since it is certain that even in the same aurora, different portions are at very different elevations. In the crown for instance, the beams are almost perpendicular, and must be often of very great length.

The aurora has frequently been supposed to be a sign of coming wind and stormy weather, but careful comparison of the meteorological records failed to establish any such connection. No sound could be attributed to the northern lights.

Weyprecht's observations confirmed the fact that there is a zone of maximum frequency and intensity of auroræ some distance south of the pole, and led further to the conclusion that the zone moves northward towards the winter solstice, and southward again towards the equinoxes. It is of course impossible to observe its course during the summer.

Observations of the daily period gave a maximum at 10 P.M., and a minimum at 11 A.M., which closely coincides with the results of other observers.

No very clear conclusion could be drawn as to a yearly period, since the length of the nights, the cloudiness of the sky, and above all, the before-mentioned shifting of the zone of maximum frequency so complicated the problem.

Want of space unfortunately forbids us to enter into any detailed discussion of Weyprecht's extensive magnetic observations. Many magnetic disturbances were unaccompanied by aurora, and on the other hand, some aurora produced little or no magnetic variation. Those appearances which have an indistinct outline and diffused light, and especially which have no rays and no noticeable motion, scarcely affect the needle; while, on the contrary, those which appear to be low and near, which have distinct contour and rapid motion, and above all sharply defined rays, affect the needle vigorously. Broad darting lightning-like rays, with brilliant colours, red and green, cause the most violent disturbances. HENRY R. PROCTER

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

WE are pleased to find that local schools of science are increasing their facilities for teaching practical chemistry to their students. During the summer the committee of the Birkbeck Institution (the first London Mechanics' Institute) has built a new chemical laboratory, to replace the somewhat inconvenient one which has done service for fifty years. The space available was very limited, but has been made the most of, and nineteen benches have been fitted up in a space of 36 feet by 10½ feet, besides the necessary accommodation for stores, &c. This effort to meet the requirements of students is all the more commendable, on account of the probability of the Institution remaining but a few years longer in its present home. Its success is so great that a new and larger building is absolutely necessary, and the building fund being raised amounts already to nearly a thousand pounds.

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, September 16.—M. Fizeau in the chair.—The following papers were read:—On the cause of the periodic movements of flowers and leaves, and on heliotropism, by M. P. Bret.—On a new telephonic transmitter, by M. P. Dupont.—Remarks on a new proposal made with regard to the analysis of milk, by M. E. Marchand.—Description of a new apparatus for the transfer of gases from one vessel to another, by M. A. Blanc. The appatatus is constructed with the special view of avoiding any loss of gas on its being transferred over mercury.—Various communications regarding phylloxera, by MM. E. Fortier and Capbladoux.—On the intra-Mercurial planet seen in the United States during the solar eclipse of July 29 last, by Mr. Swift.—On the observations made of the transit of Mercury on May 6, 1878, at the Imperial Observatory of Rio de Janeiro, after the new method, by M. Emm. Liais.—On the form of the integrals of differential equations of the second degree in the neighbourhood of certain critical points.—On the compressibility of gases at high pressures, by M. E. H. Amagat.—New researches on the physiology of the vesicular epithelium, by MM. P. Cazenove and Ch. Lyon.—Note on the interior temperature of the globe, by Mr. W. Morris.—Note relating to an apparatus named galioscope, by M. A. Boillot. This apparatus is constructed to demonstrate the invariability of the direction of the plane of oscillation of a pendulum.—Note relating to a new thermohydrometer, by Mr. H. Douglas.

September 23.—M. Fizeau in the chair.—The following among other papers were read:—Dissociation of oxides of the family of platina, by MM. Saint Claire Deville and Debray. Osmium and ruthenium combine directly with oxygen, and the product of oxidation is volatile and forms at the highest temperatures. In this they are distinct from the other platinic metals, and come near arsenic and antimony, with which they might be placed among the metalloids. From experiments with oxide of iridium the authors show that, at a temperature below 1003·3°, it is decomposed in free air, and consequently that, at this temperature or any other higher, iridium is absolutely inoxidable in the air.—Memoir on a universal law relative to the dilatation of bodies, by M. Levy. This is designed to prove that the pressure which any body supports can only be a linear function of its temperature, so long as this body does not change its state; in other words, and under a physical form, if any body be heated with constant volume, the pressure which it exerts on the immovable walls of the inclosure containing it, can only increase, rigorously, in proportion to its temperature. This, he thinks, an absolutely rigorous corollary from the two fundamental propositions of the mechanical theory of heat, and of the hypothesis that the mutual actions of the atoms of bodies are directed in lines which join their points of application, and only depend on the distances of these points apart.—Nocturnal variations of the temperature at different altitudes recorded at the observatory of Puy-de-Dôme, by M. Alluard. He has traced, for each month since January, the curves of minimum, maximum, and mean temperatures of the two stations, and it appears that the two curves of minimum temperature often cut each other, both in summer and in winter, so that, often, at night, it is less cold at the top of the Puy-de-Dôme than at Clermont, the difference sometimes reaching five degrees. The curves of maximum temperature do not intersect; they are in general nearly parallel. The temperature at night, then, varies with the height, quite otherwise than during the day. A new meteorological station has been formed about midway between Clermont and the summit of Puy-de-Dôme, the three heights being thus—400 m., 1,000 m., and 1,470 m. It is purposed, too, to take observations on some of the numerous extinct volcanoes (in form of truncated cones) about the Puy-de-Dôme, the object being to study the atmosphere, layer by layer.—Discovery of a small planet at the observatory of Hamilton College, Clinton, by Mr. Peters.—On a new species of curves and of anallagmatic surfaces, by M. Picquet.—On the development of chilostoman bryozoa, by M. Barrois. This he finds *mesoblastic*, the exoderm gives rise to all the organs and plays the part of a veritable blastoderm.—M. Lancy presented a work by M. Ennes, surgeon in the Portuguese army, on "Men and Books of Military Medicine."

CONTENTS

THURSDAY, OCTOBER 10, 1878

THE ELECTRIC ARC AMONG THE GAS SHARES

IN Wednesday's *Daily News* we read as follows:—

"Gas shares have been subjected to considerable depreciation owing to the publication of some statements stated to have been made at a meeting in Birmingham. The folly which shareholders exhibit in sacrificing their holdings on the slightest alarm cannot be too strongly deprecated." Among the changes recorded in the official list we find Imperial Continental fell 7; Gas Light and Coke, H issue, $7\frac{1}{2}$; ditto, Ordinary, 5, and so on, and so on.

There is little doubt that some excellent fooling (if fun were the writer's only object) on the part of the *New York Sun* is among the causes of this wonderful exhibition of alarm on the part of gas shareholders. But we doubt the fun. Any man quoting the remark that the sun shines on the evil and the good, would, if he combined candour with perfect knowledge, make an exception in favour of the *New York Sun*. It has a decided predilection for the evil. Nor is this all. Mr. Edison, besides being the most wonderful inventive genius of the age, is one of those rare beings, an American humourist. Indeed, if we are to believe the Western newspapers, those universities and stock raisers beyond Chicago who hastened, after the manner of our own Oxford, to enshrine his name on the rolls of their illustrious men and beasts, did so not because he had benefited mankind or lived laborious days in his laboratory at Menlo Park, but because a word dropped by him had given rise to the rumour that he had just put the finishing-touch to a swearing-machine.

Here is the *New York Sun's* story;—

"Mr. Edison says that he has discovered how to make electricity a cheap and practicable substitute for illuminating gas. Many scientific men have worked assiduously in that direction, but with little success. A powerful electric light was the result of these experiments, but the problem of its division into many small lights was a puzzler. Gramme, Siemens, Brush, Wallace, and others, produced at most ten lights from a single machine, but a single one of them was found to be impracticable for lighting aught save large foundries, mills, and workshops. It has been reserved for Mr. Edison to solve the difficult problem desired. This, he says, he has done within a few days. His experience with the telephone, however, has taught him to be cautious, and he is exerting himself to protect the new scientific marvel, which, he says, will make the use of gas for illumination a thing of the past.

"While on a visit to William Wallace, the electrical machine manufacturer in Ansonia, Connecticut, he was shown the lately-perfected dynamo-electric machine for transmitting power by electricity. When power is applied to this machine it will not only reproduce it but will turn it into light. Although said by Edison to be more powerful than any other machine of the kind known, it will divide the light of the electricity produced into but ten separate lights. These being equal in power to 4,000 candles, their impracticability for general purposes is apparent. Each of these lights is in a substantial metal frame, capable of holding in a horizontal position two carbon plates, each 12 in. long, $2\frac{1}{2}$ in. wide, and $\frac{1}{2}$ in. thick. The upper and lower parts of the frame are insulated from each other, and one of the conducting wires is connected with each carbon. In the centre and above the upper carbon, is an electro-magnet in the circuit with an armature, by means of which the upper carbon is separated from the lower as far as desired. Wires from the source of electricity are placed in the binding posts. The carbons being brought together the circuit is closed, the electro-magnet acts, raising and lowering the upper carbon enough to give a bright light. The light moves towards the opposite end from which it starts, then changes and goes back, always moving towards the place where the carbons are nearest together. If from any cause the light goes out the circuit is broken and the electric magnet ceases to act. Instantly the upper magnet falls the circuit is closed, it relights, and separates the carbon again.

"Edison on returning home after his visit to Ansonia studied and experimented with electric lights. On Friday last his efforts were crowned with success, and the project that has filled the minds of many scientific men for years was developed.

"'I have it now!' he said, on Saturday, while vigorously turning the handle of a Ritchie inductive coil in his laboratory at Menlo Park, 'and, singularly enough, I have obtained it through an entirely different process than that from which scientific men have ever sought to secure it. They have all been working in the same groove, and when it is known how I have accomplished my object, everybody will wonder why they have never thought of it, it is so simple. When ten lights have been produced by a single electric machine, it has been thought to be a great triumph of scientific skill. With the process I have just discovered I can produce 1,000—ay, 10,000—from one machine. Indeed, the number may be said to be infinite. When the brilliancy and cheapness of the lights are made known to the public—which will be in a few weeks, or just as soon as I can thoroughly protect the process—illumination by carburetted hydrogen gas will be discarded. With fifteen or twenty of these dynamo-electric machines recently perfected by Mr. Wallace, I can light the entire lower part of New York City, using a 500 horse-power engine. I purpose to establish one of these light centres in Nassau Street, whence wires can be run up town as far as the Cooper Institute, down to the Battery, and across to both rivers. These wires must be insulated, and laid in the ground in the same manner as gas-pipes. I also propose to utilise the gas-burners and chandeliers now in use. In each house I can place a light meter, whence these wires will pass through the house, tapping small metallic contrivances that may be placed over each burner. Then housekeepers may turn off their gas and send the meters back to the companies whence they came. Whenever it is desired to light a jet it will only be necessary to touch a little spring near it. No matches are required.

"'Again, the same wire that brings the light to you,' Mr. Edison continued, 'will also bring power and heat. With the power you can run an elevator, a sewing-machine, or any other mechanical contrivance that requires a motor, and by means of the heat you may cook your food. To utilise the heat it will only be necessary to have the ovens or stoves properly arranged for its reception. This can be done at trifling cost. The dynamo-electric machine, called a telemachon, and which has already been described, may be run by water or steam power at a distance. When used in a large city the machine would of necessity be run by steam power. I have computed the relative cost of the light, power, and heat generated by the electricity transmitted to the telemachon to be but a fraction of the cost where obtained in the ordinary way. By a battery or steam-power it is forty-six times cheaper, and by water-power probably 95 per cent. cheaper.'

"It has been computed that by Edison's process the same amount of light that is given by 1,000 cubic feet of the carburetted hydrogen gas now used in this way, and for which from \$2.50 to \$3 is paid, may be obtained for

from 12 to 15 cents. Edison will soon give a public exhibition of his new invention."

So much for the *New York Sun*. Although a student of science will have little difficulty in associating the results promised with the discovery of perpetual motion, it is quite probable that Mr. Edison has actually succeeded in doing what he states he has done in his telegram: "I have just solved the problem of the sub-division of the electric light indefinitely." What we wish to point out is that it is one thing to do this and another thing to produce an electric light for ordinary house and street use. Once put the molecules of solid carbon in motion, and just because a solid is in question, the light must be excessive and the expenditure of energy must be considerable.

While it is easy to believe that the future may produce a means of illumination mid-way between the electric light and gas, it is equally easy to see that the thing is impossible without great waste, and therefore cost, with dynamo-electric machines and carbon poles. So long as carbon is employed we shall have much light which, perhaps, can be increased and steadied if various gases and pressures are tried. But streets and rooms full of such suns as these would be unbearable unless we sacrifice much of the light after we have got it. Split up the currrent in the manner so cheerfully described by the New York paper, and the carbon will refuse to flow altogether if an engine of 5,000,000 horse-power be employed instead of the modest one of 500 which is to light the south part of the island. If Mr. Edison has succeeded in replacing carbon he may have turned the flank of the difficulty to a certain extent.

Although, however, we may pity the ignorance of those who act upon such statements as those made by the imaginative *New York Sun*, gas companies may well begin to feel uneasy at the general attention which is being drawn to the electric light as a substitute for gas *if they are prepared to let things alone*. That in one form or other it is likely to be partially adopted in all large cities and at extensive public works seems most likely. It will be one of the lights of the future, but not to the excluding or superseding of gas-light.

Our own columns have repeatedly borne testimony to the success which has attended its introduction into Paris, where it is to be met with at almost every corner, and at one or more of the railway stations. The general testimony of those who are unprejudiced is that at least for wide streets, squares, and open places, its lighting effect is all that could be desired. Every Londoner is familiar with the effect of the display which the enterprising Mr. Hollingshead has placed in front of the Gaiety Theatre, and the glowing contrast presented to the miserable yellow flames of the neighbouring street-lamps; but this contrast exists because the gas is bad and dear. Mr. Hollingshead, in a letter to the *Daily News*, corrects the view of the gas companies, that the electric light must necessarily cost more to produce than gas. His own display, necessarily wasteful, costs four-fifths what gas would, and he is probably correct in saying that with proper management it need not cost more than one half. Moreover, in yesterday's *Times*, Mr. E. J. Reed refers to the case of M. E. Manchon, a large manufacturer at Rouen, who has gone to considerable expense to alter his premises to suit the electric light, and who, even with hired engine power, finds that there is an annual saving of 22·6 per cent. over gas, with infinitely better light and a wholesome atmosphere. Mr. Reed is of opinion that even if the electric light cost more than gas, its advantages are so great, that for the lighting of public places, museums, art galleries, manufactories, &c., he would advocate its general introduction. Even Madrid, one of the most backward cities in Europe, has introduced the light, one great benefit of which, especially in theatres and other much frequented places, is that the heat generated and the contamination of the air is greatly less than in the case of gas.

Let the directors of gas companies do all they can to improve their gas. They may be certain that it will never cease to be required; a considerable splitting up of the electric current is impossible, while the brilliant light that we shall always get when electricity is employed will gradually so raise the *pitch* of illumination that more gas than ever will be used.

THE MEDICAL FACULTIES

THE opening addresses of the various London medical schools always form an interesting episode in the scientific year, and this session they have been even more interesting and have attracted more attention than usual. This is especially the case with the vigorous and trenchant address (published in full in the *British Medical Journal* of October 5) of Prof. Ray Lankester. On another page we reprint a remarkable article from the *Lancet*, in which it is plainly stated that without endowment of research the progress of medicine must soon become impossible in this country; that the work of scientific investigation demands practically the whole energies of a competent man and is incompatible with the necessity of earning a living in any other direction. It is somewhat remarkable that such an article should be published simultaneously with the outspoken address of Prof. Lankester, who aimed to show that the great Universities of this country are faithless to their duty and to the end for which they were established, in not providing for the pursuit of scientific research, in so far at least as that bears on the healing art.

"The work of the medical profession—its function in the community"—he showed, "is to bring into practical use an immense mass of accurate knowledge with regard to the conditions affecting the healthy working of the human body. Accordingly, two distinct kinds of activity—one dependent on the other, one as important as the other—are to be recognised as essential to the business of the medical profession. The one consists in the accumulating of knowledge relating to the human body and to the conditions affecting its health, the sifting of false from true knowledge, the producing of new knowledge; the other consists in the application of this knowledge to particular cases of disease or danger in such a way that action may be taken, avoiding the disease or danger, or alleviating the suffering which results from them.

"There is a most mischievous notion current at the present time," Mr. Lankester went on, "that the first of these lines of work is 'theoretical,' and that the second is 'practical;' and it is not unusual to separate the 'theoretical' from the 'practical' man,

and to speak of the "theoretical" and the "practical" portion of medical studies. "Such a division," Mr. Lankester truly said, "is a relic from the bad old times. Theory and practice in medicine, as elsewhere, go hand in hand. The results of scientific investigation cannot be applied in the treatment of disease by the man untrained in scientific method any more than the delicate tools of a lathe can be used by one who has not himself devised them, or a watch be mended by the aid of a treatise on watch-making."

Mr. Lankester pointed out that in the present unsatisfactory state of the multitude of medical faculties in England, dependent on the voluntary services of busy medical practitioners, medical education must necessarily be defective; and that so far as this is concerned, the wealthy Universities of Oxford and Cambridge have for long most shamefully neglected their duty. As a result of this neglect on the part of our universities, medical education in the last century was a thing almost unknown in England. Those who were desirous of qualifying themselves for carrying on the profession with anything like thoroughness, had to go to Scotland, or Paris, or Italy, where the idea of a university with its various "faculties," has all along been kept in view. The multitude, however, were content with a simple "apprenticeship" to a medical practitioner, while a few latterly took to following the hospital physician round the wards, to take note of the "great man's" receipts. "But as for instruction in physics, in chemistry, in comparative anatomy, in physiology, in the general properties and activities of living things, it had no existence in London, and was not in any way required on the part of the licensing bodies. The English universities meanwhile, which possessed rich endowments for carrying on these studies, allowed jobbery and indifference to convert their ancient medical officer into sinecures." It was from Scotland, where the torch of true university life was kept burning, that an impulse towards the establishment of better things in benighted England came and set men to work in London. The origin of University College was then referred to, it being pointed out that Government, in its caprice, denied the privilege of granting degrees to the vigorous young institution, and conferred it upon a shadowy body, to which it misapplied the title of London "University," a mere *nominis umbra*, and an utter misapplication of the venerable term. However well the London University may have carried out its anomalous duty, we share in Prof. Lankester's profound regret, that the grand old title of "University" should have been in this way completely divorced from the work of study and teaching. The result is that, in this country, not one man in a hundred, even amongst those possessing university degrees, knows what a university is. "The Universities of Oxford and Cambridge, on the one hand, have entirely departed from the old standard, and ought long since to have been checked in their career and reformed by the power which chartered and protected them in their early days; whilst the admirable body which we call the University of London has precisely the same claim to be called a university as has the Archbishop of Canterbury."

Prof. Lankester then went on to show on how wide a basis of scientific investigation and study the medical ar ought to be built; it is the outcome, the final result, of observation.

"This is the spirit," he said, "in which the great universities of Europe, with the exception of Oxford and Cambridge, have fostered the study of medicine. This is the explanation of the existence of chairs of Chemistry, of Physics, of Botany, and of Zoology, in all their Medical Faculties. Such is the nature of his work that the medical man needs instruction and training in all the great branches of physical science; and from time to time the methods of investigation, the modes of speculation and the generalisations with which he has become familiar in the course of these apparently remote studies, render him most efficient service in the attempt to ascertain and to deal with diseased states of the human body. It is thus that a thorough knowledge of the organisation of both plants and animals becomes part of the equipment required by a medical man, but it is even more directly that the progress of knowledge relative to other organisms affects knowledge relative to the human organism. The knowledge of diseased and of healthy conditions of all organisms, all knowledge of living things, including necessarily man himself, forms one compact interwoven body of science termed biology, and upon this directly the medical art is built, in it all medical practice has its foundation."

But in order that the results of scientific research may be applied to the alleviation of human suffering, there must be continued investigation in order to produce new knowledge. "The production of new knowledge," Mr. Lankester justly said, "is a most absorbing and arduous business. Men who have anything else to do except a small amount of teaching can do very little—only a bit here and there—in the production of new knowledge. Men who are earning their livelihood in the practice of a profession can do very little at it. Men who are preparing students for examination all day all the year round can do but little at it. Only men with fortunes, or men who are paid by the institutions especially founded and meant for the production of new knowledge, can be expected to do much in this way. The institutions especially founded and designed for this production of new knowledge, and richly supported by large annual grants of money in the form of salaries and stipends, are abundant on the Continent of Europe; they are the Universities. In London we have no such institution; there is no University of London in this sense of the word. The medical profession in England, though it has eleven "Faculties" in London and other "Faculties" in provincial towns, is almost totally devoid of those splendid opportunities for profound investigation—for the production of new knowledge bearing on medicine—which the appropriation of public money and ancient endowments to the payment of the Medical Faculties in Germany, for instance, provides."

It is certainly, as Mr. Lankester said, at first sight rather astonishing that we laborious, hard-headed Englishmen, the countrymen of Harvey and Darwin, should have to go to Germany for so much of our new knowledge, and that our text-books of science, instead of being provided by the richly-endowed Fellows of Oxford and Cambridge, should to a large extent bear on their title-pages the names of German professors. This surprise

ceases when we are told that the German university system, consisting of twenty-one universities and 1,250 salaried professors, is carried on at an annual expense of more than 600,000*l.*" At the least, half of that sum and half of the number of salaried workers are devoted to the branches of science connected with medicine. "How is it we have nothing of the kind in England? Is it impossible?" The answer is, in Mr. Lankester's words, that "the production of new knowledge cannot go on without the assistance of endowments or their equivalent. It is impossible to name a single case of a man who did not enjoy either a private fortune or an endowment, and yet has added greatly to scientific knowledge. Medicine and the sciences which she protects have most urgent need of endowment for the purpose of supporting men who shall chiefly occupy themselves in the production of new knowledge." Mr. Lankester's words will show how things are managed in Germany :—

"It is a disgrace to English civilisation that a true university — an endowed university, a university in which new knowledge is continually being produced, and in which men are trained for this work of production as the work of their lives—does not exist in London and in each one of our large cities. I can briefly tell you some of the circumstances which have prevented the foundation of such desirable institutions in this country; and I will further indicate to you what we may hope to see done in this direction in the future. But first let me give you a sketch of one of these German universities which we so much admire and envy. I advisedly select one situated in a small town—the University of Heidelberg. Heidelberg is one of the two universities of the Grand Duchy of Baden, Freiburg being the other; whilst at Carlsruhe, in the same state, is an important technical school. The town of Heidelberg numbers but 22,300 inhabitants. The university has 61 professors, and, by the last returns, 834 students. Of these, 23 professors and 106 students belong to the faculty of medicine. The government of Baden, by which the salaries of the professors are paid and their number determined, does not consider that this proportion of one professor to every five students is an excessive proportion on the side of the professorial staff. This university was founded nearly five hundred years ago (in 1386), and, like all the German universities, was remodelled and greatly improved at the beginning of this century, whilst since that time its wants have continually been provided for with ever-increasing liberality by the state government. There are now four faculties—that of Theology, that of Law, that of Medicine, and that of Philosophy. The professors are divided into the ordinary and the extraordinary. The ordinary professors receive a stipend of about 400*l.* yearly, besides their fees, which in some cases bring their incomes up to 1,000*l.* When a vacancy occurs in a professorship the state minister invites the members of the faculty in which the vacancy has occurred to name two or more individuals whom they would recommend for appointment. The faculty meets and the name of a professor in some other university is proposed. He is written to and asked whether he will come; he probably replies that he would require an increased salary and a new laboratory; very usually his terms are agreed to by the state minister on the recommendation of the faculty, and he is installed in the vacant chair. Sometimes, of course, a younger and less known man is appointed at a lower salary. As an example of the way in which these things are managed in Germany, let me give you an actual history of what recently occurred at Strassburg. I quote from an American journal. 'After the transfer of that city to Germany neither pains nor money was spared to make the university a success. For the chairs of the medical faculty rising men were selected, all of whom were known for original research, and had practically proved their ability as teachers and writers. The chair of pathological anatomy was given to von Recklinghausen, one of the most brilliant of Virchow's pupils, who vacated a similar position at Würzburg in order to accept this new position. When the chair of pathological anatomy at Vienna became vacant by the retirement of the veteran Rokitansky it was offered to von Recklinghausen, and the salary proposed was 25,000 *francs* (1,000*l.*), or about three times the usual salary of such a professorship. But the Prussian government was quite as anxious to retain Prof. von Recklinghausen as the Austrian government was to obtain him, and asked him to say what he wanted. His reply was to demand, as the condition on which he would remain, that there should be constructed a large pathological institute, in accordance with his plans, and in connection with the hospital—an institute which will cost something like 50,000*l.*, and will require a change in the fortifications. His demand was acceded to, and he is hard at work now in Strassburg.'"

Prof. Lankester then describes the magnificent arrangement in Heidelberg for carrying on all kinds of research by men whose great business is to add to new knowledge, with the minimum of destruction of any kind. Here, among other well-known names, Bunsen, "the most eminent of living chemists," and Kühne, the physiologist, the author of the text-book as well known in England as in Germany, have their laboratories and class-rooms; and Gegenbaur is the head of the anatomical institute.

Other names equally great in original research in the various departments of science and other towns in Germany could be mentioned. "Berlin possesses laboratories and museums on a palatial scale, and a perfect army of investigators and students supported by State endowments. Leipzig, again, Strassburg, and Munich, are larger and more richly provided than Heidelberg. All the twenty-one German universities, the eleven Austrian, the four Swiss, and six or more Russian universities (I do not speak of those in Scandinavia, France, Holland, Belgium, and Italy), are fitted out in the same way. In all, medicine is being advanced and developed by the never-ceasing production of new knowledge."

"We, in England," as Mr. Lankester goes on to say, "benefit by this knowledge; we, in common with the rest of mankind, reap the rewards won by the activity of these noble corporations; and yet, it is neither more nor less than the fact that we Englishmen do not possess, throughout the length and breadth of our land, a single institution of our own where such work is done." In the three or four institutions where anything like original research is carried on in this country, the endowments are so inadequate as to seriously hinder anything like complete and satisfactory work. "To speak of these institutions as taking the place in this country of the vast machinery and resources of the Medical Faculties of Germany would be about as reasonable as to compare the pleasure-boats on the Lake of Geneva with the British navy."

Prof. Lankester then sketches the state of things which have come to exist at Oxford and Cambridge. These Universities were founded for the purpose of giving education in medicine as well as in theology, and endowment after endowment was made by men anxious that the Universities should fulfil their functions with efficiency;

but through intrigue these magnificent endowments have been almost entirely filched from the purposes for which they were meant, and the property which was thus consigned to the tender mercies of the Church, is now estimated to produce yearly in each University over 300,000*l*.

"For many years the faculties of law and medicine struggled on in Oxford, growing weaker and more neglected in each decade, until now, after 200 years of this usurpation, there is not a single medical student in the place. In Cambridge the story was very much the same, excepting that there the degradation of the medical faculty has never proceeded quite so far as it has at Oxford, and medical studies are now, we have some reason to hope, being resuscitated in that university by the strenuous efforts of Prof. Humphry and Dr. Michael Foster." We take a few instances from Mr. Lankester's address:—At Oxford, shortly before the destruction of its character as a university, the King, Henry the Eighth, had founded a Regius Professorship of Medicine. The office still exists, and is worth about 500*l*. a year, but the present tenant of the office gives no lectures and has no pupils. Linacre, the founder of the College of Physicians of London, Mr. Lankester tells us, "left to Merton College in Oxford (in the reign of Henry the Eighth) a piece of land, the rental of which was to pay a lecturer in medicine. Within 100 years the office was abolished, and the money converted to the private uses of the Fellows of the College. Confiding benefactors came forward last century and put down their money, in the hopes of promoting medical study in Oxford. But they did not know—and at the present day you cannot make people believe—how shameless and unprincipled were the bodies to whom they entrusted their money. Lord Lichfield bequeathed 200*l*. a year for the reading of clinical lectures in the Radcliffe Infirmary to the students in medicine of the University. The office is now held by the Regius Professor of Medicine, but no lectures are given. About the same time, Matthew Lee confided money to the care of the governing body of the cathedral house of Christ Church, for the payment of a teacher of human anatomy, and to buy subjects for his demonstrations, but no such teaching is given; the money is applied to other purposes. Dr. George Aldrich, in 1798, left 9,000*l*. for similar purposes, but, at the present day, the bequest bears no fruit for the benefit of medicine."

These are only a few instances of the scandals connected with the history and present condition of our great universities, mainly owing to their complete subjection to clerical influence. The colleges, instead of being lodging-houses for poor students, as they were intended to be, were converted into boarding-schools, into which the Fellows received the sons of the landed gentry and wealthy citizens as pupils, on condition of certain payments. To quote Prof. Lankester:—

"The fees demanded by the College-Fellows increased at last to such an extent, and the expense of residing in one of these boarding-houses became so great, that the universities entirely ceased to be popular or national institutions in function, though they were so in foundation. They became the exclusive possession of the clergy and the wealthy classes, and so they remain at the present day. Long ago, students ceased to seek the lecture-rooms of Oxford and Cambridge for the purpose of serious study or professional training. Whilst the Scotch farmer's lad can earn enough in the fields during summer to keep him during a winter's session in the University of Glasgow or Edinburgh, whilst all classes of the community contribute to form the student-world of the German Universities, Oxford and Cambridge, under the influence of clerical domination, have become simply 'finishing schools for young gentlemen' (I quote the words of Prof. Max Müller). Men of moderate means—that is to say, the majority of our fellow-countrymen—now only go to Oxford or to Cambridge with the view of sharing in the scholarships and fellowships, which are annually distributed there by competitive examination. In their whole tenour, purpose, and being, these places are as different as they possibly can be from their quondam sisters the universities of Germany."

What Prof. Lankester insists upon is the establishment of a fully-developed and amply-endowed Medical Faculty in both Cambridge and in Oxford, and, still further, the establishment of one or two such faculties in London. We are glad to think that there is an immediate prospect of a great development of the Medical Faculty at Cambridge, where already experimental physiology, human anotomy, and clinical medicine are taught and prosecuted with energy. There is indeed some prospect that in the course of years, when men with a better spirit have sway in both universities, they will be brought to fulfil all the functions for which they were originally established; and we trust that Prof. Lankester's address may act as the little leaven in the minds of all who heard or may read it, and that gradually not only professional men but the constituencies generally will wake up to a realisation of the immense benefits which are the nation's birthright, but from the enjoyment of which it has for so long been barred.

Prof. Lankester then urges that one, or at most two, medical faculties should be established under the University of London.

"In this way," he concludes, "we might have in London, each provided with ample laboratory, museum, and assistants, two professors of physiology, one of surgical anatomy, one of comparative anatomy, one of embryology, one of botany, one of pathological anatomy, one of pharmacology, one of hygiene, one of forensic medicine, two of chemistry, one of experimental physics, and others of the history and practice of medicine, of surgery, of midwifery, and of psychiatry. The maintenance of such a staff, with their laboratories and assistants, would require an endowment of 20,000*l*. a year, whilst 100,000*l*. would have to be sunk in providing the necessary buildings. This proposition appears Utopian, but all I have to say further in defence of it is this, that in Berlin, Vienna, Leipzig, and other continental cities the thing is done, and on a more costly scale than I have here suggested.

"When such medical faculties as I have sketched to you exist in Oxford and in London, England will have begun to do her duty by the great profession of medicine. Until then we are but hangers-on of foreign nations; until then we reap where we did not sow, we gather where we did not straw. Until that time I earnestly beg every man who enters on a medical career to remember that he is joining the cause of a profession deprived of its heritage, and to make it his business to reinstate medicine in her seat, and to secure the restitution of her possessions."

Prof. Lankester is not only Professor of Zoology at University College, but a Fellow of Exeter College, Oxford: so that he speaks with full knowledge, and

not as an outsider, who might be accused of ignorance and of interested motives. The subject which he has thus brought prominently into notice concerns the highest welfare of this country and the place which she holds among the cultured nations of the world.

MILLER'S CHEMISTRY

Elements of Chemistry, Theoretical and Practical. By William Allen Miller, M.D., LL.D. Revised by Charles E. Groves. Part II. *Inorganic Chemistry.* (London: Longmans and Co., 1878.)

THE number of editions through which this part of the late Dr. Miller's work has passed and the high position it has attained as a Text-book of Inorganic Chemistry render the work of the reviewer almost superfluous, and we have now only to notice what improvements or additions may have been made in the present edition.

The revision of the volume just published has been entrusted to Mr. C. E. Groves, whose position as sub-editor of the Chemical Society's *Journal* renders him particularly fitted for this work, by reason of his being constantly brought into contact with communications upon the more recent discoveries in the science.

The order in which the various elements and their compounds are discussed, as we are told in the preface, remains the same as in the last edition, this order commencing with the least complex compounds and finishing with those of a more complicated nature. Hydrogen is first studied as the standard of atomic weights, densities of gases, &c., and as affording a good example of an electro-positive element; it is followed by the monatomic but electro-negative element chlorine, subsequently by oxygen as illustrating the diatomic condition, boron illustrating the triatomic, carbon the tetratomic, nitrogen the pentatomic, and sulphur the hexatomic. We have thus presented to us in the first chapters of the work the consideration of seven typical elements and their compounds, which tends to give the reader in a simple manner a general idea of the scientific arrangement of the other elements which follow them in their respective groups.

We rather regret that Mr. Groves has thought it necessary to retain boron in its position as the typical element of the triatomic group to the exclusion of nitrogen, taking that latter substance as the illustration of the pentatomic group. Our reasons for saying so are, that boron is not so well known to the student as nitrogen, nor does it possess a hydrogen compound like ammonia; its atomicity, therefore, must be shown by its chlorine compound, thus destroying the similarity with the other types, which in the first four groups are illustrated by their compounds with hydrogen. Had nitrogen been taken as the type of triatomic bodies, phosphorus would then of course have replaced it as the example of a pentatomic element. Farther, by taking nitrogen as the type of pentads, as is done in Chapter x., a little confusion, we think, is liable to be produced in the mind of the beginner; as immediately after the consideration of that body as a pentad, he proceeds to consider ammonia where nitrogen is not *pentatomic* but *triatomic.*

Many of the more recently-discovered non-metallic compounds have been added to the work, and to some parts a considerable amount of new matter has been contributed by Mr. Groves in the description of recent experiments, as in Dr. Frankland's work on the luminosity of flames, &c. The metallurgy of iron, also, which is very fully described, occupies a considerable portion of the part devoted to the consideration of the metals, but not, however, to the exclusion of other important matters connected with these bodies. Mention is also made of the two recently-discovered metals gallium and davium.

Competing as this work must necessarily do with other large and recently written text-books, we should have liked to see a little freshening-up given to some of the diagrams, the apparatus in some of the illustrations appearing of rather an antiquated form. As an illustration of this point we might give the diagram illustrating the manufacture of sulphuric acid on the small scale, as shown in Fig. 322. A considerable improvement in the letter-press has, however, been introduced by printing the headings of the paragraphs in larger type than has hitherto been used; this, combined with the references to the original papers from which the information contained in the work has been taken, will, we think, prove of great value to the more advanced student.

The work throughout bears evidence of thorough as well as careful selection in regard to the new material introduced, and we think the publishers are to be congratulated on obtaining the services of such a conscientious worker as Mr. Groves for the revision of this standard text-book.

OUR BOOK SHELF

A Glossary of Biological, Anatomical, and Physiological Forms. By Thomas Dunman. (London: Griffith and Farran, 1878.)

MR. DUNMAN'S glossary is the result of an attempt "to place before the student the pronunciation, derivation, and definition" of the terms "usually employed in that department of biological science which treats of animal life, as set forth in standard text-books of Huxley, Carpenter, Foster, Flower, and others," and will be a useful book, no doubt—the more so as there is no other work covering exactly the same ground. At the same time the derivations and definitions appended to the terms are not always quite correctly given, particularly as regards the zoological terms. The order of birds called "Dromæognathæ" was so named by Prof. Huxley because the Tinamous which compose it have the palate formed like that of the ostriches (*Dromæus*, an Emeu)—not from the Greek "dromaios" directly. "Holothuridea" is from Holothourion—a good Greek Aristotelian word—and has certainly nothing to do with "thuris," a little door, as Mr. Dunman would have us believe. A more probable derivation is θοῦριος, *furiosus*, because the Holothuria burst in pieces when touched. There are no such Greek verbs as "πνέυσω, I breathe" (given under Pharyngnopneusta), or "πτόω, I fall" (given under ptosis). The correct Greek derivations in these cases are πνέω and πίπτω. "Egesta" is not formed from "*egestio*—getting-rid-of," but is simply the participle of *egero*, meaning such things as are got rid of. Nor are Mr. Dunman's explanations of the purely anatomical terms always faultless, although there is less occasion for criticism here. The "*ligamentum nuchæ*" is formed of elastic not of "connective" tissue. The "sectorial" tooth of the dog is certainly not definable as the fourth premolar, for the dog has no fourth premolar. The "tri-

facial pair of nerves" are not so called because they arise by three pairs of roots, but because they send three main branches to different parts of the face. We doubt "amnion" having anything to do with "amnos—a lamb." It is an old classical word for one of the fœtal membranes, as may be seen by reference to a lexicon. Lastly, we may remark that "hernia" is very imperfectly, not to say incorrectly, described by Mr. Dunman.

It would not be difficult to pick more holes in Mr. Dunman's volume—which, however, in spite of some defects, will be serviceable to the persons for whose aid it is designed.

The Native Flowers and Ferns of the United States. By Thomas Meehan. Illustrated by Chromolithographs. Issued by Subscription. (Boston: L. Prang and Co., 1878.)

THIS book is intended to be "an anthology in the truest sense of the word," and aims at culling the most beautiful, interesting, and important from the vast number of plants found in the enormous region ruled by President Hayes. Further, it is not merely scientific; a familiar treatment is adopted so that the cultivator and mere lover of flowers may derive both profit and instruction. The first parts which we have received lead us to think that the editor has hit upon a good working way of carrying out his intentions. The text is very readable, the printing is most excellent, and the name of Prang as publisher tells those who know that the chromolithographic part cannot be excelled.

Magnetism and Electricity for Schools and Science Classes. By George Porter. (Belfast: William Mullan and Son, 1878.)

SO many school manuals of Electricity and Magnetism have appeared during the last ten years, particularly since the establishment of the South Kensington Science Examinations, that one is led to question the advisability of adding to their number. The existing manuals are, as a rule, as complete and comprehensive as works of their size and price can be, and until new facts are discovered, or new methods of treating old facts are in vogue, we do not see the necessity of multiplying such text-books. The work before us does not present any special feature. It is suitable for low Forms in a Public School, and for the elementary examination in Electricity at South Kensington. It is cheap and sufficiently illustrated, but occasionally insufficiently explicit for young boys. The chapter on Terrestrial Magnetism might with advantage be somewhat enlarged, and would be distinctly improved by the addition of one or two simple figures.

La Revue Magnétique, Organe du Cercle Electro-magnétique de Paris. Rédacteur-en-Chef, H. Durville. Nos. 1, 5, 6. (Paris, 1878.)

WE have received the above three numbers, the first of which was published on April 16. Acting upon the saying *ex pede Herculem*, we give an extract or two from the "A nos lecteurs." "Il ne faut pas considérer le magnétisme comme une panacée universelle; mais son emploi peut rendre d'immenses services à l'humanité. Sans remplacer complètement la médecine, il peut la seconder puissamment, dans les maladies même les plus désespérées. Quand il sera bien compris de tous, dans la plupart des cas le père de famille deviendra le meilleur médecin de ses enfants; le frère traitera son frère; l'ami, son ami; et tous travailleront alors au progrès de tous. En publiant la *Revue Magnétique*, nous avons l'intention de rallier tous les éléments qui divergent encore autour de la doctrine; d'affermir celle-ci sur une base inébranlable, et de découvrir les secrets les plus cachés de la nature pour les utiliser au profit de l'humanité. . . . Nous ne reculerons devant aucun sacrifice; nous répondrons à toutes les objections qui nous seront posées, et nous insérerons avec empressement les articles qui nous seront adressés. Flétrissant de toutes nos forces le charlatanisme partout où nous le rencontrerons; combattant les abus de toutes nos forces, nous enseignerons le MAGNÉTISME comme une œuvre de science et de charité." These are fine professions. There are articles on magnetism and somnambulism, an unpublished manuscript of Mesmer, and other short notes. A paper by the Editor, entitled, "Les Nombres considérés dans leurs rapports avec les Sciences occultes et les différents Cultes," runs through all the numbers, and contains much curious matter, which, from the style in places, reminds us of the *brochures* we have noticed in these columns from the pen of the Comte L. Hugo. To an outsider the journal appears strange; it carries on its face a good motto from Bossuet: "La Vérité est un bien commun; quiconque la possède la doit à ses frères."

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

The Zoological Record

DR. HOEK'S complaint in NATURE, vol. xviii. p. 569, that not much care had been taken in the record as to zoological papers published in the Netherlands in 1875 and 1876, doubtless appears well grounded to himself from his locally restricted view, but, when analysed, is scarcely of sufficient importance to call for more than an expression of regret that a certain percentage of omission and error must inevitably occur in any large undertaking. No one of the recorders fancies that his work is complete, and the omissions of any one year are constantly supplemented in the following volumes, our best thanks being given to those who will take the trouble to put us right.

The copy of Dr. Hoek's paper, "Die Zoologie in den Niederlanden," sent for our guidance, was duly received after the usual delays, and I beg to thank him publicly for his forethought and kindness in presenting it. It bore the date 1877, and, if it had contained matter to be recorded, would be included in the volume for that year now being compiled. But, being bibliographical only, its use is as an index to the different recorders. It contains notices of eighty-seven papers, on no particular branch of zoology, and referring to no particular locality, but merely published in the Netherlands in 1875 and 1876. Of these eighty-seven papers, I find only seventeen not given in the records for those years; and, as no less than ten of these were published in the latter year, the delay of one volume only is caused by the omission. One of the seventeen (by Benjamins) is purely histological, and not in our scope; four others (by De Man and Winkler) are palæontological, and two at least of the rest are of the most unimportant nature (by De Graaf and Everts). The remainder are one in Mammalia (but more correctly to be referred to Vermes), one in Pisces, two in Mollusca, one in Insecta (Lepidoptera), and five in Crustacea, so that the charge of want of care, when shared by the recorders of these divisions, appears founded upon a very slight basis.

A certain querulousness in Dr. Hoek's last sentence is probably to be attributed to the incompleteness in the record of Crustacea, the five omissa in which are unfortunately all his own papers. (I observe that four papers of Dr. Hoek's on Crustacea are included in the records for 1875 and 1876, though apparently not the same as these.) Dr. von Martens, the recorder of that division, would, however, be the first to deplore this, and to feel the loss to himself as a worker; and his reputation for accuracy and fidelity is too great to suffer from this *lapsus*.

It may be mentioned, also, that two of the omissa are contained in Academical Inaugural Dissertations (which it is needless to explain are practically never published); and that another of them is published in the papers of the Zeeuwsch Society of Sciences (Middelburg), which, to say the least, are not in the hands of every working naturalist in this country.

October 1 E. C. RYE

Intra-Mercurial Planets

THE places sent you of the objects which I designated by (*a*) and (*b*) in my observations during the total eclipse on July 29 were derived from the hurried readings of the circles made immediately upon my return from the Eclipse Expedition, in order to be able to answer numerous inquiries addressed to me for information in regard to these observations. Subsequently I made a careful determination, and the readings of the circles and all the data for a definitive reduction of the observations were communicated to astronomers in this country and in Europe. These have probably already come to your knowledge and need not be repeated here.

The only outstanding question in regard to the place of the star which I designated by (*b*), is whether any disturbance of the telescope by the wind is to be feared. The position was marked on the hour circle first, and but a moment was occupied in passing from the eye-piece to the place where this was done. The wind was blowing fresh from a direction south of west, but our telescopes were, as you know, well sheltered by the semi-circular ledge behind which we observed. My own instrument was near the ledge on the west, and was more completely protected than any of the others, and hence it became desirable to know whether any such disturbance of their instruments was noticed by Prof. Newcomb, Commander Sampson, and Lieut. Bowman, who observed near me. Accordingly, I addressed letters to these gentlemen for information upon this point. Prof. Newcomb read the circles of his instrument for a pointing made at about the same instant, which proved to be on a fixed star, and there was no disturbance whatever of the position of his telescope. Lieut. Bowman says that there was no disturbance of his instrument by the wind during the totality, and Commander Sampson says that his assistant, Dr. Dewitt, who pointed for him while using the spectroscope, did not notice any disturbance.

These reports might be regarded as conclusive upon the question which I raised, when I came to reduce the observations, since otherwise the star (*b*) could not be ζ Cancri. If the totality had only lasted a few seconds longer I might have moved out to ζ Cancri, and by observing it also there would have been no uncertainty whatever. But I hardly realised at the time the possibility of there being two planets near the sun, and being sure of one, I gave more attention to it. The record of (*b*) was made just before the sun reappeared. In fact, the sun came out just as I turned to go to the eye-piece again, and anxious to have Prof. Newcomb's telescope also directed on (*a*), I ran across to where he was observing, but his telescope being then directed toward a suspicious object, for which he was reading the circles, it could not be disturbed. Returning to my own instrument it was too late to re-observe (*b*) or to find ζ Cancri, and I could not then determine whether the object observed was a stranger or not. It was very much brighter than I expected to see ζ Cancri, judging from the appearance of δ Cancri, which I had seen in a preceding sweep.

In order to obtain further evidence as to the stability of the instrument, I have made careful experiments with it, clamped as it was then, and I find that the danger feared has no significance whatever. During the present week, also, there have been days when the wind was blowing very strong from the same direction as on the day of the eclipse, and I have placed the telescope in the position as to direction in which it then was, but fully exposed to the wind, and it has remained hours at a time thus exposed without the pointing being sensibly changed. I conclude, therefore, that the object which I designated by (*b*) is also a new star.

I have lately examined, on two mornings, the stars in that part of Cancer, and my recollection of the appearance of the stars (*a*) and (*b*) being still vivid, I have compared, with the same telescope and magnifying power, the stars which I then observed in the vicinity of the sun. The moon shining brightly in the west, and the bright twilight in the east, gave a sky-illumination in some respects similar to that at the totality of the eclipse. By observing when the approaching daylight had extinguished the light of two small stars which I saw on July 29 east of the sun, so that they were just visible in the telescope as they were on that day, I proceeded to compare the light of θ and ζ Cancri. As a result of this examination, I am convinced that I under-estimated the magnitudes at the time. I think that (*a*) must be classed as good fourth magnitude, and (*b*) as third magnitude, if not brighter. JAMES C. WATSON

P.S.—I have begun some calculations, but being pressed just now in the preparation of elements, perturbations, and ephemerides of ten or twelve of the minor planets for the *Berliner Astr. Jahrbuch*, I have not yet progressed very far. It is probable that M. Gaillot will have worked up all the material available for this. J. C. W.

Ann Arbor, September 21

Sun-spots and Weather

IN the last number of NATURE (p. 567) there is a very interesting communication from Mr. Fred. Chambers of Bombay. He shows that the barometric pressure at Bombay when graphically exhibited for a series of years, gives a curve which is very similar to the sun-spot curve, and he remarks that the barometric curve lags behind the sun-spot curve particularly in the years of maximum sun-spots. He argues that the sun is probably hottest at times of maximum sun-spots. I have grounds for thinking that I found traces of a somewhat similar relation in discussing the daily range of the thermometer at Kew Observatory, although the results obtained were not so definite as those of Mr. Fred. Chambers.

When, however, we go from the meteorological to the magnetical influences of the sun we find a very marked and well-known relation between the sun-spot areas and the magnitude of the diurnal range of declination,—this diurnal range being unmistakably greater when there are most spots. Here also the lagging behind comes prominently out whatever may be its cause.

Mr. Frederick Chambers quotes the following remark made by me (NATURE, vol. xvii. p. 326):—

"It is nearly, if not absolutely, impossible from observations already made, to tell whether the sun be hotter or colder as a whole when there are most spots on his surface. The sooner we get to know this the better for our problem."

I ought here to mention that in these words I referred more particularly to *direct* observations of the heating effect of our luminary. I ought also to state that the fundamental importance of such observations was impressed upon me by the remarks of a very distinguished physicist, who considers that a persistent and well-organised attempt should be made to determine by means of actinometric observations whether our luminary is in reality of variable heating power.

We know a good deal about sun-spots, although not nearly so much as we ought, but we know next to nothing about the variations (if any) in the direct heating effect of the sun. I can only here repeat what I said before, that "the sooner we get to know this the better for our problem." BALFOUR STEWART

Manchester, September 27

Cyclones and the Winter Gales of Europe

THE following figures may interest some of your readers as a contribution to the theory put forward in NATURE, vol. xvi. p. 505, regarding the meteorological effects of variations in the intensity of solar radiation, and of the consequent changes in terrestrial temperature.

According to this theory, the high temperature which generally coincides with sun-spot minima should have the effect of increasing the steadiness and velocity of the prevailing winds of the globe, whilst, at the opposite epoch of the solar cycle, the weakness and unsteadiness of these currents ought to give rise to heavy rain on the coasts and islands of the tropics, and to facilitate the generation of cyclones, which (as has been shown by Messrs. Blanford and Eliot in the case of the Bay of Bengal), are most probably caused by the condensation of aqueous vapour over the place of its production. If this view of the action of variations in temperature upon the convection currents of the atmosphere be the true one, it follows that the south-westerly gales of Europe should be most frequent and powerful at times when the cyclones of the West Indies are least frequent. This is borne out by the accompanying table, which shows the number of hours in each year during which the wind-velocity in the British Isles exceeded thirty miles, as compared with the number of cyclones in the West Indies, according to Poëy. The figures in the second line are taken from the *Quarterly Weather Reports*, and represent the averages of the annual totals for Valencia, Armagh, Glasgow, Aberdeen, Sandwick, Falmouth, Stonyhurst, and Kew. These are the only stations which give a continuous register for the six years in the table.

	1869	1870	1871	1872	1873	1874
Hours of high wind (British Isles) ...	714	570	537	679	571	658
Cyclones (W. Indies)	0	7	3	0	1	?

The period covered by the table is too short to afford any ground for a definite induction; but, as far as they go, these figures afford *primâ facie* evidence in favour of the theory.

I may add that the probability of this relation between the gales of the temperate zones and the cyclones of the tropics has been pointed out on purely speculative grounds by Mr. E. D. Archibald, in a pamphlet on the rainfall of the world, recently published by him in India. S. A. HILL

Allahabad, September 11

Magnetic Storm, May 14, 1878

IN the *Bulletin Mensuel* of the observatory of Zi-ka-wei, near Chang-hai, China, the following interesting remark occurs in the number for May, 1878:—

"Durant le mois de Mai, une seule perturbation a été enregistrée par le magnétophotographe; elle commença le 14, à 2h. 20m. de l'après-midi, et se termina 24 heures après; ce jour-là la déclinaison, par extraordinaire, ne présenta pas d'oscillation diurne normale, mais de très-nombreuses petites oscillations comme le bifilaire."

On examining the photographic trace of the Stonyhurst magnetograph I find that the only magnetic storm in May last commenced on the 14th at 6h. 4m., and lasted rather more than twenty-four hours. The longitude of Zi-ka-wei being 8h. 15m. 38s. E. of Stonyhurst, the storm began at the same time at both stations. The character of the movement was also identical, for the only disturbance at Stonyhurst from 6 A.M. until 4 P.M. was a tremulous motion of the declination and horizontal force magnets.

The storm was at its height at midnight, when all the magnets were much disturbed, and the vertical force magnet was thrown completely off its balance.

It is impossible to obtain more than the roughest outline of a magnetic storm from hourly readings, but even this slight datum from China shows a general agreement in the declination curves during the storm at these two distant stations.

The *Monthly Record* of the Melbourne Observatory also mentions the same magnetic disturbance, which commenced there at 4 P.M. on the 14th, and lasted until 8 A.M. on the 17th. Melbourne lies 9h. 39m. 54·8s. E. of Greenwich, and therefore 1h. 34m. 10s. E. of Zi-ka-wei. We thus see that the Zi-ka-wei storm commenced at 3h. 54m. Melbourne time: it was therefore simultaneous at the three observatories.

Stonyhurst Observatory, September 27 S. J. PERRY

Winds and Currents in the Pacific

THE occasional prevalence of westerly winds and of currents setting east in the intertropical portion of the Pacific, has such an important bearing on the possible eastward migrations of the Polynesians that I think the following, which I take from the *Samoa Times* for April 20, is worth recording in NATURE.

The brigantine *Ryno* is reported as having arrived at Apia, Samoa, on April 16, from the Tokelau, or Union Group. She was among those islands from March 1 to April 11, and, while there, she "experienced a succession of strong westerly and north-west winds, with high sea and frequent squalls and much rain, which made sad havoc amongst the vessel's canvas. Capt. Bower states that, when off Tokelau, he found a strong current setting to the eastward at the rate of two miles per hour. The *Ryno* left Tokelau on April 11, had fine E.S.E. weather, and no current whatever." S. J. WHITMEE

Blackheath, September 27

Blackburn's Double Pendulum

I TRUST I may be pardoned for observing, in reference to Prof. A. M. Mayer's description of the curves produced by Blackburn's Double Pendulum (NATURE, vol. xviii. p. 594), that a typical series of those curves was placed before the readers of NATURE in the year 1871 (vol. iv. pp. 310, 370), in illustration of a paper of mine on "Pendulum Autographs."

Prof. Mayer adheres to the funnel and sand as the mode of laying the curve on paper. The sand-trail thus left is necessarily rather coarse, and cannot be conveniently preserved. A more delicate and more enduring trace, in ink, can be obtained by the use of a tubular glass pen, as described by me in the paper above-mentioned. Only the increased friction makes it necessary to use a heavier bob. HUBERT AIRY

Circulating Decimal Fractions

As a supplement to the interesting properties of circulating decimal fractions which have been published in two recent numbers of NATURE, I give you the following, which I think is sufficiently curious to merit attention:—

If the decimal fraction equal to $1 \div n$ *recur in a cycle of* $n - 1$ *digits, the average value of the digits is constant, viz.,* $4\frac{1}{2}$ *for all values of* n; *in other words, the sum of the digits of the cycle is* $4\frac{1}{2}(n-1)$.

For example,—

$$\tfrac{1}{7} = \dot{1}4285\dot{7},$$

and $(1 + 4 + 2 + 8 + 5 + 7) \div 6 = 27 \div 6 = 4\frac{1}{2}$.

Again,

$$\tfrac{1}{17} = \dot{0}58823529411764\dot{7},$$

and $(0 + 5 + 8 + 8 + \text{\&c.}) \div 16 = 72 \div 16 = 4\frac{1}{2}$.

The theorem is easily established from the two facts (1) that $1 \div n$, $2 \div n$, $3 \div n$, ... have the same digits in their cycles, and (2) that the sum of $1, 2, 3, \ldots, n-1$ is $\frac{1}{2}n(n-1)$.

The properties of circulating decimal fractions have been often studied from the time of Wallis downwards, and very probably those lately and now brought forward have been noted before, but have lain entombed in some out-of-the-way corner since. However, until a full index to mathematical literature is prepared, or exhaustive monographs on special departments like this are written, such resurrections are very desirable.

If either of your two previous correspondents on the subject would care to undertake a full examination of it I should be glad to furnish him with my quota of references to the extent of ten or a dozen, and, I have little doubt, other readers of NATURE would give like help. THOMAS MUIR

High School of Glasgow, September 23

[Mr. Muir's theorem is practically contained in the result that the two halves of the period are complementary, whence the sum of the figures is equal to half as many nines as there are figures in the period.—ED.]

An Old Map of Africa

EARLY in the year 1870 I visited the vineyard of Mr. J. L. Cloete at Constantia, near Cape Town.

Among other things of interest Mr. Cloete showed me an old map of Africa done, I think, in Amsterdam. This map had been in the possession of his family from time immemorial. My acquaintance with the geography of Africa was too slight to enable me to pronounce upon its accuracy, but I was greatly surprised to see marked upon it several large lakes and many rivers in the region now so well known to us through recent explorations.

In the critical study of this subject I have thought that a knowledge of the existence of this map, if it be as I remember it, might prove of value. C. F. GOODRICH,

Torpedo Station, Lieut.-Commander, U.S. Navy

Newport, Rhode Island

[We would refer Commander Goodrich to our article on Old Maps of Africa, in NATURE, vol. xviii. p. 149.—ED.]

Earth Pillars

A LETTER in your issue for September 26 (p. 569) refers to miniature earth pillars seen in the Tyrol. But there is no need to travel out of the country to obtain these mimic representations, at any rate on a moderate scale. Twice I have found them, formed by the drops from railway bridges, upon bare clays; and once in a half-finished building, in a sand-heap containing numerous pebbles. Evidently the heavy drops and the protection from driving rain favoured the result. One of the bridges, between Shepton Mallett and Wells, Som., covered Lias clays, protected by cinders, &c. The other instance, the bridge on the new North Approach just outside York Station, is the more interesting in its mimicry of the original Botzen earth pillars, since small boulders and pebbles in the glacial clays form the caps and shoulders in the Lilliputian columns.

September 28 J. EDMUND CLARK

White Swallows

Mr. Herbert W. Page, in Nature, vol. xviii. p. 540, refers to the rare occurrence of a white *Hirundo riparia*.

I may mention that two white swallows of that species were captured in a high bank of sand at Hungryside, on the Forth and Clyde Canal, by Mr. Martin, of that place, in August, 1876, which I exhibited at a meeting of the Natural History Society of Glasgow, August 26, 1878. Both birds were white all over, with the exception of a brown tinge on the back of one of them. Mr. Stevenson, in his "Birds of Norfolk," vol. i. p. 342, mentions a light cream-coloured variety got at Eaton in July, 1861, and another at Weasenham in the following September.

David Robertson, Jun.

Glasgow, September 27

Secondary Lunar Rainbow

During the very stormy and unsettled weather we had about a fortnight ago, I was one of a party of friends on a visit at a country-house near Huntly, about forty miles from this, who were witnesses, on the evening of Sunday, 15th inst., to a very striking, and, as a friend well versed in meteorology has since informed me, very uncommon phenomenon. It was that not only of a complete and brilliant primary lunar rainbow with colours, but also, a few minutes later, of a complete and well-defined, but, of course, less brilliant, *secondary* bow. No trace of colour could be observed on the secondary, but, inside the primary, the space seemed, in contrast with the faint moonlight, even more brilliantly and uniformly illuminated than I recollect ever to have seen it within a solar rainbow. The time was about eleven o'clock, and the centre of the bows, therefore, bore about W. or a little to the N. of W.

R. Walker

Aberdeen, September 30

BONE CAVES IN STYRIA[1]

1. TWO prehistoric implements, found some years ago in the "Badel" Cave, near Peggau, in Styria, are preserved in the Museum of the Johanneum, at Gratz. Count G. Wurmbrand has lately conducted an exploration of this cave. The floor is formed for the most part of a layer of fragmentary stalagmite, about seven centimetres thick, resting on a loam, with bones and gravel, forty-three to seventy centimetres thick. A peculiar laminated and very compact loam, permeated by a blackish substance (*pyrolusite*) lies on the rocky bottom. In the loam have been found abundant bones and teeth of *Ursus spelæus*, undeterminable remains of rodents, a canine of a stag, bones gnawed by *hyæna*, &c., but no human remains or relics.

2. In a cave on the north slope of the Erzberg, east of Wildalpe, in Upper Styria, Dr. A. Redtenbacher has found abundance of bones and teeth of *Ursus spelæus*. All these bones, except the phalanges, were broken; and the long ones of the extremities were split longitudinally.

3. On July 1 of this year, Dr. R. Hoernes and Dr. R. von Fleischhaker visited the Drachenhöhle (cave of Dragons) near Mixniz, about 1,292 (Austrian) feet above the town, and extending 1,440 feet into the rock, with an entrance 90 feet high and 72 feet wide. The floor of the cave is mostly covered with reddish-yellow cave-loam; but rock-fragments abound within the entrance. The superficial loam had been searched long ago. The inner parts of the cave, accessible only by ladders, were nearly untouched. On the day mentioned, numerous remains of *Ursus spelæus*, both young and adult, fragments of jaws with teeth, single teeth, long bones, ribs, phalanges, &c., were soon found. Some of the teeth and bones in the terminal fissure of the cave could have been brought there only by moving water. No other animal but the cave-bear was represented by the remains met with.

In the portion of the cave next the entrance, a still untouched layer of fragmentary stalagmite, including angular pieces of rock (some very large) was found beneath an accumulation of rock-fragments. Underneath it is an irregular dark-brown and nearly black stratum, about 15 centimetres thick, containing charcoal and partially burnt bones; and in a brown loam, immediately beneath, similar objects were abundant. These bones, differing from those of the yellowish loam of the inner cave, by their greater consistence and dark colour, belong mostly to *Ursus spelæus*, and a few to an undetermined ungulate. The long bones are nearly all broken. Among these dark-coloured bones are phalanges, cervical vertebræ, and the lower portion of a right humerus measuring 16 centimetres between the condyles, which must have belonged to a colossal individual. The connection of the cervical vertebræ (the atlas bearing traces of fire), and the presence of several other bones of this individual (as well as those of smaller ones) lead to the conclusion that it was brought into the cave after being killed, and prepared there for the food of the cave-folk. No implements were found.

[1] Imperial Geological Institute of Vienna Report, July 31, 1878.

OUR ASTRONOMICAL COLUMN

The Satellites of Mars.—The results of the complete reduction and discussion of the Washington observations of the satellites of Mars, undertaken by their discoverer, Prof. Asaph Hall, have just been published by the United States Naval Observatory. The observations of the outer satellite *Deimos* extend from 1877, August 11, to October 31, and those of the inner one, *Phobos*, from August 17 to October 15. On November 7 and 12 the satellites were looked for, but could not be seen. In treating these observations with the view to the determination of the most probable orbital elements of the satellites, Prof. Hall assumes in the first place that their paths are circular, and by a graphical projection finds the angle between the orbit-plane and the plane perpendicular to the line of sight, as well as the angle of position of the major axis of the ellipse into which the circular path is projected. The resulting elements are then compared with the observations, and elliptical elements deduced from the resulting differences between calculation and observation, both for position and distance, by means of equations of condition. Thus the following values of the elliptical elements have been obtained from the Washington observations alone:—

Epoch 1877, August 28·0 Greenwich Mean Time.

	Deimos.	Phobos.
	h. m. s.	h. m. s.
Period of revolution	30 17 53·86	7 39 15·07
Semi-axis major at distance unity ...	32″·3541	12″·9531
Ascending node on equator	48° 5′·7	47° 13′·2
Inclination to equator	35° 38′·7	36° 47′·1
Angle between the lines of nodes and apsides	40° 53′·6	45° 30′·4
Distance from node at epoch	357° 30′·5	285° 20′·2
Eccentricity	0·005741	0·032079

It will be seen that the planes of the orbits of the satellites are nearly coincident with the equator of Mars, the ascending node of which for the above epoch is in 47° 56′, and the inclination 39° 45′. Prof. Hall considers the elements to be determined with tolerable accuracy, with the exception of the periodic times, which remain to be decided within closer limits from the observations of another opposition; nevertheless they are sufficiently exact to carry forward an ephemeris to 1879. The eccentricity in the case of *Deimos* is so small that for most purposes of calculation circular elements will suffice. In the case of *Phobos*, Prof. Hall thinks the eccentricity has a real existence, every comparison of distance with the circular orbit confirming it.

The mass of the planet deduced from the measures of *Deimos* is $\frac{1}{3095313}$, and from those of *Phobos* $\frac{1}{3078456}$, that of the sun being taken as unity; the adopted mean value

is $\frac{1}{3093500}$. The latest value of the mass resulting from theory is that given by Leverrier (*Annales*, vol. xi. p. 3), viz., $\frac{1}{2812526}$.

Prof. Hall compares his elements with the observations made at other observatories, of which those of Cambridge, U.S., Glasgow, U.S., Pulkowa, and Mr. A. Common, of Ealing, were the most successful. The magnitudes of the satellites, free from the glare of the planet, were estimated at about 12 and $11\frac{1}{2}$, *Phobos* being the brighter of the two. On October 15, when their distances from the centre of the planet were respectively 23″ and 57″·5, they were of nearly equal brightness. Prof. Hall further remarks : "The chief difficulty of observing these satellites is on account of the brilliancy of the planet. At their elongations at opposition in 1877, they appeared to me brighter than the outer satellites of Uranus and much brighter than Hyperion, and on October 31 Deimos resembled Umbriel, the second satellite of Uranus, which, at elongation, is fainter, I think, than any other satellite."

With the angular values of the mean distances given above, the real distances of the satellites from the centre of their primary will be for *Phobos*, 5,800, and for *Deimos*, 14,500 miles ; the former, from the rapidity of its motion (it performs more than three revolutions in the Martian day) will appear to rise in the west, meeting and passing the outer moon, and setting in the east, and it will have a horizontal parallax amounting to 21°.

Prof. Hall has definitively adopted the names proposed by Mr. Madan, of Eton, in the columns of NATURE.

THE SATURNIAN SATELLITE, TITAN.—The following are the approximate times of conjunction of the great satellite of Saturn, with the perpendicular to the plane of the ring, during the next month, at which times occultations by the planet, or transits over its disc take place. The elements used are very nearly those of Bessel, but with a somewhat larger ellipticity of the planet.

	G.M.T. of conjunction. h.	Angle of position. °	Distance from Saturn's centre. ″	Saturn's polar semi-diameter. ″	
Oct. 8	13·9	4·2	8·17	8·69	On disc ?
,, 16	5·7	184·3	7·32	8·64	Occulted.
,, 24	11·7	4·3	6·53	8·58	On disc.
Nov. 1	3·5	184·4	5·91	8·50	Occulted.
,, 9	9·6	4·4	5·42	8·40	On disc.

WINGLESS INSECTS OF THE FALKLAND ISLANDS

WHILST on an excursion to Port Darwin, in the Falkland Islands, during the visit of H.M.S. *Challenger* to that group, I found at Darwin Harbour, Choiseul Sound, some insects which are of considerable interest, since, as I believe, they are closely allied to those of Kerguelen's Land. Amongst them were a gnat, practically wingless (Tipulidæ), and a fly with rudimentary wings. The gnats were found crawling on the rocks on the sea-shore, in sheltered places, and also on the sunny, sheltered side of a fence composed of a peat bank, with furze growing on the top of it. They run quickly, and, when in danger, draw up their legs and drop in order to escape, and they are not by any means easy to catch amongst the grass.

The flies were found only on the sea-coast, in hollows under overhanging slabs of the sandstone rocks, sheltering themselves in crevices. They have short wings, which they seem to use in jumping, and they spring nimbly, like fleas or small grasshoppers, and are difficult to catch. On comparing specimens of these flies with specimens of *Amalopteryx maritima*, one of the flies of Kerguelen's Land, with rudimentary wings, described by Mr. Eaton (Rev. E. A. Eaton, the *Entomologist's Monthly Magazine*, August, 1875), I have little doubt that they are very closely allied to this species, and to be referred at least to the same genus. Dr. Kidder describes the habits of the Kerguelen *Amalopteryx* as closely similar (J. H. Kidder, M.D., *Bulletin United States National Museum*, No. 3, 1876, ii. p. 52). Von Willemoes Suhm found a species of the same genus in Marion Island, and we all observed the fly at Kerguelen's Land as well as the wingless gnat which Mr. Eaton has named *Halyritus amphibius*, and which lives on the Kerguelen sea-shore amongst sea-weed constantly wetted by the tide. It would be interesting if the Falkland Island gnat proved allied to the Kerguelen one on further examination.

I found one beetle with wings at the same locality in the Falklands, and one wingless species. All the Kerguelen beetles are wingless. Two genera and all the species of that island are endemic (Mr. C. O. Waterhouse, *Entomologist's Monthly Magazine*, August, 1875, p. 50). The close connection between the Fuegian flora and that of the far distant Kerguelen's Land is well known from the investigations of Sir Joseph Hooker. It is interesting to find a further connection in the insects. The four wingless flies of Kerguelen's Land are assigned by Mr. Eaton to four new genera. I believe, though I am no entomologist, that the Falkland Island and Marion Island fly will come under one of these, and possibly further search may prove the existence of representatives of some of the other genera in Fuegia or the Falklands. I see from the "Histoire Nat. des Insectes Diptères" of the Suitesa Buffon, the only authority immediately at hand, that a wingless gnat, *Chionea araneoides*, is found in Sweden in woods on the snow throughout the winter, whilst two flies with rudimentary wings, *Apterina pedestris* of France and Germany, and *Myrmemorpha brachyptera* of Spain, exist in Europe. These merely as examples. Prof. Westwood tells me many other such diptera are known to entomologists, and he has shown me a specimen of a wingless fly, *Borborus apterus*, which occurs in England.

Exeter College, Oxford H. N. MOSELEY

SUN AND EARTH

THE Meteorological Reporter of the Bombay Presidency, Mr. F. Chambers, at the end of his recently-published report for 1877-78 gives us the first results of some important researches which he has not yet been able to complete, owing to want of clerical assistance. In a brief sketch of the meteorology of the Bombay Presidency in 1876 prepared for the Bombay Administration Report for 1876-77, he showed that the abnormal meteorological conditions which produced the famine of 1877 were of the same type as those which produce the usual alternations of seasons, and therefore are attributable to similar causes.

A commencement was made some time ago with the discussion of the Kurrachee wind observations. One of the most important results already obtained is that the numerical relation existing between the abnormal wind and abnormal barometric movements is exactly similar to the relation between the *annual* variations of the wind and barometer leading to the same conclusion as above, viz., that most of the abnormal variations of weather in India are due to causes which are similar to, if not identical with, those which produce the normal variations. The comparison of the normal and abnormal barometric movements at different stations points decidedly in the same direction, and Mr. Chambers believes that further investigation will prove this to be a general law, affecting perhaps all abnormal meteorological variations which are not cyclonic. He adds :—

"The fact that a famine has been raging in China, at the same time that one has ravaged Southern India, does not appear to be without its significance, in pointing to

other than local causes of the meteorological disturbances which produce famines. Indeed, the inference that most of the unusual variations of weather in tropical climates are induced by corresponding variations in the *absolute* heating power of the sun, in the *same manner* that the seasonal variations are induced by those changes of heating power which depend on the relative *motions* of that body, seems almost irresistible, if it may not be regarded as already partly proved. The importance of this conclusion, if true, will be readily admitted, for it will be at once apparent, that if the absolute variations of the sun's heat are fitful in their occurrence, and do not obey definite periodical laws, it will perhaps never be possible to predict by more than a few days in advance the unseasonal variations of weather induced by them, while if such laws can only be discovered, the possibility of our being able to predict their consequences is equally certain.

OCCURRENCE OF FOSSILIFEROUS TERTIARY ROCKS ON THE GRAND BANK AND GEORGE'S BANK

AMONG the most important results of the investigations made by the party connected with the United States Fish Commission, stationed at Gloucester, Mass., during the present season, is the discovery of fragments of a hitherto unknown geological formation, apparently of great extent, belonging probably to the miocene or later tertiary. The evidence consists of numerous large fragments of eroded, but hard, compact, calcareous sandstone and arenaceous limestone, usually perforated by the burrows of *Saxicava rugosa*, and containing in more or less abundance fossil shells, fragments of lignite, and in one case a spatangoid sea-urchin. Probably nearly one-half of the species are northern forms, still living on the New England coast, while many others are unknown upon our coasts, and are apparently, for the most part, extinct. From George's Bank about a dozen fossiliferous fragments have been obtained, containing more than twenty-five distinct species of shells. Among these one of the most abundant is a large thick bivalve (*Isocardia*) much resembling *Cyprina islandica* in form, but differing in the structure of the hinge. This is not known living. *Mya truncata*, *Ensatella Americana*, and the genuine *Cyprina* are also common, together with a large *Natica*, a *Cyclocardia* (or *Venericardia*) allied to *C. borealis* (Con.), but with smaller ribs, *Cardium islandicum*, and also various other less common forms. These fragments came from various parts of the bank, including the central part, in depths varying from 35 to 70 fathoms, or more.

From Banquereau, N. S., we received one specimen of similar rock, containing abundant fragments of a large bivalve, and about a dozen other species, among which are *Fusus* (*Chrysodomus*) *decemcostatus*, *Latirus albus*, Jeff. (?), unknown species of *Turritella*, &c. From the Grand Bank two similar specimens were received. One of these, from thirty-five fathoms, lat. 44° 30', long. 50° 15', contained numerous specimens of *Cyprina islandica* in good preservation.

In gathering these specimens from the fishermen and working out the specimens Mr. W. Upham has been very active. It will probably be possible hereafter, when these specimens shall have been more fully examined, and more obtained, to give a pretty long list of species, especially from George's Bank.

At present it appears probable that these fragments have been detached from a very extensive submerged tertiary formation, at least several hundreds of miles in length, extending along the outer banks, from off Newfoundland nearly to Cape Cod, and perhaps constituting, in large part, the solid foundations of these remarkable submarine elevations.

A. E. VERRILL

THE BALLOON EXPERIMENTS AT WOOLWICH

THE military balloon experiments at Woolwich have been so far successful, that last week an aëronaut was lifted some 700 feet, to a height, therefore, sufficient for reconnoitring purposes. There is nothing of novelty in this, as a matter of aërial navigation, although it is the first instance, we believe, of any one in this country being raised from the earth by the agency of pure hydrogen, but it is, nevertheless, something to have achieved in the circumstances under which Capt. Templar has been working. Everybody knows that hydrogen is gifted with extraordinary lifting power, just as every chemist is aware that the gas may be produced in the way Capt. Templar produced it, namely, by passing a jet of steam over iron turnings. But the problem under solution was not to send up a hydrogen balloon so much as to discover whether the thing could be done in a haphazard fashion, and with such simple means as an army in the field would be provided with. It is one thing to make hydrogen in the laboratory, and another to make a sufficient supply of it just whenever the commander of an army may order a balloon reconnaissance to be made.

Capt. Templar has practically proved that this can be done. He requires a supply of steam, an improvised furnace of some sort, and a tube filled with iron turnings; given these, he can provide hydrogen sufficient to lift a scout high into the air. The tube at present employed by Capt. Templar is six or eight inches in diameter, and some half-dozen feet long; it is filled loosely with iron turnings and placed in a furnace where it becomes red hot. Steam is now passed through the tube, and hydrogen issues forth, the oxygen from the decomposed steam going to form ferrous oxide. So completely do the iron turnings do their work under these circumstances, that not only is the surface of the metal acted upon, but it is oxidised well-nigh throughout.

Naturally enough, the hydrogen comes away with a good deal of vapour, and, if pure gas is desired, some desiccating arrangement will have to be employed; but so far Capt. Templar has used none. His balloon, which is of lawn, dressed with boiled oil and glue, will contain about 10,000 cubic feet, but last week not more than 9,000 feet of hydrogen was introduced. The gas was generated from the tube at the rate of something like 1,000 cubic feet per hour, and there can be little doubt that, during the long period of filling, a large quantity of the vapour that was mixed with the hydrogen condensed, and ran out of the balloon in the form of water. Pure hydrogen should have a lifting power of 70 lb. per 1,000 feet, or perhaps a little more, but it is hardly likely that gas produced in a rough-and-ready fashion in the field will possess this degree of buoyancy. Still, Capt. Templar was successful in lifting balloon, aëronaut, ballast, and 700 feet of rope—for the ascent was a captive one—by means of 9,000 cubic feet of hydrogen, prepared in the way we have mentioned.

Another point is worthy of note in connection with the experiment. The fabric of the balloon kept the hydrogen imprisoned for a much longer period than had been anticipated. A dozen hours scarcely impaired the buoyancy of the balloon, and by adding yet another waterproof coating it is anticipated that the balloon will remain inflated for four-and-twenty hours.

The next step will be to discover how far it is possible to compress hydrogen so manufactured into cylinders for conveyance in transport waggons, so that a supply of hydrogen may be at hand whenever an ascent is determined upon in the field. Capt. Templar is sanguine of compressing the gas to a fourth of its volume, and thus decreasing its bulk considerably, when the balloon-train is on the march. How far this is practicable experiment only can prove.

THE WEATHER CASE, OR FARMERS' WEATHER INDICATOR[1]

Description

STAND facing and look at the weather case. Now: The right of the case is at your right hand; the left of the case is at your left hand.

The pointer or index at the top of the case (No. 1) slides on the brass arc; it is known as the "Sunset Barometer Index," and indicates, when set by the figures to which it points on the "Main Barometer Scale," which is just below it, the reading of the barometer at the time of the sunset yesterday.

The "Main Barometer Scale" (No. 2) exhibits all the barometric readings likely to be used with this instrument.

The pointer (No. 3) just below the "main barometer scale" is called the "mean barometer index," and indicates, when set, the mean or average reading of the barometer at the place at which the instrument is set and for each separate month. When the barometer reads above or below this reading at any place, such reading is said to be "above the mean" or "below the mean" for that place in that month. This index is set once for each month in the year.

When the barometer pointers go toward the right from this mean or average reading, the barometer is said to be "rising." When the barometer pointers go toward the left from the mean or average reading, the barometer is said to be "falling."

The mean barometer reading for each district for each month is stated in the *Farmers' Bulletin*, or can be had by application to this office.

The long brass hand over the glass face of the barometer is known as the "long pointer," and indicates, by the figures of the "main barometer scale" to which it points when set, the reading of the barometer when last set.

The black pointer on the face of the barometer under the glass face is known as the "short pointer, and indicates the existing pressure of the atmosphere at any time the instrument may be examined.

To Read the Barometer

If the observer stands facing the barometer the "short pointer" (black) moves toward the right as the pressure of the atmosphere (or weight of the air) increases, and to the left as the pressure of the atmosphere (or weight of the air) diminishes. The "long pointer" (brass) should be moved by the turning screw so as to coincide with, or exactly cover, the "short pointer" (black). The barometer is now set for reading, and the "barometer reading" is found by reading from the left to right on the "main barometer scale" from the lowest figures (or readings) on that scale to that exact division or mark upon that scale to which the "long pointer" points or which it covers. The inches and hundredths of inches are marked on the scale. The inches and hundredths are counted from left to right, or in the same direction as the hands of a watch move, and they are counted in the same way as the hours and minutes on a watch-face are counted. The inches and hundredths are written down, if they are to be written, in the same manner as dollars and cents are written, thus: one dollar and seventy-five cents; that is, one dollar and seventy-five hundredths of a dollar would be written $1.75, or one and seventy-five hundredths; $29.35, twenty-nine dollars and thirty-five hundredths. The "long pointer" pointing on the "main barometer scale" to twenty-nine inches and thirty-five hundredths, the barometer-reading would be twenty-nine inches and thirty-five hundredths of an inch, and would be written "29.35 inches," and so for other readings.

[1] Circular issued by the Signal Service of the United States Army—communicated by General Myer, Chief Signal Officer.

Rain Winds and Dry Winds

There are for each place and for each month two kinds of winds:—

First—Winds which, blowing from certain directions, are at that place and in that month more likely than any other winds to be followed by rain. These are called "rain winds."

Second—Winds which, blowing from certain directions, are at that place and in that month less likely than other winds to be followed by rain. These are called "dry winds."

The "rain winds" and the "dry winds" for each district and for each month are stated in the *Farmers' Bulletin*, or can be had by application to this office.

The wind direction for any day or time must be seen and taken at each place or station by a vane as well located as practicable.

The "wind disc" (No. 8) consists of a brass circle, on which slide freely two arcs—a red arc, called the "dry-wind arc" (No. 9), and a blue arc, called the "rain-wind arc" (No. 11). In the centre of the disc is a pointer, turning with a turning-screw, and called the "wind-disc pointer" (No. 10). Around the disc are letters to show directions, as N for north, E for east, NE for north-east, &c.

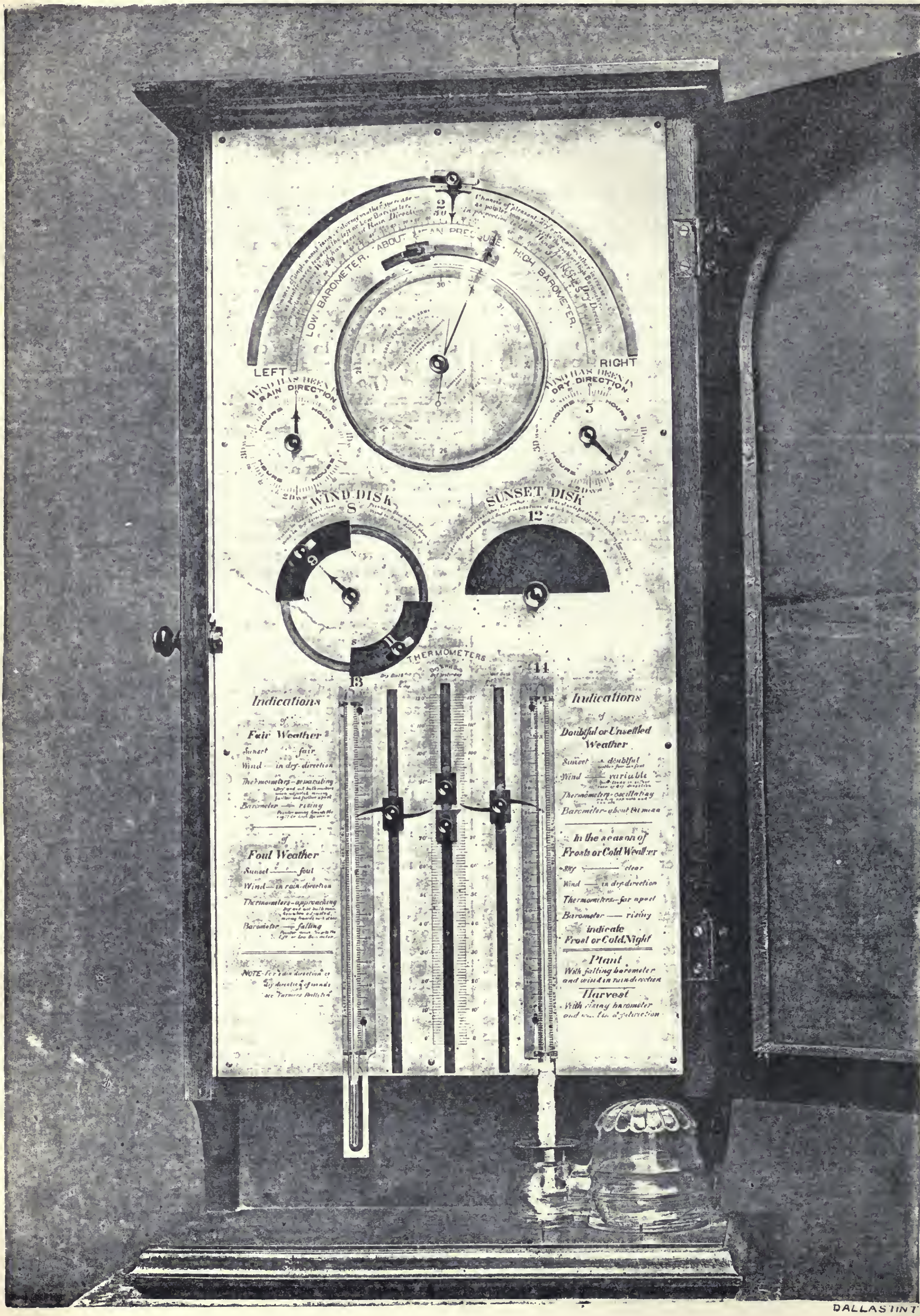
The wind disc is made ready for use as follows:—If, for instance, the *Farmers' Bulletin*, or other report, states that, for any district or place, and for any month, "winds blowing from south or east, or from directions between those points, are found to be the winds most likely to be followed by rain, winds blowing from north or west, or from directions between those points, are found to be the winds least likely to be followed by rain"—or, in other words, "winds blowing from east or south, or from directions between those points, are rain winds. Winds blowing from north or west, or from directions between those points, are dry winds;" then, if the instrument is to be used in that month and in that district or place, the rain-wind arc No. 11 (the blue) is moved on the brass circle until one end of the arc is at the letter E, which stands for "east," and the other is at the letter S, which stands for "south;" the dry-wind arc No. 9 (the red) is moved on the circle until one end of that arc is at the letter N, which stands for "north," and the other is at the letter W, which stands for "west." The arcs remain as they are thus placed for the whole of the month.

At the beginning of the next month the rain-wind direction and dry-wind direction must be located for that month, and the arcs must be again moved on the circle in the same manner until the rain-wind arc and the dry-wind arc touch, respectively, with their ends, the letters for the points named for the rain-winds and the dry-winds for that month. The arcs then remain so placed for that month—so for each month of the year.

Now, when the wind disc is thus ready, and on any day, the weather case is to be used. (1) The direction in which the wind is blowing is seen by a vane. It is noticed from what direction the wind is blowing, as from the north, south, east, &c. (2) The wind disc pointer (No. 10) is moved by the turning-screw to point to the compass-letters on the disc, or between them, showing as nearly as practicable by those letters the same direction, *i.e.*, that *from* which the wind is blowing when observed. If this pointer, so set, points at either of the ends of either arc, or any part of either arc, it shows, if it so points at the *red* arc, that the wind is at that time in the *dry*-wind direction, or is a *dry* wind; if it so points at the *blue* arc that the wind is at that time in the *rain*-wind direction, or is a *rain* wind.

The hour is noted by a clock or watch, and the time at which a rain wind or a dry wind commenced to blow, or was first noticed, is written down.

It must be also noticed if the wind shifts, as from blowing from a rain direction to blowing from a dry direction, or


LOW BAROMETER · ABOUT MEAN PRESSURE · HIGH BAROMETER
LEFT
RIGHT
RAIN DIRECTION
DRY DIRECTION
HOURS
WIND DISK
SUNSET DISK
THERMOMETERS
Indications of Fair Weather
Sunset — fair
Wind — in dry direction
Thermometers — separating
Barometer — rising
of Foul Weather
Sunset — foul
Wind — in rain direction
Thermometers — approaching
Barometer — falling
Indications of Doubtful or Unsettled Weather
Sunset — doubtful
Wind — variable
Thermometers — oscillating
Barometer — about the mean
In the season of Frosts or Cold Weather
Sky — clear
Wind — in dry direction
Thermometers — far apart
Barometer — rising
indicate Frost or Cold Night
Plant
With falling barometer and wind in rain direction
Harvest
With rising barometer

DALLASTINT

is blowing from such direction as not to be within either arc.

The length of time for which the wind has blown continuously from a rain direction or a dry direction is of importance to be considered with other local signs and indications.

The pointer and scale (No. 5) on the right of and below the barometer are called the dry-wind time-record, and the pointer (No. 7) is called the "record-pointer," and indicates, when set, the length of time the wind has been blowing continuously from a "dry" direction, by the figures showing the number of hours on the scale to which it points.

The pointer and scale (No. 4) on the left of and below the barometer are called the rain-wind time-record, and the record-pointer (No. 6) indicates, when set, the length of time the wind has been blowing continuously from a "rain" direction, by the figures showing the number of hours on the scale to which it points.

The record pointer on the rain-wind time-record (No. 6) is always turned by the thumb-screw, and set pointing at the figure 0 on the scale when the wind is not blowing in the rain-wind direction. In the same way the "record-pointer" on the dry-wind time-record (No. 7) is always set pointing at the figure 0 when the wind is not blowing in the dry direction.

When in actual use, the hour at which the *wind-disc pointer* has been set is carefully noted. When the weather case is next examined, the wind-vane is again noticed, and the wind-disc pointer again *examined* or adjusted. If it still continues to point at any part of the same arc as before, the number of hours which have elapsed since the last setting and during which the wind-disc pointer has been so pointing is noticed, and the record-pointer on either the rain-wind time-record or dry-wind time-record is turned to show the *number of hours* the wind has been thus noticed as blowing from the *rain* direction or from the *dry* direction, as the case may be. This proceeding is repeated every time the instrument is set. It can thus be seen at a glance whether the wind is, or not, blowing in a rain-wind or a dry-wind direction, and for how long it has been so blowing. Whenever, on noticing the wind-vane, it is seen that the wind has shifted, the wind-disc pointer is set accordingly If it now points at neither arc, or points to the different arc from that at which it pointed at its last setting, the time-record pointer on the rain-wind time-record or dry-wind time-record (whichever may have been in use at the last setting), is turned to point at the figure 0 (zero).

The wind-disc pointer and the time-record pointer ought to be set thrice daily at least, early in the morning, at noon, and at sunset.

The sunset disc (No. 12) consists of a circular disc, one half of which is coloured red and one half of which is coloured blue. The disc turns upon a central turning-screw in such manner that half of the disc shows through a semi-circular opening in the face of the weather-case. The sunset-disc is set as follows:—At the exact time of every sunset the western sky and the character of the sunset is carefully observed. The examination ought to be minute and careful, lasting for about fifteen minutes. If the sunset sky is clear or red, or markedly what is known as a "fair weather sunset"—a sunset such as is generally held to indicate a clear or fair day to follow on the next day, a day on which it will not rain—the sunset-disc is turned by the turning screw until the semi-circular opening shows all red. The sunset-disc, thus turned, is described as set for a "fair weather sunset."

If the sunset sky (the western) is cloudy or foul, or markedly what is known as a "foul weather sunset," a sunset such as is generally held to indicate foul weather to follow on the next day—a day on which it will rain—the sunset-disc is turned by the turning screw until the semi-circular opening shows all blue. The sunset-disc thus turned, is described as set for a "foul weather sunset." If the appearance of the western sky and the character of the sunset are neither markedly those of a "fair weather sunset" or of a "foul weather sunset," but such as to leave the observer in doubt how to style it, the sunset-disc is turned to show half red and half blue, or "doubtful." The sunset-disc, thus set, is described as set for a "doubtful weather sunset."

The term "fair weather sunset" expresses such condition of the sky, particularly the western, and such character of the sunset, as is considered to indicate a fair day, a day on which it does not rain, for the day ensuing.

The term "foul weather sunset" indicates that the appearances are such as to presage a rainy day, a day on which rain falls, for the ensuing day.

The term "doubtful weather sunset" indicates that the conditions are such as to leave the mind of the observer in doubt as to what the sunset presages for the following day. The indication is considered to be for the period of time from the "sunset" of the day on which the character of the sunset is examined until the "sunset" of the following day.

Dry-bulb and Wet-bulb Thermometers

In the lower part of the weather case there are two thermometers, a dry-bulb thermometer (No. 13) on the left-hand side of the case, and a wet-bulb thermometer (No. 14) on the right-hand side.

The dry-bulb thermometer is like any other thermometer, and shows by its readings the temperature of the air.

The wet-bulb thermometer is one, the bulb of which is kept constantly moist by the water passing up from the glass reservoir, through the wicking which covers the thermometer bulb.

The readings of the dry-bulb thermometer and those of the wet-bulb thermometer, are more and more unlike, or farther and farther "*apart*," as it is called, in proportion as the air contains less and less moisture; that is, is becoming *dryer*.

The readings of the dry-bulb thermometer and those of the wet-bulb thermometer become more and more *alike*—are nearer and nearer together—in proportion as the air contains more and more *moisture*; that is, is becoming saturated or *wet*.

By the side of the dry-bulb thermometer (No. 13) is the dry-bulb pointer which slides on the brass slide (No. 15). By the side of the wet-bulb thermometer is the wet-bulb pointer which slides on the brass slide (No. 16). In the centre of the case is the "dry- and wet-bulb scale," marked on the paper on which is the central brass slide-bar (No. 19), and on this slide move the dry-bulb keeper (No. 17) and the wet-bulb keeper (No. 18). To set the thermometers examine first the dry-bulb thermometer and move the "dry-bulb pointer" (No. 15) on the slide until the outside point is exactly level with the top of the mercury in the thermometer—as near to it as practicable. Examine next the wet-bulb thermometer, and move the wet-bulb pointer (No. 16) on the slide until the outside pointer is exactly level with the top of the mercury in the wet-bulb thermometer, or as near to it as practicable, then turn to the "dry- and wet-bulb scale," and on the "central brass slide-bar" (No. 19) move one of the keepers until it touches, as nearly as possible—is on an exact level with the inside pointer of the "dry-bulb pointer," then move the other keeper until it touches, as nearly as practicable—is on an exact level with the inside pointer of the "wet-bulb pointer." The thermometers are now set, and the difference between their readings can be known by counting on the "dry- and wet-bulb scale" the number of degrees between the keepers.

When the thermometers are examined and set again, following the same plan, it will be easily seen whether the

"keepers" are, when set, *farther apart* than they were at the previous setting, or whether they are, when set, *nearer together* than at the previous setting.

If they are farther apart the thermometers are said to be "separating." If they are nearer together the thermometers are said to be "approaching." Other things being equal, the thermometers show, when they are "separating," that the air is becoming more dry—one sign of approaching fair weather. The thermometers show, when they are "approaching," that the air is becoming more moist or damp—one sign of approaching rain.

The reservoir at the bottom of the weather case ought to be kept half filled always with pure water. The wicking must be kept clean and changed occasionally—say, once in each month.

Uses of the Weather Case

The weather case is not intended to be used independently of the official weather reports. It is to be used always in connection with them. The weather case is for the purpose of supplementing the official reports by showing the local instrumental indications, and giving other information. It is intended especially for use at Farmers' Post Offices and places reached with difficulty by the printed reports. It will supplement often whatever knowledge there be of local signs, with the indications of the instruments. Its careful use taken either with the furnished reports or even without them (if they chance to fail), will often enable the character of the coming weather on the coming day to be so judged as to determine what kind of work or undertaking it is wise to plan for or to omit. The case gives the local instrumental indications, and will frequently aid in making fair forecasts for the next day.

It is well to limit the forecasting to the attempt to tell only whether it will or will not *rain* on the next day. Days on which it does not rain at all, are rated "fair," though the sky may be covered with clouds. Days on which there is rain enough to injure crops in the harvesting, are rated "foul."

It must never be forgotten that the weather case is only to aid, sometimes, in making up one's mind as to what the weather of the next day will be. While it will often be very useful, there will be many instances in which everything will be left in doubt.

Location

The weather case should be hung or stand in a fair light, where it will be always shaded, preferably on the northern side or part of the house, where it will not be exposed to artificial heat, and where there will be a free circulation of the air. It will be easier to find good locations in summer than in winter. As the readings of the instruments are examined for their general indications only, the great care as to the location will not be so needed for general use as if the readings were for exact record. For particular uses, the case may be particularly sheltered.

To Use the Weather Case at Sunset

(1) Read the description: See that the mean barometer index is set to the mean barometer reading for the place and for the month.

(2) Set the rain wind segment (or arc) and the dry wind segment (or arc) at the proper places on the wind-disc circle for the place and month.

At sunset turn the "long barometer pointer" until it covers the "short barometer pointer." Note whether on the "main barometer scale" the long pointer is then at a reading *above* or *below* (greater or less than) the mean barometer reading for the place and for the month. Move the "sunset barometer index" until it points to the same reading on the "main barometer scale" with the "long barometer pointer."

Note the direction of the wind and set the wind-disc pointer.

Set the sunset-disc.

Set the dry- and wet-bulb thermometers.

Endeavour to apply the "indications" printed on the face of the case, to determine what is to be the character of the next day. Read these "indications" carefully, and see how many of the instruments or discs *agree* in showing one or another kind of weather as to be expected.

Study the character of the clouds. The scud cloud is one of the prominent signs of coming rain. Learn to apply the local signs of weather changes, the more the better.

Examine the case frequently during the day.

It can always be found whether the "pointer" is moving toward the right or left—that is, whether the barometer is rising or falling—by turning the long pointer, so as to cover exactly the black barometer pointer; if then, on next examination, the black pointer is found to have moved toward the right, the barometer is rising. If the black pointer has moved toward the left, the barometer is falling.

At sunset of the next day set the case again; note whether the barometer has risen since the "Sunset Barometer Index" was set at the last sunset, whether the wind is in the rain or the dry direction, and for how many hours it has been in either, approximately. Study and record the character of the sunset and what it foretells. Set the wet- and dry-bulb thermometer, and note from the "keepers" whether the thermometers are approaching or separating.

Endeavour to make a forecast whether it WILL or NOT rain for the ensuing day, as well as a more general forecast.

For instance, if with a "fair weather sunset" there is a "high and rising barometer," "winds in dry direction," and "thermometers separating," the chances for a fair day for the next ensuing are increased; four indications of pleasant weather coincide. If, with a "foul weather sunset," the barometer is falling" (the pointer moving toward the left), the winds are in the rain-wind direction, and "thermometers approaching," four indications of rainy or unpleasant weather coincide. So three indications may coincide, as, with "thermometers approaching" there may be "fair weather sunset"—barometer pointer moving toward the right or high barometer—wind steady in the dry direction. The indications may be divided. In such cases weight must be given the different indications as experience may show their correctness. The purpose of the case is to be one aid only in making up a forecast.

The greater the number of the "indications," as stated on the face of the case, which are found when the instrument is set, to coincide in indicating dry or clear weather, or rainy or stormy weather, the more likely the success of the forecast.

Continue this practice at sunset from day to day. Skill will be found to increase with the knowledge and the use of the instrument and the habit of close observation of local signs which such use makes necessary.

In instances where the printed synopses and indications are had regularly, or where official weather charts, bulletins, &c., can be consulted, the weather case must be used with careful study of those papers. Locate the areas of high and low barometer on the map, and in reference to any location, as nearly as is practicable from the descriptions or data. Areas of cloudiness, rain-areas, areas of unusual temperature, &c., may be similarly located. When any such areas are found to the westward of and at all near any place, it is rare that the effects of them fail to be shown at such place by weather conditions similar to those within the areas noticed, and in a short time, as of hours or days. As the movements of the

areas, easterly, have a certain regularity, it is soon learned to tell nearly *when* the effect of any area noticed as approaching will commence to be felt. The instrumental signs of the weather case will show the changes commencing, and it may be judged whether or when the weather conditions noticed in the approaching area are likely to prevail. Some elementary knowledge of meteorology is needed; but much can be done by a careful watching, solely. The instruments of the weather case will show the changes anticipated when they begin to be felt and before they have arrived in their full force.

If the area noticed has been large or small, with weather conditions decided or variable, it can, from these facts, be judged often if the "coming weather," of whatever kind it may be, will be steady for a day or for days, or more rapidly changing. The case makes practicable other and many studies.

ALBERT J. MYER,
Brig.-Gen. (Brevet Assg'd.), Chief
Signal Officer, U.S.A.

WAR DEPARTMENT,
Office of the Chief Signal Officer,
Washington, D. C., July 21, 1878

ARE THE "ELEMENTS" ELEMENTARY?[1]

II.

IT would be a curious speculation were one to ask one's self what *is* the atomic weight of ozone? Is it 24? Is its atomic formula O1·5? or has oxygen the atomic weight of 32 and ozone of 48, and are the molecular weights 64 and 96 respectively? This can scarcely be, for the smallest amount of oxygen in two volumes of a gaseous compound of that element is certainly not 32, but 16 parts by weight. In fact *atoms* of allotropes scarcely appear to exist, the molecule appears to be the smallest amount of one of these substances that can exist either in, or out of combination. But can elements really exist in combination under various allotropic forms? We do not know. Weber thinks they can, Clarke thinks they cannot. The example of graphitic acid sometimes cited as proof of the existence in the combined form of allotropic carbon does not appear to me to *prove* either one view or the other. Graphite, may we not say, is an intermediate stage in the formation of graphitic acid from carbon? But it does not follow that the carbon in that acid is in a form different from that under which it exists, say in sugar. But it is exceedingly difficult, as yet, to attach a definite meaning to such a statement as "Carbon exists *under different forms* in this and in that compound."

The specific heats of allotropes vary. Weber has most carefully determined the specific heats of the modifications of carbon and boron. The numbers obtained at low temperatures are different, but when we come to those temperatures at which optical differences disappear, we find that differences in specific heats disappear also. At high temperatures there is but one specific heat for carbon and but one for boron; at low temperatures there are two or more specific heats for each. This seems to mean that at sufficiently high temperatures there is but one form of carbon and but one form of boron. As we do not know the atomic or molecular weights of the allotropic modifications of these elements, we can, it seems to me, draw no conclusions of any value concerning the atomic heats of these allotropes, and therefore the fact that the atomic heat of the elements is a constant number may be explained equally well on the hypothesis that the elements are all allotropes of one of themselves, or of an unknown substance, and on the hypothesis that the elements are essentially distinct forms of matter. It is well to bear in mind that, so far as our knowledge of allotropy goes—and it goes but a very little way—we have reason to believe that each allotrope has a different molecular, and therefore, probably, a different atomic weight, from every other allotrope of the same element; and, further, that we know that allotropes are at high temperatures resolved into one and the same form. Phosphorus has an abnormal vapour density: two volumes of the vapour of this body contain four (relative to hydrogen as two) atoms; red phosphorus (P_β) changes into common phosphorus (P_α) at comparatively low temperatures, therefore we do not know the vapour density of P_β. At a point not far from its boiling point, two volumes of the vapour of sulphur contain six atoms (if $S = 32$); at a higher temperature two volumes contain two atoms. We appear to have here a real gaseous allotrope. Is it analogous with ozone? Is it not therefore probable that the densities of P_α and P_β would be found to be different, supposing they were both obtained as vapours? But even P_α is abnormal. May it not be, then, that we have not as yet obtained normal phosphorus at all? That what we call phosphorus is an allotrope of the true phosphorus, viz., P_2? May it not be that at very high temperatures P_α splits up and yields true normal phosphorous vapour? Now let us briefly glance at isomers, or compounds having the same composition, but with different molecular weights. The mere fact that compounds of the same composition, but different molecular weights, exist, especially when taken in conjunction with the further fact that different compounds having the same composition and the *same* molecular weights also exist, renders the theory that the elements are really compound bodies not altogether improbable. So little has been done in the way of exact determinations of the specific heats, specific volumes, and other physical constants, of isomeric bodies, that I forbear from pressing the facts that are known into the argument, but content myself with saying that a more or less simple relation appears to exist between the physical and the chemical nature of the various isomers. The generally accepted theory of isomerism seeks to account for the facts by supposing that the atoms of isomers having equal molecular weights are differently arranged: another theory—to me it appears that the one theory is complementary of the other—supposes that the differences in the action of isomers are to be traced to differences in the amounts of "energy" possessed by these isomers (would it not be better to say, to differences in the relations between the potential and kinetic energy of isomers, and also perhaps to differences in total energy?); whichever theory is applied to isomers may be equally applied to the elements, on the assumption that these bodies are really compounds of one simple form of matter.

The positive evidence in favour of the theory of the non-elementary nature of the so-called elements is not very great. Yet, to say that the elements *are* truly elementary is, I am persuaded, a statement which is not justified by the facts which we possess. Either hypothesis may be adopted as a working hypothesis: the former, that the elements are *not* elementary, is, it seems to me, likely to lead to more discoveries, and to pave the way to more far-reaching generalisations than the latter.

But why should no one have succeeded in decomposing one of the so-called elements? In a sense we have succeeded. Ozone is an element, but it can be decomposed. Oxygen may, I think, be said to be a simpler form of matter than ozone. The introduction of the battery into chemistry led to the decomposition of potash and soda; the introduction of new engines of research may lead to the decomposition of some of those bodies, our conclusions regarding the elementary nature of which rests upon the same kind of evidence as did the conclusions regarding the elementary nature of potash and soda before the experiments of Sir Humphrey Davy. Analogy prompts us to ask, Is it not possible that what we cannot accomplish in our earthly laboratories may be actually brought about

[1] A paper read before the Owens College Chemical Society. Continued from p. 593.

in the great natural laboratories of the sun or stars? And facts recently observed by Lockyer give some countenance to those who would answer this question in the affirmative. The spectra of compounds are as a rule more complex than those of elements. The former bodies, speaking broadly, yield channelled or band spectra, the latter line spectra. The non-metallic elements, also, yield spectra which may be generally described as channelled or band spectra, while the spectra of the metals are more to be described as line spectra. Again, with increase of temperature band tend to change into line spectra; with increase of temperature compounds tend to be decomposed into their constituent elements. But the solar spectrum is a line spectrum, the spectra of certain stars—Sirius, &c.,—are line spectra, but simpler in their character than that of the sun; the hydrogen line is predominant in the Sirian spectrum. The spectra of certain other stars—*e.g.*, the red stars—are very complicated, and consist for the most part of bands and channelled spaces.

Putting these facts together, the hypothesis has suggested itself to Lockyer that the atmosphere of the red stars contains certain compounds and many non-metallic elements, that the atmosphere of the sun is characterised by the presence of metals, there the non-metals are decomposed into simpler forms, that those elementary bodies which are not found in the hotter part of the sun's atmosphere *are being* formed in the upper and cooler portions, but as they descend they are again dissociated, and lastly, that in the very hot stars our elements are for the most part resolved into simpler forms. Hydrogen, however, appears to exist there in the form in which it is known on this earth. This hypothesis is put forward tentatively by its author, and must only be accepted as a working hypothesis. It is most interesting to the chemist; in some of its bearings it also tends to throw light upon the physical conditions of the existence of stars and suns. Yet it is only an hypothesis, we must beware of accepting it as a dogma.

There is one point as bearing on Lockyer's hypothesis to which I should wish to direct attention. Non-metals show more variations in their spectra than are exhibited by the spectra of the metals. Metals yield, as a rule, only line spectra, non-metals channelled, then line spectra, as the temperature increases. Analogous with these changes in the spectra of non-metals is the well-known "plasticity" of these bodies. The instances of allotropy about which we have any accurate knowledge are instances among the non-metallic elements; probably, then, these bodies will be more readily decomposed than the metallic elements.

Putting together all that is known on the subject, the balance of probability appears to me to be in favour of the hypothesis that the elements are not really elementary. But if one is asked to put forward a positive hypothesis, not merely to favour a negative one, the task becomes much harder.

In a short, but exceedingly suggestive paper ("Speculative Ideas respecting the Constitution of Matter," *Phil. Mag.*, February, 1864), Graham tentatively put forward the hypothesis of original matter having a molecular or atomic structure, all the molecules being uniform in size and in shape, but not all possessed of the same amount of motion. In the differences in the motions of the parts of this original matter Graham sees the origin of all differences in the properties of our various elements. The gaseous molecules which we are accustomed to measure are not, says Graham, to be regarded as the ultimate molecules of the original matter, but as composed of a "group or system" of these. This hypothesis of Graham appears to me to be one of great merit. But if we start with one matter, whose molecules are of equal mass, may we not imagine these molecules originally possessed of equal amounts of motion? Having got these molecules, it is not, I think, beyond the powers of the scientific imagination to regard some of them as coming within the sphere of each others' action, and as coalescing to form new compound molecules, the mass of such new molecules being of course different from that of the original molecules. After such an encounter the new molecule will possess an amount of energy different from that possessed by the original molecules; hence it will exhibit new properties. The original matter has thus become differentiated; we have now more than a grained structure, the grains vary in mass, and in the amounts of energy which they possess. This process of evolution of higher and higher orders of molecules may proceed (may be now proceeding) until we arrive at those systems which are at present generally regarded by chemists as the molecules of distinct forms of matter, as the elementary bodies of to-day. These elementary bodies are again ready, under proper conditions, to form yet higher orders of molecules; these are our compounds, but these higher orders are less stable, under average conditions, than the lower (elementary) orders of molecules. If by any means a very large amount of energy be added to our elementary molecules they would tend to dissociate and to reform the simpler orders or groups from whence they have been derived. Such addition of energy appears to be given in the intensely hot atmosphere of the sun, where metallic bodies may seemingly remain in company with heated oxygen, yet unoxidised. If, however, a small amount of energy only be given to an element, then that element becomes ready to unite with another, or with others, to form a compound body. The bodies which we call elements would, on this hypothesis, be but intermediate stages in the evolution of complicated compounds from one original form of matter. At certain stages in this process points of comparative rest are reached; one of these points marks the existence of our so-called elements. Bodies which are elementary in our laboratories are compounds in the more energetic laboratories of the sun and hot stars. Many of our compounds, again, are elementary in the cold, listless atmosphere of the moon. Just as it is very difficult, if not impossible, to define a chemical compound, to say where the mere mechanical mixture or aggregation ends and the true chemical compound begins, so, in this view, would it be impossible to define a chemical element. Whether a substance is compound or elementary depends upon the point of time at which the investigation is made and upon the conditions of the environment. Graham has pointed out that the "colloidal state" seems to intervene between the liquid and crystalline states; the experiments of Faraday, of Caignard de la Tour, of Andrews, and more recently of Pictet and Cailletet, have taught us that between the gaseous and liquid states there is no hard line of demarcation; many facts in chemistry and in chemical physics appear to be explicable only on the supposition that the passage from mechanical mixture to chemical union is a gradual and continuous, not an abrupt and discontinuous one. Why, then, should not the passage from the one original element to the one final compound be also a gradual passage?

And as in animate nature we know that the (comparative) permanence of a species is in no way contradictory to the general law of gradual development, so in the history of molecular arrangements it may be that the present permanence of our so-called elements only marks a resting point in the slow but sure process of formation of more and more complicated compounds. The average conditions of our present surroundings may not allow of the existence of any less complicated molecular aggregations than those which we call elementary, just as they do not appear to allow of the existence of any extremely complicated aggregations of chemically united molecules.

There is another aspect of the question upon which one

word may be said. I think that I am not in error in saying that the minds of most persons are imbued, more or less deeply, with the idea that nature is ultimately very simple; that could we but grasp the great laws of nature we should find them extremely simple, although the results of their actions are so wonderfully complex. This belief appears to be deep-rooted in most minds, yet if we are to study nature aright we must, I am persuaded, set it aside. We must be content to take nature as she is, *i.e.*, we must do our best to amass facts, and from these we must draw the conclusions warranted by the facts. Now as our knowledge of nature extends do we find that she becomes to us more and more simple? Yes, and No. It has been now and again given to a gifted few to pierce through the maze of tangled facts and to espy the great principle which binds them into an harmonious whole. But even in the case of these great generalisations, exact experiment and observation frequently show that little details have been overlooked—that the great simple law is too simple—that there are discrepancies, *very small*, it is true, but still there they are, demanding an explanation, telling us that our law does not express the whole of Nature's facts. Nature appears to be truly infinite; and it is well to remember that, *we can never get sensibly nearer a knowledge of an infinity*.

This idea of the simplicity of nature is very apt to lead us to adopt the hypothesis of the non-elementary nature of the elements without sufficient evidence. The idea that all the elements are really compounds of one primary form of matter is a most fascinating idea, it *seems* to be so much in keeping with the simplicity of nature; it is so symmetrical, it surely *must* be true. This is just how the old alchemists reasoned; we must absolutely forbid these *à priori* conclusions to influence us as students of nature. The hypothesis of the compound nature of the elements, of the existence of but one, or even of a few primary forms of nature, fits in with the nebular hypothesis of the formation of the worlds, but have we facts to support it? If one can only come back to facts we need not fear to start what may appear to be wild and romantic theories.

The outcome of the whole matter is this:—we want more knowledge, our facts are few and vague; there is room for almost unlimited work. Ask Nature; trust her: be sceptical of your own interpretations of her answers. M. M. PATTISON MUIR

THE LATE SIR RICHARD GRIFFITH, BART.

WE have just laid to rest all that is mortal of the "Father of Irish Geology" in Mount Jerome Cemetery, at the ripe age of ninety-four years. Few public men in Ireland have done so much for the material advancement of their country. If "the age makes the man" the late Sir Richard Griffith was the man whom the age called forth to indicate the road to material improvement at a time when roads, railroads, drainage works, and similar agents were urgently required in this country. Griffith's geological knowledge was the basis of his power; and while few understood, or cared to understand, the principles by which his judgment was guided, Government and the public were always ready to put faith in their application. Amongst the useful works carried out under his direction were the roads which he constructed or improved in the counties of Cork, Kerry, and Limerick, during the time when the Marquis Wellesley was Lord-Lieutenant. Some of these roads are striking examples of engineering skill. I have recently travelled on one of them, namely, that which crosses the wild and rocky range between Kenmare and Glengarriff. Before this road was made the country was inaccessible and the haunt of Whiteboys; now no district in the British Isles is safer for the traveller, and, I may add, more full of bold and beautiful scenery. With reference to Griffith's services to the cause of Irish geology, it is unnecessary for me to say a word here, except in so far as regards the public department with which I have the honour to be connected. It is to this subject that I wish especially to direct the attention of the readers of NATURE, as I am anxious to pay a tribute to the remarkable acumen which Griffith exhibited in determining the age of the various formations which are to be found in this country, as exemplified in at least one special instance.

It is well known that there is one point in the geological structure of the south-west of Ireland on which there has been a difference of opinion between the Government geological surveyors and Sir R. Griffith; I refer to the age of that great group of rocks which occupies the mountainous districts of the Dingle promontory, and those of Killarney, the Reeks, and Glengarriff. These were called by the late Prof. Jukes "The Dingle Beds," and they consist of a series of purple slates traversed by cleavage planes and massive green grits, and thrown into numerous grand flexures. They are of great but unknown thickness, as in the Dingle promontory they are overlaid unconformably by the beds of the Old Red Sandstone. In the Dingle promontory these beds are seen in contact with fossiliferous beds of recognised Upper Silurian age, and the whole series had been referred by Griffith to the "Silurian" formation, as may be seen by reference to his geological map of Ireland (edit. 1855). In a similar manner the mountainous regions abovenamed, and lying to the south of Dingle Bay, were mapped and coloured as "Silurian," and were separated off from the Old Red Sandstone throughout the counties of Cork, Kerry, and Waterford.

The views thus held and published by Griffith with regard to the geological age of the rocks forming the south-western highlands were not upheld by the officers of the Geological Survey, who, we may be sure, spared no pains to come to some clear decision on the question. On the maps of the Survey the mountains of Kerry and Cork are coloured "Old Red Sandstone," and "the Dingle Beds," with a distinct colouring, are placed in a position intermediate between the Old Red Sandstone and that of the Upper Silurian. Prof. Jukes, in the "Explanations" to accompany the maps of the Survey, has fully entered into the reasons which induced him and his able colleagues to arrive at this decision. Certain apparent obscurities in the sections of the Dingle district and those of the neighbouring regions prevented them accepting Griffiths' views, and the whole matter was left an open question, subject to further investigation.

Under these circumstances—the time being favourable—I received the sanction of the Director-General to make a preliminary examination of the sections at Dingle and in the districts of Killarney, Kenmare, and Glengariff—with a view (if possible) of coming to some decision on a question which has been confessedly left in an unsatisfactory position. In this tour I was accompanied by Mr. J. O'Kelly and Mr. A. McHenry, officers of the Survey—and we have returned from it fully satisfied in our own minds of the correctness of Sir R. Griffith's views regarding the age of the beds of the Dingle, Killarney, and Glengarriff Ranges. To our minds the evidence is clear and satisfactory that these beds are really of Upper Silurian age, as maintained by Griffith. Into this evidence I cannot enter here, but hope to do so at some length in another place. It was with great gratification that some days since I addressed a letter from Eccles' Hotel, Glengarriff, to Sir R. Griffith, announcing the result of our investigations. I little knew that at that moment the spirit of our venerable friend had passed away! Few men were less dogmatic in maintaining their conclusions than Griffith. If others differed from him he remitted the matter to the arbitrament of time, satisfied that if he was in the right time would show it. In *this* case it is only justice to his

memory to bear testimony to the soundness of his judgment. EDWARD HULL

Geological Survey Office, Dublin, September 27

ROBERT HARKNESS, F.R.S.

ANOTHER of the captains in the phalanx of British geologists has dropped from the ranks. Robert Harkness died suddenly in Dublin on Saturday last. He had been ailing for some time, and the disease from which he suffered—an affection of the heart—had gained ground so much this year that he lately felt himself compelled to resign the chair of geology at Cork. It was the expectation of his friends that, released from duties which he had so long conscientiously performed, he might yet enjoy some years of comparative health in the quiet retirement of his Cumberland home, to which he used to return with such pleasure every summer. But this was not to be. He has fallen just as he had himself brought the public labours of his life to a close.

It is now some five-and-thirty years since the name of this able geologist first appeared as a writer on his favourite science. During this long period he had explored, on foot, the geology of large districts in the north of England, in Scotland, and in various parts of Ireland. The reports of the British Association and the *Quarterly Journal* of the Geological Society bear witness to his industry and to the painstaking minuteness of his method of investigation. To him we owe our earliest exact information regarding the correlatives of the reptiliferous sandstones of Dumfriesshire and Cumberland. It was his patient labours continued year after year over ground most difficult to unravel, that led the way to the working out of the structure of the silurian uplands of the south of Scotland. To his research, too, is due the identification of the metamorphic rocks of the north-west of Ireland with those of the west of Scotland. To the elucidation of every one of the palæozoic systems of deposits he contributed something of value.

But important as was his scientific work, it had not a wider and more hearty recognition among his brother geologists than his own admirable qualities of head and heart. Who that has been privileged with his friendship will not cherish the memory of his earnestness over even the driest of details, his quiet enthusiasm, his generous admiration for the work of others, his unfailing cheerfulness? Who will forget that beaming ruddy face, never absent from the platform of Section C at the British Association meetings, always ready to rise among the speakers there and to reappear at the festive gatherings in the evening? There have been men who have graven their names more deeply on the registers of scientific thought and progress, but there have been few whose sunny nature has more endeared them in the recollection of their friends than Robert Harkness. A. G.

MANGANESE NODULES IN LOCH FYNE

ON September 21, this year, I anchored the steam yacht *Mallard* near the mouth of Loch Fyne, in 104 fathoms, for the purpose of making physical and chemical observations on the water of this, the deepest part of the Firth of Clyde. When the anchor was got up a large mass of clay and shells was found sticking to one of the flukes. It was gently dried, and on examining it I observed a number of nodular concretions, which, on being freed from the surrounding clay, presented a finely mammillated black surface, were easily cut with a knife, giving a brownish-black powder, which liberated chlorine from strong hydrochloric acid, and possessed all the properties of peroxide of manganese; in short, they were identical with the manganese nodules which we found in the *Challenger* to form so important a constituent of the sea-bottom in the greatest depths.

One half of the dried mud was carefully broken up and searched through, the nodules being collected by themselves and also the shells. It was thus separated into three portions, which were weighed, with the following results:—

Manganese nodules ...	142·7 grammes. ...	30 per cent.
Shells	35·0 ,, ...	7·5 ,,
Sandy clay	289·0 ,, ...	62·5 ,,
Total	466·7	100·0

The manganese nodules, therefore, made up thirty per cent. of the weight of the mud. Compared with those frequently met with on board the *Challenger*, the nodules were small. In the sample examined there were eighty-three nodules weighing 142·7 grammes, hence the average weight was 1·7 grammes. Their volume was found to be 58 c.c., so that the average volume was 0·7 c.c., and the specific gravity 2·46. Their form was roughly spherical, the largest, which was somewhat elongated, measured $13 \times 9 \times 6$ millimetres, the average diameter of them all being 11·4 millimetres.

Of the eighty-three nodules so obtained I have split twenty-two. When subjected to this treatment they are found to differ in constitution from the majority of those obtained on board the *Challenger*. Although they had not been exposed to any heat they were hard and sandy to the knife, and when treated with strong hydrochloric acid, they left a large amount of mineral (chiefly quartz) sand. This difference, however, is explained by the different kind of bottom from which they were obtained. In dissolving up nodules which had come from "red clay" in 2,500 or 3,000 fathoms I always found the same mineral sand left as on treating the clay in the same way. But the amount of sand was always quite insignificant, as compared with the clay; hence the nodules were easily cut with the knife. They, however, got harder on keeping. In Loch Fyne the bulk of the mud consists of quartz sand, giving the nodules the appearance of sandstone, whose binding material is made up to a great extent of peroxide of manganese, and hence the gritty feeling to the edge of the knife.

Where a hard nucleus has been found it has always been a piece of rock from the neighbouring shore, but in most instances (in sixteen out of twenty-two examined) the ordinary arrangement has been reversed, the nodule consisting of a soft rich nucleus of peroxide of manganese, surrounded by a black sandy rind, the whole enveloped in the characteristically mammillated black skin.

I hope very shortly to be able to report more fully on them; in the meantime, I have only been able to verify their nature by finding abundance of a higher oxide of manganese, easily recognisable quantities of cobalt, and the presence of water, which, on being expelled by heat, has an alkaline reaction and an empyreumatic odour, properties in which they agree with those which I had occasion to test on board the *Challenger*.

Their position in the mud, with dead shells above, below, and on all sides of them, will, when carefully studied, no doubt throw much light on their age and method of formation. I have observed two nodules firmly attached to the interior of shells, one having evidently been directed in its growth by the shape of the shell.

In endeavouring to procure a further supply I dropped anchor in about the same depth, but about a hundred yards further down the loch, and I obtained about the same amount of mud, but it contained very much more shell and no nodules. Also in Kilbrennan Sound, between Arran and Cantyre, in a depth of eighty-five fathoms, there was much shell and pebble, but no nodules. So far, therefore, this occurrence appears to be very local.

J. Y. BUCHANAN

STATE AID TO SCIENCE

UNDER this title the last number of the *Lancet* has the following :—

It was for a long time the fashion with zealous workers in the field of science to protest that the cause of discovery would not be advantaged by State patronage or State aid. The more thoughtful inclined to the belief that the patronage would be more mischievous than the pecuniary assistance was helpful. For some years past this persuasion has been losing ground, and, whether scientists are becoming more worldly or less exclusive in their views, it is abundantly evident that the reverse of a feeling of unwillingness to accept aid from the State prevails. It will doubtless be contended that the way in which help has been placed at the disposal of explorers and investigators so completely removes all difficulties, that it would be not less ungracious than impolitic to refuse the proffered assistance. The labourer chooses his own form of enterprise, or applies for aid in the course of an inquiry instituted at his own wish, that he has simply to satisfy a committee of fellow-workers as to the object of his pursuit or the nature of his researches, and, upon their recommendation, the necessary funds are forthcoming, without the least semblance of dictation or interference. The explanation is obviously satisfactory, but the fact remains, the objections which many of the older and more successful discoverers urged against seeking or accepting the assistance of the State in their investigations have been discarded, and the only grievance felt by contemporary inquirers relates to what they conceive to be the paucity of the grant and its wrong distribution, for which last fault, if fault there be, the governing bodies of the principal scientific societies are mainly responsible. We offer no present opinion as to the comparative merits of the old and new view of the State aid question, and we do not propose to discuss the complaints arising out of the system of administration extant; it is for the more pressing issue, whether a permanent provision should not be made for the support of men who live by investigation rather than teaching, we now ask a few minutes' attention.

It must be conceded by all who have any acquaintance with the subject, that not only the pioneers, but, in a practical sense, the advanced workers in science must necessarily be debarred from the ordinary rewards of their profession. If it be difficult to practise and preach, it is incomparably more embarrassing to study and teach. In short, the explorers and investigators in any department of work cannot live by communicating the knowledge they accumulate. The business of utilising the store of information amassed by the labourers in the advanced field must be performed by men who are not themselves engrossed with research. The enterprise of discovery cannot be delayed while the explorers strive to popularise their acquisitions, nor are the faculties which prove most useful in the field of inquiry especially well adapted for successfully imparting the knowledge obtained. The functions of the scholar and the schoolmaster, the collector and distributor, are essentially different; we might go further and affirm that they are scarcely compatible. It follows, therefore, that the two classes of workers must always exist. Some must make knowledge and live by that form of labour, while others distribute or apply it to general uses. It seems to follow, without the need of argument, that by some expedient the means of self-support must be placed within reach of those who are not in a position to render their produce marketable. This necessity has been long recognised, and with a view to meet the case, College fellowships and snug sinecures in the Church and at Law have been preserved for the shelter and support of those who required scholarly leisure for the pursuit of inquiries. The spirit of the age is, however, eminently utilitarian and strongly opposed to sinecures; and, as a matter of fact, the system was manifestly open to abuses. The medical profession has never largely enjoyed these advantages, although the discoveries made by its members are equal, if they do not surpass, those of any other branch of labour in science. The time has come when the whole question needs to be discussed from a new standpoint, and in a more practical fashion than hitherto. We venture to suggest that it should not be left in the hands of interested persons who cannot speak freely on the topic, but considered by the profession as a body. The points to be adjudicated are: first, has medicine its full and fair share of State aid? and secondly, has not the time arrived when some formal provision should be made for the support of men who devote their lives to inquiry and cannot reasonably or expediently be expected to practise or teach? In the old days much of the hardest work in science was done by Churchmen of the various orders who were supported by the ecclesiastical institutions of the country; now the labour of research is performed by laymen, and they must live.

THE TOURNAMENT OF INCUBATORS

IN a recent number we referred to the hydro-incubator invented by Mr. Christy. This incubator, along with a number of others, has been subjected to a comparative trial at the Hemel Hempstead Waterworks. An account of this competition appears in the *Live Stock Journal*. It commenced 6 A.M. September 5, and concluded on September 26, at 12 o'clock noon. The object of the committee who tested the incubation was—

(1) To ascertain whether incubators were of any practical value to the public generally; and

(2) If proved to be of value, to decide which was the best incubator for the ordinary purchaser to select.

That the person to whose management the incubators were entrusted should be unskilled and inexperienced in their use, was one of the conditions specially insisted on by the exhibitors. This was considered the best means of proving which incubator was of the simplest construction and could be worked most successfully without any previous apprenticeship. A large room in the Waterworks building was secured, where steam, hot water, or gas, could be employed, and where perfect privacy and quiet could be secured. Exhibitors were requested to send their incubators with full and clear instructions as to their method of working.

The eggs, which should be laid on Wednesday, September 4, had been bespoken some time before at several farmhouses in the neighbourhood, and these having been collected during the afternoon of that day, were brought to the Waterworks, and thoroughly intermixed by the members of the committee. Each egg was then marked with the word "*Couveuse*," by a stamp made at Brighton expressly for the occasion. By six o'clock on the morning of Thursday, September 5, the eggs had all been placed in the incubators by the members of the committee, the machines having been fully prepared for their reception. The incubators were then intrusted to the engineer, Mr. Twigg, with strict injunctions to follow implicitly the instructions of the exhibitors, and to admit no one without a written order from the committee to the room, which was to be kept locked, especially the exhibitors themselves or their agents. The following are the statistics of the competition itself, which were attached to the incubators as soon as possible after mid-day on September 26:—

No. 1.—VOITELLIER: Hydro-Incubator.

Of fifty eggs placed in incubator none were hatched.

This machine, from its simplicity and the ease with which the thermometer could be consulted, was quite a favourite with the engineer, who was most sanguine as to

its results, and much disappointed at its failure. On subsequent examination no chickens were found in the eggs.

No. 2.—CHRISTY: Hydro-Incubator.

Eggs placed in incubator		50
Found fertile after testing	45	
Unfertile	5	
Broken during competition	0	
Hatched by 12 noon, September 26 ...	34	
Not hatched 11	11	
Percentage of eggs hatched		75·55

Three more chickens were hatched alive after the competition had closed. The other eggs, on being examined, were all found to have living chickens in them. The prize of 25*l*. was awarded to this incubator.

No. 3.—CHRISTY: Hydro-Incubator.

Eggs placed in incubator		50
Found fertile	45	
Unfertile	5	
Broken during competition	1	
Hatched by 12 noon, September 26 ...	20	
Not hatched	24	
Percentage of eggs hatched		44·44

Two chickens were hatched alive after the competition was ended. Of the remaining eggs ten were found to have living chickens in them.

No. 4.—BOYLE: Heated by lamp.

Eggs placed in incubator		48
Found fertile	40	
Unfertile	3	
Broken during competition	2	
Hatched by noon, September 26 ...	11	
Not hatched	27	
Percentage of eggs hatched		27·55

On examining the eggs, Saturday, September 28, four chickens were found ready to break the shell. This incubator worked with great regularity, and deserves much commendation.

No. 5.—BOYLE: Heated by gas.

Eggs placed in incubator		52
Found fertile after testing	42	
Unfertile	10	
Broken during competition	26	
Hatched by 12 o'clock noon, Sept. 26...	0	
Not hatched	16	
Percentage of eggs hatched		0

The egg rests in this machine are spiral wire springs The egg drawer did not fit well, and is certainly capable of improvement. Full and very clear instructions should always accompany this incubator. Its want of success must not be attributed altogether to the inexperience of the attendant.

No. 6.—PENMAN'S (worked by lamp): Exhibited by Messrs. E. T. Brown and Son, Newcastle-on-Tyne.

By this no eggs were hatched, but twenty dead chickens were found in the eggs on September 28, having been dead apparently several days. The lamp in this incubator worked very irregularly, needing constant attention by day and night.

No. 7.—PENMAN'S (worked by gas).

By this also no eggs were hatched, but on examining the eggs at 6 P.M. on Saturday evening, Sept. 28, thirty-four chickens were found to be alive in them, two having been hatched out on the same morning alive. The source of heat—viz., gas—had been turned off at 8 P.M. on the Friday night previous, and the drawer had been opened and shut constantly after the exhibition was opened at noon on Thursday. The irregularity of heat from the gas was doubtless the cause of failure in this instance, the pressure being very unequal.

The committee subjoin to this report a register of the temperature maintained in the drawer of each incubator, together with that of the water drawn off from the boilers in the case of the hydro-incubators. The chickens hatched are doing well, some under them, some in artificial mothers.

Such are the facts of this interesting trial, and they seem to us to prove not only that artificial incubation is possible, but that by Mr. Christy's machine, if not with some of the others, it might become a remunerative business, and add materially to the sources of our food supplies.

NOTES

BY the kindness of Gen. Myer, the distinguished head of the U.S. Army Signal Service, we are enabled this week to give the official description of the weather case, the distribution of which among the 27,000 rural post-offices in the United States has just commenced. It is for use in those parts of the country where the daily weather indications cannot reach in time to facilitate agricultural operations, and its issue has been forced upon the Government because the American farmers are wise enough to see that for them, as well as for sailors, to be forewarned is to be forearmed. In a few centuries we may expect to have something of the same kind here.

M. BOUILLAUD, the once celebrated medical practitioner, who is a member of the Paris Academy of Sciences, assailed M. du Moncel in the sitting of September 30, and asserted that the phonograph and microphone experiments must be the work of ventriloquists. This fit of incredulity was occasioned by the recital of experiments made with the singing conductors. M. du Moncel asked for a commission of investigation to be appointed, although such accusations are not deserving of any notice, and have, indeed, raised universal ridicule. But the regulations of the Academy forbid any commission to be appointed to pronounce on the works or communications of members. Another curious scene took place at the sitting of last Monday. M. du Moncel presented to his colleagues, the "condensateur chantant," which had been exhibited on the previous Saturday. He retired to the room of the Académie Française, in company with M. Faye, closed the door and sang. His voice was heard coming from a number of sheets of paper, in which six sheets of tinfoil had been inserted, and connected with the wires of an induction coil. M. Bouillaud was obliged to retreat from the position he had taken at the sitting of September 30. He made no allusion to the accusation of ventriloquism, but read a long quotation from Descartes, to show that "even if a speaking machine had been constructed, it could by no means be considered as a thinking machine." He said that speaking was not only a mechanical action, but also an intellectual work, so that neither the phonograph nor the singing condenser could be regarded by any means as really speaking! The whole assembly, in spite of its usual gravity, burst into roars of laughter. M. Milne-Edwards, who spoke at the previous sititng, said with much propriety, he should not have answered M. Bouillaud if he had understood such was his issue. Unfortunately he had understood, as everybody in the assembly did, that M. Bouillaud questioned the honesty of the experimenter. At the end of the sitting M. du Moncel performed all the principal experiments of the phonograph.

SOME remarkable experiments in Electric Telephony were shown by Prof. Barrett in a lecture at the Midland Institute a

few days ago. By means of Edison's carbon telephone, which promises to be the telephone of the future, the lecture was electrically transmitted to the Arts Club in an adjoining street, though the transmitting instrument was several feet from the speaker; conversely, an assistant, speaking near to the distant carbon transmitter, was heard in a dozen different receiving magneto-telephones distributed through the lecture Hall. Further, by employing a single "Phelps" telephone as receiver, and using a paper cone as the mouth-piece of the telephone, 300 or 400 people in the neighbourhood of the instrument were able to hear distinctly sentences and songs given to the distant carbon telephone. And finally, by the same means, the entire audience of some 1,500 people heard single words, such as Bravo! Halloa! &c., spoken to the far telephone. Other experiments, of a more crucial character, demonstrated that this new telephone of Edison's will probably place electric-telephony on an entirely new and more practical basis. Prof. Barrett also showed Mr. Edison's tasimeter, an adaptation of the principle of the carbon telephone, the instrument having been kindly sent over by Mr. Edison for this lecture. The extraordinary delicacy of the tasimeter to heat radiation was shown to the audience by the heat radiated from the face throwing the beam of light reflected from a galvanometer completely off a ten-foot scale. The megaphone and several other of Mr. Edison's recent inventions were also successfully shown in this lecture.

NEWS of Prof. Nordenskjöld's North-East Passage Expedition has reached Stockholm. It left the north coast of Norway on July 25, reached Jugor Straits on the 30th; steamed on August 1, and arrived at the mouth of the Yenissei on the 6th. It was intended to start afresh on August 10. The Kara Sea was nearly free of ice. A little scattered drift ice near White Island was the only ice met with during the whole voyage. The expedition has thus a good prospect of success.

THE *Times* correspondent writes from Naples, September 29, that for the past two or three days, according to Prof. Palmieri's report, the activity of the mountain had much diminished, and the seismic instruments had been quieter. At the time of the new moon there was an increase of activity, as is always the case, and it will be witnessed, no doubt, at the time of full moon; but the mountain pursues its regular course, except at these seasons, and some little time must elapse before what the world calls an eruption will occur. Of what character it will be it is impossible to say precisely, but appearances indicate that it will be a lava eruption—presenting, indeed, a most brilliant spectacle, but unaccompanied by those horrors which marked the eruptions of 1854, 1861, and 1872; but, as Prof. Palmieri observes, it is impossible to say how it will terminate. A *Daily News* correspondent sends an interesting account of a visit he has paid to the crater of Vesuvius, into which he descended, and tells what he saw:—The actual crater is placed almost in an amphitheatre, three-fourths of which are inclosed, while one-fourth is open. The inclosing walls rise above the bed of the crater from 250 or more feet in some parts, apparently composed of sulphur. The diameter, judging by the eye, from one side to the other, is about 300 yards, and the whole of this area is filled with lava on fire, but crusted on the surface with a skin some inches deep of lava that has been chilled. "Looking between the cracks or down the 'crevasses,' the glowing fires a few inches below our feet, and in the blocks whereon we were standing, were seen. The ten months' activity has enabled the volcano to raise a cone almost in the centre of the crater at least a hundred feet in height, very wide at the base, converging to the summit like a sugar-loaf, but with the summit of the loaf removed. With a pulsation as regular and as marked as that of the piston of a steam-engine in full motion, did the huge mountain carry on its work, so that now we were able clearly to understand what was meant by 'every pulsation of the volcano being duly registered at the observatory.' Clouds of smoke and fumes were issuing from the summit of the cone—now densely dark, as if a fresh supply of coal had been heaped on the fire; then intensely light, as if the engine were blowing off its steam; then most beautifully and delicately tinted with the tenderest rose-pink, as if an artist were testing how best to combine the loveliest tints of his art; then a pale salmon, a little while, and then as if five thousand torpedoes were simultaneously exploded. The huge mountain seemed to heave, and forth from its mouth issued immense quantities of molten lava, shot scores of feet high up into the air—apparently at the mouth all in one body, but there separating into millions of pieces, great and small, all glowing with the most intense red heat that can possibly be seen. Each piece as it ascended into the air was separate; no piece was partly red and partly black, but was on fire and at red-heat throughout; mostly the lava emitted fell back again into the bosom of the heaving mass, but with every emission quantities, large or small, fell on the outside of the mouth, and thus we saw readily how the cone had gradually but continuously increased in size and height. Every now and then a huge mass would drop outside, and then would be heard an immense crash, followed by vast quantities of lava rolling down the sides of the cone. As we stood watching, at intervals there seemed to be the firing of 10,000 guns of mightier calibre than Krupp's, and we soon found that this was the precursor of a grand display. Up rose, possibly 100 feet above the cone, an immense mass spreading in the shape of a lady's fan, and presenting one of the most magnificent sights the eye of man can ever see. And this upheaval was not a thing for which we had to wait till our patience was exhausted, and to wonder if it would be repeated or not, but was continuous and incessant, and almost seemed as if every renewed expulsion were grander than its precursor, or as indicating a trial of actual strength prior to the great event proposed to be completed."

THE foundation-stone of the proposed railway bridge across the Firth of Forth was laid last week.

LAST March a microscopical society was formed in Highbury under the presidency of Dr. Alabone. The numbers have steadily increased, and the society appears to have all the elements for a career of great prosperity. The opening *soirée* for the winter session will be held at Harecourt Hall, St. Paul's Road, commencing at 7.30 p.m. to-night.

MANY valuable papers were read at the meeting of the Sanitary Institute at Stafford last week, and if Government and the public are ignorant of the laws of public health and the best methods of carrying these into practice, it is from no want of enlightenment. The great want at present seems to be organisation and an efficient central authority, and we trust the practical and vigorous address of Sir Henry Cole on Sanitary Co-operation will meet with attention in the proper quarters, and lead to more systematic and efficient action than has hitherto existed. In an able paper Dr. Richardson advocated the appointment of a Minister of Health.

MESSRS. HARDWICKE AND BOGUE announce the following works for publication:—"A Manual of the Infusoria, comprising a Descriptive Account of all known Flagellate, Ciliate, and Pentaculiferous Protozoa," by W. Saville Kent, F.L.S.; "The Herefordshire Pomona," containing Coloured Figures and Descriptions of the most esteemed kinds of Apples and Pears, edited by Robert Hogg, LL.D., F.L.S., Part I., illustrated with coloured figures and woodcuts; "Clavis Synoptica Hymenomycetum Europæorum, conjunctis studiis scripserunt M. C. Cooke, A.L.S., et L. Quelet, M.D., O.A., Inst. et Sorb. laur.;

"The Sphagnaceæ, or Peat Mosses of Europe and North America," by R. Braithwaite, M.D., F.L.S., &c., illustrated with 29 plates; "Pollen," by M. P. Edgeworth, F.L.S. F.R.S., second edition, revised and corrected, illustrated with 438 figures; "The Ferns of North America," by Prof. D. C. Eaton, of Yale College, illustrated with numerous coloured plates by James H. Emerton, to be completed in 20 parts, published at intervals of about two months; "Flowers, their Origin, Shapes, Perfumes, and Colours," by J. E. Taylor, F.L.S., F.G.S., second edition; "Health Primers," edited by J. Langdon Down, M.D., F.R.C.P., Henry Power, M.B., F.R.C.S.; J. Mortimer-Granville, M.D., F.G.S., F.S.S.; and John Tweedy, F.R.C.S. Under this title will be issued a series of shilling primers on subjects connected with the preservation of health, written and edited by eminent medical authorities. The following volumes will be issued in October:—"Premature Death, its Promotion and Prevention;" "Alcohol, its Use and Abuse;" "Personal Appearances in Health and Disease (illustrated);" "Exercise and Training" (illustrated); "The House and its Surroundings;" "The Skin and its Troubles" (illustrated); "Baths and Bathing." Others will follow at short intervals.

The national *fête* for the distribution of the Paris Exhibition awards will take place, as stated, on October 22. The whole of the Versailles Park will be lighted by electricity.

In connection with the meeting of the Library Association last week at Oxford, we would recommend to our readers' attention a most interesting and really amusing little *brochure* by Mr. H. B. Wheatley, entitled "What is an Index? A few Notes on Indexes and Indexers." Mr. Wheatley gives many amusing instances of how not to do it, and his pamphlet will be found useful not only to indexers, but to all who in any way have to do with the arrangement of written or printed matter. It is published by Sotheran and Co. The Library Association has already assumed vigorous proportions, and in spite of its much talk seems likely to do real service to existing libraries and to the promotion of new ones.

The great work of connecting the triangulation of Algeria with the geodetic net-work of Europe, through Spain, is progressing favourably. The Spanish staff officers under General Ibanez have established their post on Sierra Nevada and Mount Tetica, and the French near Nemours, and Ben Sabra, near Oran. M. Perrier, member of the Bureau des Longitudes, and director of the French Survey, will very shortly proceed to Algiers to take the last readings from the French side.

Of all the accidents to which submerged submarine cables are liable, one would suppose that that by fire would be the very last that would occur. Nevertheless, such an accident has happened to the Forth cable belonging to the Post Office. Lately all four wires were found earthy. The fault showed itself by test to be close to the shore. It was found below high-water mark at the foot of the cliff. Some boys during low water had been making a fire with the shavings and rubbish found on the beach, immediately over the cable, melting the compound and gutta percha of the core, and leaving the copper wires bare and in contact with the outside sheathing.

The municipality of Prague, advised by the Hygienic Council of that city, have just issued an edict prohibiting ladies from wearing dresses with long trains in the public streets, on account of the dust which the appendices raise being detrimental to public health. The municipality of Leipzig published a similar edict some time ago. These measures are easily explained by the habit assumed by many representatives of the fair sex of letting their trains drag through dust (and worse) for the sake of producing an effect which we presume milliners consider important from the point of view of sexual selection.

The Meteorological Central Office of Vienna reports upon an aurora borealis of immense extent on September 25. It appears that the phenomenon was visible for several nights in the whole of Scandinavia and Northern Russia. It covered the larger portion of the northern sky, and appeared in a yellowish red light, with frequent undulations of bright and intensely yellow rays.

We are glad to see that Prof. Geikie has added to the usefulness of his "Elementary Lessons in Physical Geography" by preparing a series of questions. These have recently been issued by Macmillan and Co.

The Paris *Temps* publishes daily the charts of the Central Bureau of French Meteorology. The experiment was tried by the *Opinion Nationale* two years ago, but was discontinued.

Prof. Lebour has prepared a convenient and complete catalogue of the Hutton collection of fossil plants, which are specially valuable as illustrating the carboniferous flora of some of the horizon in the Newcastle coal-field. The system followed is, with few exceptions, that of Schimper. The catalogue has been drawn up by order of the Council of the North of England Institute of Mining and Mechanical Engineers.

The *Cologne Gazette* announces that on Saturday night two slight shocks of earthquakes were remarked at Buir and in the surrounding district. The first occurred about 10 o'clock, and the second about an hour later. There was a slight shock of earthquake at Parma during the night of the 2nd inst.

The Annual Exhibition of the Photographic Society was opened yesterday.

There will be a meeting of anthropologists combined with an anthropological exhibition at Moscow during the summer of 1879.

The appearance of phylloxera in some vineyards near Bonn, on the Rhine, has been officially announced.

"Tables for Use in the Verification of Standards of Weight and Measure," by Dr. O. J. Broch, Standards Commission, Christiania, is the title of a valuable paper recently translated under the directions of the Standards Department, Board of Trade. It embraces tables of specific gravity, coefficients of expansion, elastic force of aqueous vapour, and the weight of water.

Mr. L. S. Benson, New York, of π notoriety, has submitted to English mathematicians his *demonstration* (this time in ink) of a *discrepancy* between the analytical and geometrical proofs of a property of the parabola, viz., that the area of any segment is exactly two-thirds of the rectangle on abscissa and ordinate.

The Dutch Government, encouraged by the excellent results obtained, in a commercial point of view, through the construction of the Y-Muiden Canal, which connects Amsterdam directly with the German Ocean, has now the intention to construct a similar canal to connect Amsterdam with Gorinchem, and to render the Waal and the Rhine navigable for sea-going vessels, so that even larger vessels could in future sail as far as Arnheim, and s'Hertogenbosch. Of course Rotterdam, Dortrecht, Moordijk, and Flushing would also benefit by the completion of the intended new works.

The New York *Daily Graphic* furnishes some particulars of interest respecting the Nez Percés, a tribe of Indians, the greater portion of which was captured by the United States' troops about a year ago and confined in an encampment near Fort Leavenworth, on the Missouri. Their chief, it appears, carries in his hand a looking-glass which "is used to direct military manœuvres in battle by means of reflected rays of light. Their various significations, however, have never yet been found out by the white man . . . . The orders are apparently

conveyed to distant parts of the field by a system somewhat similar to the dashes and dots of the Morse telegraphic code." The Nez Percés are described as a particularly fine race and well behaved. Their women are very industrious, and when not engaged in carrying wood and water, &c., "are generally hard at work in the manufacture of beaded mocassins, gauntlets, and Indian doll babies . . . The little boys, too, reap quite a harvest by displaying their skill with the bow and arrow." One of the most curious institutions of the tribe is a primitive description of Turkish bath. "The Missouri River runs close by their encampment, and on the bank of the river they have built what has the appearance of a gigantic ant-hill; in shape it is similar to a small Esquimaux hut, about six feet in diameter and two or three feet high. There is an aperture on one side just large enough for a man to get through, and in the interior there is just sufficient room for him to lie in a cramped position. They first build a fire outside in which they heat limestones until almost red-hot. They then shovel them into the hut and pour water on them so as to produce a dense vapour, after which they quickly rake the stones on, crawl in through the entrance hole which they cover up with a blanket, and lie there until the perspiration streams from every pore, when they come outside and plunge into the Missouri River and swim to the nearest sand-bank."

THE additions to the Zoological Society's Gardens during the past week include a Mona Monkey (*Cercopithecus mona*) from West Africa, presented by the Rev. W. N. Ripley; a Macaque Monkey (*Macacus cynomolgus*) from India, presented by the Rev. E. L. Marrett; a Cape Zorilla (*Ictonyx zorilla*) from West Africa, presented by Mr. Calman; a Laughing Kingfisher (*Dacelo gigantea*) from Australia, presented by Mr. Delves L. Broughton; a Red-beaked Weaver-bird (*Quelea sanguinirostris*) from West Africa, presented by Mr. W. H. Simmonds; a Passerine Owl (*Glaucidium passerinum*), European, presented by Miss Turner; a Sumatran Rhinoceros (*Rhinoceros sumatrensis*) from Malacca, an Anubis Baboon (*Cynocephalus anubis*), a Macaque Monkey (*Macacus cynomolgus*), a Bonnet Monkey (*Macacus radiatus*) from India, a Kinkajou (*Cercoleptes caudivolvulus*) from South America, deposited; a Chinchilla (*Chinchilla lanigera*), born in the Gardens.

CYON'S RESEARCHES ON THE EAR

I.

THIS able and elaborate thesis, presented by Dr. de Cyon to the Medical Faculty of Paris, contains a further contribution of facts and speculations in reference to the function of the semicircular canals of the internal ear, a subject upon which, as Professor of Physiology in St. Petersburg, he had previously published an important and interesting paper.

As a knowledge of the form and position of these organs is absolutely necessary to enable the reader to follow a discussion of the theories as to their use, we shall preface this notice of Dr. de Cyon's thesis with a short anatomical statement.

The system of semicircular canals, which exists in the internal ear of all vertebrates, while differing greatly in size in different animals, is so nearly the same in general arrangement, that a description of it as found in man will be sufficient for our present purpose.

These organs are lodged in a bony cavity continuous with the cochlea which contains the organ of hearing. The vestibule is an irregular rounded chamber. In its walls are five openings leading to the semicircular canals. These are tunnels in the bone having an elliptical or circular section, and opening at each end into the vestibule. The central line or axis of each canal lies nearly in one plane (which we may call the plane of the canal), and is approximately an arc of a circle. At one end of each canal there is an enlargement called the ampulla. The diagram (Fig. 1) represents a section through the axis of one of the canals.

The planes of the three canals are very nearly at right angles to one another. The canals are named from their position—the horizontal, the superior, and the posterior; the two latter unite at their non-ampullary ends before joining the vestibule, so that

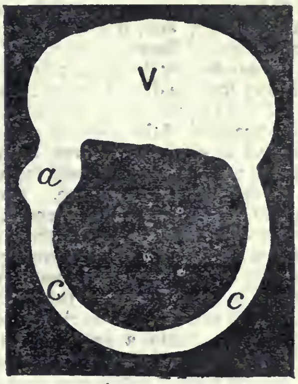


Fig. 1.

Section of bony labyrinth showing vestibule and one of the semi-circular canals. V, vestibule; *c c*, canal; *a*, ampulla.

there are five, and not six, openings into the vestibule—three ampullary, one for each canal, and two non-ampullary, one for the horizontal and one common to the superior and posterior canals.

The plane of the horizontal canal is nearly horizontal in the ordinary position of the head in all animals,[1] and is always at right angles to the mesial plane; the planes of the other two canals make nearly equal angles with the mesial plane. These relations are indicated diagrammatically in the accompanying sketch (Fig. 2), from which, to prevent confusion, the vestibule has been omitted.

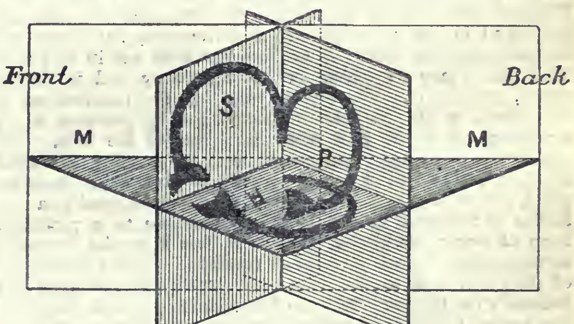


Fig. 2.

Diagram showing the relations of the planes of the three semi-circular canals of the left ear to each other and to the mesial plane. M M, mesial plane; S, P, H, the planes of the superior, posterior, and horizontal canals respectively.

In the bony labyrinth just described is inclosed a membranous labyrinth, similar in form, and consisting of the utricle, lodged in the vestibule, and of three membranous semicircular canals, each furnished with a membranous ampulla. The membranous labyrinth does not fit tight into its bony case—the utricle is much smaller than the vestibule (which contains, besides, other organs connected with the cochlea), and the diameter of the membranous canals is not more than one-fifth of that of the osseous canals. In the ampullæ the difference is not nearly so great; here there is only a narrow space between the bone and the membrane. The entire cavity is thus divided into two spaces, one within and one around the membranous labyrinth; each is filled with a liquid named the endolymph and the perilymph respectively. The external space contains, in addition to the perilymph, connective tissue attaching more or less firmly the membranous canals to the periosteum, and the blood-vessels and nerves which supply the membranous labyrinth. The nerves are distributed to a spot called the *macula acustica*, in the utricle,

[1] Recherches expérimentales sur les Fonctions des Canaux semi-circulaires et sur leur Rôle dans la Formation de la Notion de l'Espace. Par Elie de Cyon, M.D., &c., Lauréat de l'Institut de France.

[1] In man the plane of the horizontal canal slopes somewhat downwards and backwards, so that it becomes horizontal when the head is slightly bent forwards.

and to a crescent-shaped ridge, the *crista acustica*, near the middle of each ampulla. The nerves in each case end in hair cells, the hairs of which project into the endolymph. These nerves are derived from the vestibular branch of the so-called auditory nerve, or *portio mollis* of the seventh pair.

The remarkably regular, and, if we may use the expression, purpose-like arrangement of these organs must strike every one, and the mind naturally searches for the use of this curious geometrical apparatus. A theory long held, and not yet wholly abandoned, to which indeed Dr. de Cyon seems to attach some value, connects it with the perception of the *direction* of sound. The idea is not unnatural, and is obviously derived from the nearness of the apparatus to the organ of hearing and from the relation of its form to the three dimensions of space. No explanation has ever been given *how* it can serve this purpose, and a sufficient proof that it does not do so is supplied by the fact—easily tested by any one—that we have no means of knowing the direction of sound except by two or more simultaneous or successive observations. If a sound is heard louder in the right ear than in the left we conclude that it comes from the right, and by turning the head round we quickly obtain a sufficient number of observations to enable us to judge of the exact direction. If a sudden abrupt noise is made at a point equidistant from the two ears, we do not know its position unless we *see* what produces it. This theory may therefore be at once dismissed.

The first scientific inquiry into the function of the semicircular canals was made by Flourens in 1828. His experiments were conducted with an amount of care and the results described with a degree of accuracy and clearness not surpassed by any recent investigator. He observed that the section of a membranous canal was always followed by movements of the head, or even of the body of the animal *in the direction of the divided canal;* in other words, by oscillatory movements of rotation about an axis at right angles to the plane of the divided canal.

Flourens has been followed by various observers, Harless, Czermak, Brown-Sequard, Vulpian, and Löwenberg, who have confirmed the results above described, and referred them either to disturbances of hearing or to injury of the cerebellum.

An entirely new step was taken in 1870 by Prof. Goltz. Flourens had already hinted that the semicircular canals are concerned in the co-ordination of movements, but Goltz first devised a physical theory to explain how they act in this way. This theory may be called the statical theory, and is, shortly stated, as follows:—We may suppose that the terminations of the nerves in the ampullæ are stimulated by pressure or by stretching, as is the case with the nerves of the skin. The endolymph contained in the canals necessarily presses most upon that part of the wall of the cavity which is situated at the lowest point, and each position of the head will correspond to a particular distribution of pressure, and therefore to a particular form of nerve stimulation; we have thus a special sensation corresponding to each position of the head. That a knowledge of the position of the head and the power of regulating it is necessary for the preservation of the equilibrium of the body, was proved by Goltz by a very ingenious experiment. He fixed the head of a pigeon in an unnatural position by attaching its occiput to its breast, so that its beak was turned up and the vertex looked forward. He found that the animal so treated walked with great difficulty, and was quite unable to fly, and exhibited many of the phenomena observable in animals whose semicircular canals had been cut or destroyed.

The next investigations of importance as to the function of these organs are those of Prof. De Cyon in 1872. He repeated with great experimental skill, and with ingeniously-contrived modifications, the operations performed by Flourens and by Goltz, obtained the same results, and, guided by his medical observations on diseases accompanied by loss of equilibrium, formulated his theory as follows:—The semicircular canals give us a series of unconscious sensations as to the position of our heads in space; each canal has a strictly determined relation to one of the three dimensions of space; the loss of equilibrium and the other disturbances of locomotion caused by the section of the canals are solely due to the disorder of these sensations.

Before considering the next step in the development of the theory we must go back and examine the experiments on vertigo made by Purkinje about the time when Flourens first investigated the function of the semicircular canals.

Every one knows that if we stand up and turn round about a vertical axis and keep up this rotation for some time and then stop, we see, or think we see, surrounding objects moving round. Purkinje studied the conditions under which this apparent rotation occurs, and arrived at the following conclusions, which have been confirmed by all succeeding observers:—1. That the direction of the apparent motion of surrounding objects depends upon the direction of the preceding real motion of our body, and is always opposite to it. 2. That the axis about which the apparent motion takes place is always that line in the head which was the axis of the preceding real rotation. Thus, if we turn round with our head bent forward so as to look straight down at the floor, and then stop, keeping the head in this position, the apparent rotation takes place about a vertical axis; but if, when we stop, we lift up our head and look forwards, the apparent rotation takes place about a fore and aft axis, the fore and aft axis having been vertical while the real rotation occurred. A trick, illustrating this principle, is sometimes played on persons ignorant of this law of giddiness. They are asked to take a poker in their hand, plant it vertically on the floor in front of them, bow down so as to touch the end of the poker with their forehead, and walk quickly three or four times round the poker, then rise up and walk to the door. The apparent rotation takes place about a fore and aft axis, because the fore and aft line in the head was the axis of the real rotation; they see objects rise up on the one side and fall down on the other; the floor seems to incline itself to one side, and equilibrium becomes impossible. This experiment may be varied in many ways: thus, instead of the forehead, one ear may be placed on the end of the poker; the rotation then takes place about a right and left axis—the line from ear to ear. On rising, apparent rotation occurs about this line as an axis, and the floor in front seems to slope up or down according to the direction of the original real rotation.

Purkinje explained these phenomena thus:—"During the rotation of the body about its longitudinal axis, the brain, in virtue of its soft consistence, ought to have a tendency to remain a little behind the movement of the walls of the skull. This is the same phenomenon which we observe in a liquid when the vessel containing it is set in rotation. The particles of the liquid preserve their position relative to the external space, until their adhesion to the walls of the vessel forces them to take part in the motion of the latter. The cohesion of the brain is too great to allow of the reproduction of the same phenomenon exactly; but as the brain is soft and capable, to a certain extent, of internal displacement, it has some of the properties of liquids. We must therefore admit that a movement more or less intense must produce a displacement and relaxation of its parts, although an actual rupture of continuity cannot occur. Such distortions should produce the same disturbances as actual mechanical lesions, and differ from them only in degree."

This explanation is adhered to, in the main, by Dr. de Cyon, and although we do not admit that such tortional deformation of the brain is the chief or usual physical cause of the giddiness above described, it is probable that it has something to do with the phenomena when the rotation is very rapid and is suddenly stopped.

We now come to the modification of the theory of Goltz, which induced Dr. de Cyon to resume his experiments on the semicircular canals. This modification was made nearly at the same time by Prof. Mach, of Prague, Dr. Breuer, of Vienna, and Dr. Crum Brown, of Edinburgh. The papers in which they stated their views were written independently, and were published in the order above-mentioned within a period of about six weeks.

The views of the three observers are not quite identical, and it will be necessary to point out in what they differ; but they agree in so many details that a general description of their theory is possible. We have above called Goltz's theory *statical;* in opposition to it we may call the Mach-Breuer-Brown theory *kinetical.*

Goltz regarded the semicircular canals as organs which, by difference of pressure in different parts of the system, give us a notion of the *aspect* of the head in space. Mach, Breuer, and Brown regard them as organs which, by virtue of the *inertia* of the contained liquid and movable soft parts, give us a notion of the *change of aspect* of the head.

The simplest form of this theory is that in which it is stated by Dr. Breuer:—

"In a system of three ring-shaped tubes, approximately at right angles to one another, and filled with liquid, as is the case in the semicircular canals, there are produced by every

rotational movement of the whole system (that is, of the head) currents of liquid in a direction opposite to that in which the head is turned. The amount of the flow in each canal depends upon the plane in which the head is turned and upon the rate of the rotation. There are perfectly fixed relations between the rotational movement of the head and the currents in the inclosed liquid; if these currents can be perceived they will give us an exact account of the rotational movement of the head. We may regard, as possible organs for the perception of the currents, the so-called 'auditory hairs' which project at right angles inwards from a widened and flattened part of the canal; they are thus placed so as to be most sensitive to currents in the canal, and are on the other hand connected to nerves, of which they form the end-organs.

"To turn these facts to account in the sense of Goltz's theory we must assume that every flow of the endolymph, perceived by the ampullary nerves, produces a sensation of rotation of the head in the plane of the canal in which the flow takes place, and in a direction opposite to it, but that the perceptions of the six ampullæ of the two labyrinths combine to form a joint sensation. . . . .

"Our assumption has a necessary consequence. If the rotation of the head (of course along with the body) is kept up, the initial backward flow of the endolymph will be destroyed by friction against the walls. If the head then suddenly stops, the endolymph must, in virtue of its inertia, flow on in the sense of the rotation of the head; a sensation will therefore be produced of rotation of the head and body in a direction opposed to that of the previous rotation."

In this view the endolymph is held to lag behind the rotational movement of the head when this movement begins;—when the movement has continued at a uniform rate for some time, the endolymph is constrained, by fluid friction, to take part in the movement of the head, and if then the rotation of the head stops, the endolymph moves on. We have, thus, two ways in which a relative motion can occur between the endolymph and the walls of the cavity containing it:—1. When the head begins to move—here the walls leave the fluid behind. 2. When the head stops—here the fluid flows on. In both cases the sensation of rotation is felt. In the first this sensation corresponds to a real rotation, in the second it does not, but in both it corresponds to a real acceleration (positive or negative) of rotation, using the word acceleration in its technical kinematical sense.

Mach's view differs from Breuer's in this, that while Breuer assumes an actual flow of endolymph through the canals, Mach believes that the very narrow bore of the canals will preclude such a flow—the friction being so great that the most abrupt rotational movement of the head will not produce sufficient difference of pressure to cause an actual current. Instead of a current there will be produced a change of pressure in the ampulla, which would produce a current were the canal wider, and this change of pressure may be sufficient to act on the hair-cells, and irritate the ends of the nerves.

In Brown's statement of the theory, not the endolymph only, but the whole liquid and soft contents of the bony canals are supposed to lag behind the movement of the head, and in his first paper he suggested that there might be a relative motion between the bony and the membranous canals. This view, founded on the statement to be found in various anatomical text-books, that the membranous canals float nearly loose in the bony canals, is scarcely tenable when we know that the former are somewhat firmly attached at one side to the periosteum.

Another important point in which Brown's statement of the theory differs from that of Mach and from that of Breuer, lies in his regarding the two labyrinths as forming one organ, all the six canals of which are required to form a true conception of the rotatory motion of the head.

The doctrine of specific nervous action, now we believe generally accepted by physiologists, implies that while greater or less stimulation of an end-organ produces difference of sensation, a variety in the *mode* of stimulation cannot be perceived. Flow through the ampulla from the utricle to the canal on the one one hand, and from the canal to the utricle on the other, must produce a precisely similar sensation if the hairs of the hair-cells are equally moved. We must therefore look further for an explanation of our power of distinguishing between rotation in the one sense and rotation in the other sense about the same axis.

Mach was at first inclined to suppose that in each ampulla there are two sets of nerves each sensible to rotation in one sense only. He now adopts the explanation proposed by Brown, who based it upon the fact established by careful measurements in a considerable number of animals, that the *six* canals are sensibly parallel two and two. Thus the two horizontal canals are in the same plane, while the superior canal of one side is in a plane nearly parallel to that of the posterior canal of the other side. Further, in each of these three pairs (right and left horizontal, right superior and left posterior, right posterior and left superior), the two canals are so placed that when rotation takes place about the axis to which they are perpendicular, one of the two canals moves with its ampulla preceding the canal, so that the flow, or tendency to flow, is from ampulla to canal, while in the other the ampulla follows the canal and the flow, or tendency to flow, is from canal to ampulla. If, then, we suppose that flow from ampulla to canal—or, adopting Mach's view as stated above, *increase* of pressure in the ampulla—alone stimulates the hair-cells, while no effect is produced by flow in the opposite direction—or by *diminution* of pressure in the ampulla—we have in the six canals a mechanical system capable of giving us an accurate notion of the axis about which rotation of the head takes place, and of the sense of the rotation. To this explanation Dr. de Cyon objects that it assumes two organs, the superior canal of one side, and the posterior canal of the other side, which are not anatomically fellows, to be physiologically fellows. To this it is sufficient to answer that the motions which these two organs are supposed to perceive are produced by altogether different muscles. Let us take the case of the right superior and left posterior canal—the former is sensitive to rotation in one sense about an axis approximately passing through the left eye and the right mastoid process, a motion produced by muscles on the right side of the front of the neck, while the latter canal is sensitive to exactly the contrary motion about the same axis, and this motion is produced by muscles on the left side of the back of the neck. It is surely unreasonable to expect anatomical relations to exist between the organs perceiving two motions which do not exist between those producing them.

ALEX. CRUM BROWN

(*To be continued.*)

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

WE have already referred to the mathematical courses for session 1878-9 in Johns Hopkins University, Baltimore. The following is the detailed programme:—I. Prof. Sylvester will lecture on (*a*) determinants, (*b*) modern algebra, (*c*) theory of numbers. II. Dr. Story will lecture on (*a*) higher plane curves, (*b*) solid analytic geometry, (*c*) quaternions, (*d*) elliptic functions. III. Mr. Craig will lecture on hydrodynamics. IV. Lectures will be given by appointed instructors on (*a*) differential equations, (*b*) analytic mechanics, (*c*) conic sections, (*d*) theory of equations, (*e*) differential and integral calculus. V. Mathematical Seminarium:—A Mathematical Seminarium is conducted under the guidance of the Professor and Dr. Story; it comprises all the instructors and students of mathematics in the university. At its monthly meetings, besides occasional papers, such topics as may from time to time suggest themselves in the course of reading to the students or instructors, or may otherwise be of general interest to persons pursuing mathematical studies, are made the subject of free oral discussion. VI. Scientific Association:—The Scientific Association of the Johns Hopkins University meets once a month for the discussion of subjects of general scientific interest. At these meetings an opportunity is afforded for communicating abstracts of recent mathematical progress, as well as the results of individual research. VII. Mathematical Journal:—"The American Journal of Mathematics" is published quarterly in the City of Baltimore, under the auspices of the Johns Hopkins University, and affords an efficient medium of intercourse between members of the university engaged in original investigation, and a wide circle of mathematicians in America and in Europe. VIII. All the mathematical journals published at home and abroad are taken in by the university. At the Peabody Library complete sets of "Crelle's Journal" and the most important scientific transactions are also accessible. The university library and reading rooms are open daily from 9 A.M. to 10 P.M.

THE Calendar of the Yorkshire College for its fifth session, 1878-9, forms a volume of 140 pages. The college has now day classes in the following subjects:—Mathematics, experimental

physics, chemistry, geology and mining, coal mining, biology, engineering, classical literature and history, modern literature and history, modern languages, oriental languages, and textile industries, and evening classes in all the above except experimental physics.

IN reference to the question of help for lectures to the scientific societies of English public schools, a correspondent sends a Harrow list as a suggestion to other schools. He believes that all the hon. members who are masters shown in the list have delivered addresses to the society; the rule always was to invite the most eminent among the strangers who gave lectures to become hon. members. Hence several well-known names connected with literature or science are among the latter.

THE Working Men's College, which was founded by the late Frederick Maurice, in 1854 (and which naturally sustained a heavy loss by his lamented death in 1872), with a praiseworthy desire to extend its usefulness, has arranged for a series of general and popular lectures, which are intended to be perfectly free, not only to Students of the College, but also to the general public. With this view the Council has managed to secure the aid of such men as Professors Corfield and Lowne, Dr. Casson and Mr. Frederick Harrison, all of whom take part in these lectures between this and Christmas. This attempt to render the public uses of the College much more prominent than heretofore will not, as it appears, in any way interfere with its ordinary and recognised functions, and will not in any degree impede its class teaching, which has always been of the highest character. Various courses of scientific lectures by Mr. Dunman and Mr. Owen are announced.

SCIENTIFIC SERIALS

Journal of the Cincinnati Society of Natural History, July. Vol. i., No. 2.—This number gives earnest that good work is meant by the members. Its contents are chiefly interesting to palæontologists, who will find in it a list of lower silurian fossils of the Cincinnati group, by Messrs. J. Mickleborough and A. G. Wetherby, together with descriptions of many new forms found in these strata, by Messrs. Ulrich and Miller.

Reale Istituto Lombardo di Scienze è Lettere, Rendiconti, vol. xi. fasc. xiii.—We note the following papers in this number:—Causes and circumstances influencing hereditary transmission in animals (continued); Participation of the nervous system in the phenomenon of fecundation, by Signor Lemoigne.—Anæsthesia and anæsthetics in mediæval surgery, by Prof. Corradi.—Influence of water on the spinning of the cod of the silkworm, and on the quantity and quality of the silk, by Prof. Gabba and S. Textor.—On some facts relating to saccharification of amides in the digestive process, by Dr. Solera.

Journal of the Franklin Institute, August.—This number opens with a discussion, by Mr. Isherwood, of some instructive experiments on the expansion of steam in the steam-engine.—A new method of grinding glass specula is described by Prof. Elihu Thomson, the principle of it being the fact that when two equal discs of glass or other material are ground together, one above the other, the under one always becomes convex, while the upper one becomes concave, and by making the strokes of the upper disc wide and sweeping, this change of form may be greatly accelerated.—Dr. Morton gives an account of the singing telephone as made at the Stevens Institute of Technology.—A new method of reduction for diffraction spectra observations is communicated by Dr. Rosenberg.—The problem of perforated pipes, as applied to "sprinklers" (a pipe system lately introduced into cotton-mills for preventing the spread of fires), is investigated by Mr. Frizell.

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, September 30.—M. Fizeau in the chair.—The following papers were read:—Formation of an astronomical museum at the Observatory of Paris, by M. Mouchez. This is to include portraits of astronomers and *savants*, a collection of medals, drawings and photographs of celestial objects and phenomena, ancient instruments, &c.—Experimental facts showing that abundant sudoral secretions are not necessarily connected with excessive activity of cutaneous circulation, by M. Vulpian. In a dying cat, *e.g.*, when the heart's action is much weakened, and the digital parts are bloodless, the sweat exudes freely from these parts.—Remarks on the phonograph and the telephone, by M. Bouillaud.—Determination of the exact number of irreducible co-variants of the binary cubo-biquadratic system, by Prof. Sylvester.—Industrial utilisation of solar heat, by M. Mouchot. This describes experiments made during the Exhibition. *Inter alia*, he set in action, on September 2, a solar receiver with mirror having an aperture of about twenty square metres. It had, at the focus, an iron boiler weighing, with accessories, 200 kilogrammes, and having a capacity of 100 litres (30 for the steam chamber and 70 for the liquid). In half-an-hour the 70 litres were raised to boiling, and the manometer soon registered 6 atm. pressure. On September 22, with slightly veiled sun, he got 6·2 atm., and worked, under a pressure of 3 atm., a Tangye pump raising 1,500 to 1,800 litres of water hourly to the height of 2 m. With a clear sky on the 29th ult. 7 atm. was reached.—Discovery of a small planet at the observatory of Ann-Arbor, by Mr. Watson.—On intra-mercurial planets, by M. Gaillot.—On molecular attraction in its relations to the temperature of bodies, by M. Levy. To know all the isothermal and all the adiabatic lines of a body, and so to be able to study it completely, it is necessary and sufficient to know *two* of its isothermal lines and *only one* of its adiabatic lines.—On losses of charge produced in the outflow of a liquid when the section of the flow undergoes a sudden increase, by M. Boussinesq.—On the rotary power of quartz and its variation with the temperature, by M. Joubert. The angular coefficient of the curve of variation increases at first pretty rapidly up to 300°. From this to 840° (the boiling point of cadmium) it is nearly constant and the curve nearly a straight line with point of inflexion about 500°. Beyond 840° and up to 1,500°, the rotatory power increases only with extreme slowness. With a quartz of 46·172 mm., giving a rotation of 1,000° at zero, the increase from 300° to 900° is twelve minutes per degree. With a quartz of only 11 mm. the increase would still be three minutes per degree. Thus quartz makes an extremely sensitive thermometer, with the essential condition of comparability.—Phonic wheel for regularisation of the synchronism of motions, by M. Lacour. An iron-toothed wheel turns with its teeth very near an electro-magnet which is caused to exert periodic attraction by means of a vibrating diapason.—On the presence of isopropylic, normal butylic, and secondary amylic alcohols in the oils and alcohols of potatoes, by M. Rabuteau.

CONTENTS

THURSDAY, OCTOBER 17, 1878

ON THE SCIENCE OF EASY CHAIRS

THERE is a reason for everything, if we can only find it out, but it is sometimes very hard to discover the reasons of even the very simplest things. Every one who has travelled much, and even those who have merely looked through books of travels, must have been struck by the variety of attitudes assumed by the people of different countries. The Hindoo sits down on the ground with his knees drawn up close to his body, so that his chin will almost rest upon them; the Turk squats down cross-legged; the European sits on a chair; while the American often raises his legs to a level with his head. Nor are the postures assumed by the same people under varying circumstances less diverse. Climate or season, for example, will cause considerable alteration in the posture assumed, as was well shown by Alma Tadema, in his pictures of the four seasons exhibited in the Academy a year ago. In his representation of summer he painted a woman leaning backwards on a ledge, with one leg loosely hanging down, while the other was drawn up so that the foot was on a level with the body. In the picture of Winter, on the other hand, we saw a figure with the legs drawn up in front of the belly. The reason for these different postures has been explained by Rosenthal. The temperature of the body, as is well known, is kept up and regulated by the circulation of the blood through it, and a great proportion of the blood contained in the whole body circulates in the vessels of the intestines. Now the intestines are only separated from the external air by the thin abdominal walls, and therefore any change of temperature in the atmosphere will readily act upon them, unless they be guarded by some additional protection. The Hindoos are well aware of this, and they habitually protect the belly by means of a thick shawl or cummerbund, thus guarding themselves against any sudden change of temperature. This precaution is also frequently adopted by Europeans resident in hot climates, and is even retained by them after returning to England. But the function of the cummerbund may, to a certain extent be fulfilled by change of posture alone. When the legs are drawn up, as in the picture of Winter already referred to, the thighs partially cover the abdomen, and taking the place of additional clothing, aid the abdominal walls in protecting the intestines and the blood they contain from the cooling influence of the external air.

Thus it is that in cold weather, when the quantity of covering in bed is insufficient, persons naturally draw up their legs towards the abdomen, so as to retain as much heat as possible before going to sleep. In hot weather, on the contrary, they wish to expose the abdomen as much as possible to the cooling influence of the atmosphere. The posture depicted by Alma Tadema is the most efficient for this purpose. It no doubt answers the purpose to lie down flat on one's back, but in this position the abdominal walls are more or less tight, whereas, when one of the legs is drawn up as in the painting just alluded to, the walls are relaxed, and, the intestines not being subject to any pressure, the blood in them will circulate more rapidly, and the cooling process be carried on more effectually. In this attitude also the thighs are completely separated and loss of heat allowed from their whole surface.

Varying conditions of fatigue also alter the postures which people assume. When slightly tired one is content to sit down in an ordinary chair in the position of the letter N with the middle limb horizontal. As we get more and more fatigued we usually assume positions in which the limbs of the N become more and more oblique, the trunk leaning backwards and the legs extending forwards. If we lie down in bed on our back the legs will probably become straight, but if we rest upon our side they will be more or less bent. The straightness of the legs in the supine position is simply due to their weight, which is then supported at every point by the bed, but when we lie on our sides the genuflexion of the legs is most agreeable, because not only are the muscles more perfectly relaxed, but, as the late Prof. Goodsir pointed out, the bones which form the knee-joint are slightly removed one from another, and thus the joint itself, as well as the muscles, passes into a state of rest. Some of the bamboo easy chairs manufactured in India allow us to obtain the advantages of both positions. These chairs are made in the form of a somewhat irregular straggling W, and in them one can lie on one's back with every part of the body thoroughly supported, and the knees bent in the same way as they would be if one lay upon one's side.

Thus simple inaction, the relaxation of muscles, and the laxity of joints, are some of the factors necessary for complete rest, and an easy chair, to be perfect, must secure them all.

But it is possible for an easy chair to secure all these, and yet be imperfect. We have just said that usually, as the fatigue becomes greater and greater, the tendency is to assume the position of the N with the limbs at a more or less obtuse angle, but when sitting in an ordinary chair we find relief from raising the feet by means of a footstool, although this tends to make the angles of the N more acute instead of more obtuse. Still more relief, however, do we obtain when the legs are raised up on a level with the body by being placed upon another chair, or by being rested on the Indian bamboo seat already described. If, in addition to this, the legs are gently shampooed upwards, the sensation is perfectly delightful, and the feelings of fatigue are greatly lessened. To understand how this can be, it is necessary for us to have some idea as to the cause of fatigue. Any muscular exertion can be performed for a considerable time by a man in average health, without the least feeling of fatigue, but by and by the muscles become weary, and do not respond to the will of their owner so readily as before; and if the exertion be too great, or be continued for too long a time, they will ultimately entirely refuse to perform their functions. The muscle, like a steam-engine, derives the energy which it expends in mechanical work from the combustion going on within it, and this combustion, in both cases, would come to a standstill if its waste products or ashes were not removed. It is these waste products of the muscle which, accumulating within it, cause fatigue, and ultimately paralyse it. This has been very neatly shown by Kronecker, who caused a frog's muscle, separated from the body, to contract until it entirely ceased to respond

to a stimulus. He then washed out the waste products from it by means of a little salt and water, and found that its contractile power again returned, just as the power of the steam-engine would be increased by raking the ashes which were blocking up the furnace and putting out the fire. These waste products are partly removed from the muscles by the blood which flows through them, and are carried by the veins into the general circulation. There they undergo more complete combustion, and tend to keep up the temperature of the body. At the same time, however, according to Preyer, they lessen the activity of the nervous system, producing a tendency to sleep, and in this way he would, at least to some extent, explain the agreeable drowsiness which comes on after muscular exertion. It would seem, however, that the circulation of the blood is insufficient to remove all the waste products from the muscles, for we find that they are supplied with a special apparatus for this purpose. Each muscle is generally ensheathed in a thin membrane, or fascia, and besides these we have thicker fasciæ ensheathing whole limbs. These fasciæ act as a pumping apparatus, by which the products of waste may be removed from the muscles which they invest. They consist of two layers, with spaces between. When the muscle is at rest these layers separate and the spaces become filled with fluid derived from the muscle, and when the muscle contracts it presses the two layers of its investing sheath together, and drives out the fluid contained between them. This passes onwards into the lymphatics, where a series of valves prevent its return, and allow it only to move onwards, till at last it is emptied into the general circulation.

In strong and healthy people the veins and lymphatics together are quite able to take up all the fluid which the arteries have supplied to the muscles, and thus prevent any accumulation from taking place either in them or in the cellular tissue adjoining them, or at least prevent any such accumulation as might become evident to the eye. In delicate, weakly persons, or in those who suffer from certain diseases of the vascular system, this is not the case ; and after standing or walking for a long time the legs become swollen, so that the boots feel tight, and sometimes even a distinct impression may be remarked at that part of the ankle which was covered by the boot. In such persons we can actually see the swelling disappear, after the feet have been kept rested for some time on a level with the body, and it may be removed more quickly still by gently and steadily rubbing the limbs in one direction from below upwards. It is almost certain that what we thus see in weakly persons occurs to a slighter extent in all, and that even in the most healthy person after a long walk a slight accumulation of fluid, laden with the products of muscular waste, occurs both in the muscles themselves, and in the cellular tissue around them, even although we cannot detect it by simple inspection. So long as the limbs of such a person hang down, the force of gravity retards the return both of blood through the veins and of lymph through the fasciæ and lymphatics, and thus hinders the muscles from getting rid of those waste products which caused the fatigue. When the legs are raised, this hindrance is at once removed, both blood and lymph return more readily from the muscles, carrying with them those substances which had been formed by the muscles of the limbs during the exertions which they had undergone when carrying the body about. So long as these substances remained where they had been formed, they might cause in the muscles of the legs an undue amount of fatigue, although when distributed over the body generally, they may produce only a pleasing languor. When the legs are long, the obstruction to the return of blood and lymph is of course greater than when they are short, and this return will take place more readily when the legs are raised above the body than when they are only on a level with it. This may be one of the reasons why some of our long-legged American cousins are so fond of raising their feet to a level with their heads, or even higher, although it is very probable that there are reasons still more powerful, which we may discuss at a future time.

It has already been mentioned that the lymph is propelled along the interstices of the fasciæ into the lymphatic vessels by the intermittent pressure which the muscle exerts upon them from within, and it seems natural to suppose that the flow may also be aided by a pressure from without, in the form of shampooing. Even when the hand is rubbed backwards and forwards upon the leg it will relieve fatigue, but the relief is greater when the leg is firmly grasped and the hand moved gently upwards so as to drive onwards as much as possible any fluid which may have accumulated in the limb, and the grasp being then relaxed, the same process should be repeated.

But while the lymph is thus most readily removed by the pumping action of intermittent pressure either of the hand without or of the muscles alternately contracting and relaxing within, it seems to us probable that this process may also be aided by steady, constant pressure from without. No doubt it is impossible for such a steady pressure to take the place of the regular pumping action produced by the alternate contraction and relaxation of the muscles when in action, yet it will have a somewhat similar action, though to a very much less extent. For at each beat of the heart, as Mosso shows, the entire limb is distended by the blood driven into the vessels, and during the pauses between the beats it again becomes smaller. Each pulse, therefore, by distending the whole limb and each individual muscle will press out a little of the fluid contained in the fasciæ in the same way as the contractions of the muscles themselves, and it seems to us probable that it is the aid which is afforded to this process by the gentle pressure exerted on the outside of the legs by a seat which supports them along their whole extent, that renders such a seat so peculiarly restful and agreeable. For an easy chair to be perfect, therefore, it ought not only to provide for complete relaxation of the muscles, for flexion and consequent laxity of the joints, but also for the easy return of blood and lymph not merely by the posture of the limbs themselves, but by equable support and pressure against as great a surface of the limbs as possible.

Such are the theoretical demands, and it is interesting to notice how they are all fulfilled by the afore-mentioned chair in the shape of a straggling W, which the languor consequent upon a relaxing climate has taught the natives of India to make, and which is known all over the world.

SCIENTIFIC BALLOONING

Histoire de mes Ascensions. Recit de Vingt-quatre Voyages aériens (1868-1878) *précédé de simples Notions sur les Ballons et la Navigation aérienne.* Par Gaston Tissandier. Illustré de nombreux dessins par Albert Tissandier. (Paris: Dreyfous, 1878.)

M. TISSANDIER has just published a handsome and well-illustrated volume, giving a history of his twenty-four ascents with an account of their scientific results, which are of very considerable importance. M. Tissandier is one of the most scientific of modern aeronauts, and has, by his ascents, made important additions to our knowledge of atmospheric phenomena. The work to which we refer ought to interest many readers; it is not only full of adventures, of "hairbreadth 'scapes," of humorous incidents, and beautiful descriptions of atmospheric scenes, which ought to prove attractive to the general reader, but contains, besides, a large amount of data of great importance in connection with the physics of the air.

Aeronautics, M. Tissandier tells us, may be divided into five distinct branches:—1. The balloon itself. 2. Meteorological aerostation, in the scientific exploration and study of the atmosphere. 3. Military ballooning, captive aerostats, military reconnaissances, the aerial post. 4. Direction of aerostats and aerial navigation. 5. "Aviation," or mechanical flight, the principle of which has been designated under the phase "heavier than air." In the first part of M. Tissandier's work will be found a *résumé* of the principal events in the history of aeronautics, from the first rude attempts down to the magnificent captive balloon of M. Giffard, which has excited so much attention during the Paris Exhibition. The author gives an account of the present state of aerial navigation, and shows what may be expected in the future, judging from facts submitted to the test of scientific reasoning.

The second part of M. Tissandier's work contains an extremely interesting and scientifically valuable account of twenty-four aerial voyages made by himself. It is about ten years since M. Tissandier made his first ascent, and the greater number of his ascents have been made in company with his brother, M. Albert Tissandier, who by the aid of his pencil has recorded the ever new, always interesting, and often incomparable spectacles which the atmosphere opens to the eye of the explorer. Many of his sketches are of great meteorological, as well as artistic, value. In the second part of the work an explanatory diagram indicates exactly the route taken during each journey, the altitude attained, the nature and situation of the clouds, the direction of the currents, their temperature and the atmospheric circumstances connected with them. Numerous wood engravings, from drawings by M. Albert Tissandier, represent the marvellous spectacles observed, the effects of clouds, sunsets, and the various optical phenomena witnessed during the journeys described. Indeed, the work is so luxuriantly got up, and so profusely illustrated, that we are bound to infer either that M. Tissandier's publisher is unusually generous or that France must possess a considerable reading public of more than average intelligence.

The work contains a detailed description of the structure of M. Giffard's monster captive, to the understanding of which the many well-executed illustrations are a great help. An appendix gives the interesting experiments as to carbonic acid in the air made in the *Zenith* by MM. Tissandier and Mangon, and a detailed description of Giffard's new apparatus for the preparation of hydrogen in large quantities.

In a recent lecture at the Sorbonne M. Tissandier endeavoured to point out some of the important services which aeronautics might be made to render to science. A purely theoretical science has necessarily few admirers. It should not be so, however, with the investigation of aerial phenomena. This important department of the physics of the globe is not only useful to the sailors and agriculturists of all nations; it is also of great service in public hygiene. But in order that meteorology, quite a modern science, might be instituted, there should be no doubt of the materiality of the air, thanks to the conceptions of the Galileos, Torricellis, and Pascals. The Meteorological Society of the Palatinate was only founded in 1781; only then were registrations of atmospheric variations begun. The invention of the telegraph soon permitted simultaneous observations to be made, and centralised and led to the hope that prevision of the weather would cease to be an unrealisable chimera. M. Tissandier points out how serviceable to oceanic and river navigation are the warnings transmitted by the International Meteorological Service to the principal maritime stations. The organisation of this service enables us to follow effectively the progress of centres of depression, to discover and announce the general direction of winds, to figure their spirals across space, to establish the curves of the isobaric lines or lines of equal pressure; better still, to prevent, by means of the telegraph, more rapid even than the tempest, a great number of shipwrecks and accidents on coasts.

To discover the real cause of the movements of the air, of cyclones, of the mode of formation, of the direction and variations of the rate of winds, it is necessary, to use the expression of Biot, "prendre la météorologie par en haut," *i.e.*, to seek in the elevated regions of the atmosphere the explanation of the phenomena which occur at the bottom of the aerial ocean where human destinies are accomplished. In fact, "it is in the high regions of the air that meteors are formed, rain, snow, hail; it is there that the lightning traces its furrow, and the thunder rolls; it is there that the aurora borealis displays its plume of light, and that the aerolite shines forth and bursts. There are the upper currents which chariot the clouds. It is to these elevated regions that the science of meteorology ought to be directed."

The observer has the choice between two distinct methods of exploration. He may ascend the mountains or mount in a balloon. No doubt the former process appears at first most secure, but it is seen that the second presents, practically and theoretically, superior advantages, offers most security, and is attended with better results. It suffices to recall the obstacles encountered, the dangers incurred by a tourist guide, the obscure protagonist of the memorable labours of De Saussure, the energetic Jacques Balmat. If he was the first, in August, 1786, to strike his iron-shod staff on the crest of the giant of the Alps, it was only after many fruitful

experiments, and after having encountered many perils, and endured much suffering. With the balloon, however, there is less delay, less fatigue: "I launch," said Charles, "into the air like a bird uncaged; in ten minutes I am at a height of 10,000 feet."

If, besides, the aerostatic way is more accessible than the slope of the mountains, it is also preferable from the technical point of view. It is to be remarked, in fact, that the air of high mountains is always subject to the influence of surrounding glaciers. Moreover, these powerful geological reliefs are only formed at certain points of the earth's surface. We must not only climb, but often go far in search of these abrupt and gigantic ladders, while, by means of the balloon, we reach, almost unconsciously, with a rapidity which is prodigious, the laboratory of the principal atmospheric phenomena. M. Tissandier does not deny the utility of observatories like those of the Puy de Dôme and the Pic du Midi, valuable establishments when the services of men like M. Alluard and General de Nansouty can be obtained. The problems of the high regions demand for their solution more than one indefatigable investigator, and, whatever be the mode of investigation followed, the more numerous the workers, the sooner will success crown their efforts.

How many questions are there to solve however! Among the most interesting, and those which are now almost solved, since the researches of M. Tissandier, let us note the presence, in atmospheric dust, of spherules of the magnetic oxide of iron whose diameter scarcely exceeds the 500th part of a millimetre. These spherules, which contain nickel, present a striking analogy of composition with meteoric stones, and have evidently an extra-terrestrial origin. They have been observed in glaciers, in dust-showers like those driven by the wind of the desert; their appearance has been noticed after the explosions of bolides. The sediments left by rain-water present traces of them, and M. Tissandier himself has collected them on the upper steps of the stairs of the tower of Notre-Dôme.

To return to ballooning. Since the discovery of the brothers Montgolfier, since the experiments of Charles, Pilâtre, and the Marquis d'Arlandes, scarcely a century has passed, and yet we can count more than 20,000 ascents. In this number, unfortunately, it would be difficult to find more than 100 undertaken with an exclusively scientific purpose, and with the indispensable instruments of observation.

It was the physicist Robertson who, in 1803, commenced the series of scientific explorations of the air. Biot and Gay Lussac, in 1834, then Gay Lussac alone, accomplished ascents which are important events in meteorological investigation. In 1850 MM. Barral and Bixio made an ascent characterised by very remarkable circumstances. At the height of 7,000 metres they met with a cloud formed, not of vesicles of water, but of spangles of ice. The phenomenon has, moreover, been observed in many other aerial journeys.

M. Tissandier mentions in his book, in a very special manner, the magnificent enterprises of his venerable friend, Mr. James Glaisher, who has made more than thirty aerial voyages, almost always to very great heights. We owe to him observations of great interest, which shed a clear light on many questions, among others on those which refer to the decrease of temperature of humidity in the various strata of air. In later times MM. Camille Flammarion and De Fonvielle have devoted themselves to aeronautics, not without profit to science. Croce-Spinelli and Sivel, also, had made their *début* as masters; their tragic fate, and the miraculous escape of M. Tissandier, our readers must remember. This disastrous journey is fully described in the volume before us.

M. Tissandier endeavours to prove that the direction of the upper atmospheric currents, their rate of translation, their temperature, and their hygrometric state, can only be surely established by means of the aerostat, the observer on the earth being only able to appreciate those elements with reference to superficial winds—simple accidents which ought not to be confounded with the true aerial rivers which often roll their enormous masses above the clouds themselves.

There is room to hope, moreover, that the balloon, which has already received notable improvements, may become, at no distant day perhaps, a directable instrument, and traverse the fields of air as a ship ploughs those of the sea. With these attempts at obtaining control over the course of the balloon, the name of M. Henri Giffard is closely connected. Those who wish to form some idea of what the balloon is capable as an aid to the meteorologist and as a means of investigating the physics of the atmosphere, could not do better than read M. Tissandier's handsome work.

OUR BOOK SHELF

Proceedings of the Aberdeenshire Agricultural Association, 1876-78.

It has been a subject of frequent regret that while Germany and France have been active in carrying out investigations upon agricultural questions few systematic efforts in this direction have been made in our own country. We learn, therefore, with pleasure that two centres for agricultural investigations have recently been started in Scotland—one by the Aberdeenshire Agricultural Association, the other by the Highland Society. The reports of the Aberdeenshire Association for 1876 and 1877 are now before us, and demand a short notice.

The subject selected for investigation has been the turnip crop, the special object of the experiments being to ascertain the value of various forms of phosphatic and nitrogenous manures. It is impossible not to admire the pains spent on this investigation, but we are sorry to say it is equally evident that the work has been done with very little knowledge of the facts ascertained by earlier investigators of the subject. Labour is consequently spent in proving that which stands in no need of proof, while discoveries are proclaimed of facts already well known to chemists.

One principal point which the experiments are considered to have established is the little superiority of the soluble phosphate of calcium over the insoluble as a manure for turnips. Farmers are told to purchase phosphates in whatever form may be the cheapest, without reference to solubility. This is indeed a startling proposal. Mineral phosphates are at the present day treated with sulphuric acid on a very large scale, with the sole object of producing a superphosphate in which all the phosphoric acid shall be in a soluble form: this mode of treating phosphates is clearly according to the present report an entire mistake. On what experiments is this conclusion based? Will it be believed that in nearly

every case the experiment consisted in a comparison of the manuring effect of three parts of phosphoric acid in an insoluble form, with a mixture containing one part of soluble and one part of insoluble phosphoric acid. The result was somewhat in favour of the latter mixture. It will be seen at once that the experiment afforded no fair comparison of the two forms of phosphate. Besides the fatal error of mixing the soluble and insoluble phosphates together, and comparing them in unequal quantities, the amount applied to the land was far too large. Let them drill with one set of turnips 2 or 3 cwts. of superphosphate in which the whole of the phosphoric acid is soluble, and apply to another plot the same amount of phosphoric acid in the so-called insoluble form, and the result of the comparison will be very different to that at present shown.

In speaking of the effect of nitrogenous manures the report correctly states that they produce but little effect on the turnip crop. The fact is that turnips have a greater power than most farm crops of assimilating the nitrogen of the soil, and being thus able to feed themselves stand in little need of artificial help. Where, however, the soil is in an exhausted condition, nitrogenous manures will always produce a marked effect.

The analyses of the turnips grown by the experimental manures supply a variety of "new and unexpected information." Much of this is true in substance, but is already well known to agricultural chemists. We abstain from criticising, for we are sure the next report will show a far better acquaintance with the chemistry of the subject, and that the industry and zeal now displayed will finally issue in real additions to our knowledge of agricultural science.

Euclid. Books I. and II. Edited by W. H. H. Hudson, M.A. *Algebra.* By the Same. (London: The Society for Promoting Christian Knowledge).

THE Euclid is founded on Simson's second edition (1762). In addition to the text there are a few definitions and some judicious explanatory notes. The Algebra (or Primer as the author styles it) is divided into three parts, Part I. Notation, Addition, and Subtraction; Part II. Multiplication, Division, and Simple Equations; Part III. Measures and Multiples, Fractions, and Quadratic Equations. The proof here given of $-a \times -b = ab$, due to Euler, appears to be quite sound. Mr. Hudson states his belief that this proof was misunderstood by Mr. Mill in his criticism ("Logic," vol. ii., ninth edition, p. 408). This little work is an excellent one, and contains a vast amount of good matter in a small compass. Mr. Hudson has performed his task in no perfunctory manner. Both books are brought out with a view to teaching the subjects of which they treat as required by the new code. They are very neatly printed and got up.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

The Magnetic Storm of May 14, 1878

IN regard to the communication on the magnetic storm of May 14, 1878, which appeared in NATURE, vol. xviii. p. 617, it may be interesting to add some particulars furnished by examination of the magnetic photographs of the Royal Observatory.

For some days preceding May 14 the trace of the declination magnet had been very quiet, exhibiting only the ordinary diurnal change; but on May 13, at 18h. 5m., Greenwich mean time, the character of the curve abruptly and distinctly changed, small and frequent oscillations commencing then to be shown. At about 6h. on May 14 the north end of the needle began to move gradually in an easterly direction, and at about 9h. had reached a position twenty-five minutes of arc east of its usual position, the small oscillations still going on. No great change then occurred until 11h. 45m., when the north end of the needle began to move sharply westward. At midnight it had moved twenty minutes westward, occupying then its usual position nearly; almost immediately, however, it turned again eastward, and at 12h. 40m. had moved twenty minutes in that direction; after this time, with the exception of two smaller bends, no other unusual motion occurred, and the magnet gradually resumed its ordinary position. The small oscillations first spoken of ceased at about the time of the commencement of the first rapid motion, at 11h. 45m.

The commencement of the disturbance is not so distinctly perceptible in the trace of the horizontal force magnet, but continued small oscillations occur through the evening of May 14. At 11h. 45m. the northerly force had (gradually since 6h.) decreased nearly 0·01 part of the whole horizontal force; it then increased rapidly, and at midnight had about reached its usual magnitude; by 12h. 40m. it had again considerably diminished (but less than before), and after this time the magnet gradually resumed its usual position. The small oscillations ceased about midnight.

The vertical force magnet trace shows a few very small oscillations during the evening; after 7h. the force decreased gradually till midnight; at the latter time a sharper decrease occurred; at 12h. 30m. the force had altogether diminished by about 0·003 parts of the whole vertical force; after this time the magnet, rather rapidly at first, but afterwards more gradually, returned to its ordinary position.

The first start in the trace of the declination magnet, at 18h. 5m., is most distinct; the character of the trace definitely changes at that time. If the commencement of disturbance is as sharply indicated in the China, Melbourne, and Stonyhurst registers, we shall have here a well-established instance of simultaneous, or nearly simultaneous, action, at widely-separated parts of the earth's surface. The disturbance practically ceased at Greenwich, May 14, 16h. It was comparatively large as occurring in an otherwise quiet year. In years of activity these motions of the magnets, in amount, are, on many days, much exceeded.

WILLIAM ELLIS

Royal Observatory, Greenwich, October 12

P.S.—Earth currents were active at Greenwich during the whole period of the magnetic disturbance.

Cyclones and the Winter Gales of Europe

MR. S. A. HILL, in NATURE, vol. xviii. p. 617, compares together the number of hours of high wind in the British Isles with the number of West Indian cyclones observed in each year from 1869 to 1874. It may be interesting to add the result given by the Royal Observatory register. The hourly values of velocity in the years mentioned have not yet been tabulated, but adopting for comparison the number of days on which the daily velocity exceeded 500 miles, we have counted up the number of such days in each year, which numbers, for more ready comparison with those given in NATURE, we have multiplied by a constant. The comparison with the values previously printed in NATURE then stands as follows:—

	1869	1870	1871	1872	1873	1874
Cyclones (W. Indies)	0	7	3	0	1	?
Hours of high wind (British Isles) ...	714	570	537	679	571	658
Days of high wind, Royal Observatory, × 25	975	525	175	725	750	575

WILLIAM ELLIS

Royal Observatory, Greenwich, October 12

Height and Shape of Mount Hekla

I NOTICE that there is a mistake in regard to the height of Hekla in the map which accompanies my account of the eruption of February 27, 1878, for which I fear I am alone responsible. It is there stated to be 4,950 feet. The real height is 4,961 *Danish* feet = 5,108 English feet. The height has been frequently misstated. Sometimes it is asserted to be

5,700 feet, while on the other hand it has been placed as low as 4,300. It is not probable that the height has altered during the last century, for although some writers have asserted that the mountain lost 500 feet during the eruption of 1845, it has been satisfactorily proved that the crater of 1845 opened in the side of the mountain below the principal craters. The previous eruption was in 1772. Hekla is often spoken of as if it were the only volcano in Iceland, while in reality the whole island is dotted over with volcanic vents, of which Hekla is indeed the most frequently active, but by no means the highest. In fact, there are four higher mountains in the island, the highest being Oræfa Jökull—6,426 English feet.

Secondly, as to the shape of Hekla. Volcanoes often present a fairly symmetrical conical form, as we should expect from their mode of formation. This is specially the case in regard to Etna. But Hekla presents rather the appearance of a hog's back. Seen from the north or south it has a long oval outline, serrated by virtue of its three craters, and with an axis which passes from north-east to south-west. Thus the ends of the oval mass alone present the usual conical appearance of volcanoes. It is rather a line of craters than a single one; a volcanic rift elevated above the plain, with large *bocche del fuoco*. These rifts are common in all volcanic countries. In Iceland we have a notable example in Köetlagia, as we commonly call the volcano; in reality *Köetla-gja*—the Köetla rift, along the line of which various vents of fire exist. So again we have Almannagjá, the great rift at Thingvellir. In the eruption of Etna, which took place in 1865, a large rift opened in the side of the mountain, and along the line of it no less than seven small craters opened. In the last eruption of Hekla fourteen small craters opened in a line. We have to distinguish between volcanoes terminated by one large crater, which always furnishes the vent when the eruption takes place from the summit, and volcanoes terminated by a line of craters, one or other of which may be active at any one time. To the former class belong Etna and Vesuvius, to the latter Hekla and Köetla-gjá.

Apparently we cannot get out of the way of spelling Hekla *Hecla*. In one of our leading journals of October 11 I notice the spelling *Hecla*. Hekla means *hooded*, in allusion to the covering of snow, or of cloud, which so frequently rests upon its summit. *Hökull* signifies a chasuble in Icelandic. According to some writers Mount Pilatus, near Lucerne, takes its name from *Pileatus*, in allusion to its cloud-capped summit.

Marlborough College, October 13 G. F. RODWELL

Animal Intelligence

As many of the readers of NATURE have probably not seen my article on the above subject in the current number of the *Nineteenth Century*, I feel it desirable to repeat in these columns the request with which that article concludes. This request is merely that those who read it should favour me by sending to the under-mentioned address brief accounts of any well-marked instances of the display of animal intelligence which may have fallen within their own notice or that of their friends. None of these instances will be published by me without permission; but I desire to accumulate as many of such instances as possible—no matter of how dubious a character—in order that I may obtain a wide basis of suggestion as to the directions in which experiments may be most profitably employed. I may add that as the effect of publishing this invitation in the *Nineteenth Century* has been that of burying my desks in a snow-storm of letters, I should like to take this opportunity of explaining to past and future correspondents that I do not esteem their kindness the less because its bounty is too great for me to acknowledge in individual cases. GEORGE J. ROMANES

18, Cornwall Terrace, Regent's Park, N.W., October 15

The Microphone as a Receiver

I HAVE made some experiments which seem to throw light on the fact mentioned by Prof. Hughes and also by Mr. Blyth (NATURE, vol. xviii. p. 172), that the microphone, or a jar with gas cinders, may act as a receiver.

A Morse-key is set on a sounding-box; one pole of an intermitting current is connected with the lever; the other pole with the fore-part of the key (which for telegraphing purposes is connected with the positive pole of the sending battery). By regulating the screw, which is found at the after-part of the key (which for telegraphing purposes is connected with the coil of the electro-magnet), a slight contact is made between the lever and the fore-part of the key, and directly a very distinct sound is heard of the same character as that of the sound emitted by the interrupting apparatus.

A very good form of the same experiment is the following:—A leaden cylinder and a rocker are taken as used in the experiment of Trevelyan; the rocker is placed on the cylinder, and is, moreover, supported by a sharp edge; if necessary pressure downwards is applied on the stem, in order to prevent the pressure of the rocker on the cylinder being too great. The cylinder is connected with one pole of the intermitting current, the stem of the rocker with the other pole. If the pressure of the rocker on the cylinder be regulated well, a very low sound is produced, specially when the rocker is an iron one. A copper rocker or a copper plate also gives good results. The cylinder was placed on a sounding-box.

An intermitting current sent through the microphone of Prof. Broun (NATURE, vol. xviii. p. 383), as also through a jar with gas cinders, gives a feeble though perfectly audible sound. By using a stronger current, I believe the sound would have been louder. My battery was of four Bunsen cells.

In my opinion the only possible explanation of these facts is the following:—The resistance at the places of contact being relatively very great, a good part of the heat generated by the current appears here, and by dilatations the lever of the Morse-key or the rocker is uplifted. During the interruptions of the current the heat is diffused, and the lever or the rocker falls back, to be again uplifted in the next period of closed current. In this way the lever or the rocker acquires an oscillation of the same period as that of the intermitting current. That a sufficient diffusion of heat in so short a time is possible may be seen from the Trevelyan experiment itself.

My conclusion is that the action of the microphone or of the jar with gas cinders as a receiver depends upon the varying dilatation at the points of contact by the varying intensity of the current. V. A. JULIUS

Breda, Holland, October 12

Power of Stupefying Spiders possessed by Wasps.—Mimicry in Birds

MR. CECIL'S remarks on the spider-hunting wasp (NATURE, vol. xvii. p. 381) have interested me greatly, these wasps being very common here. Two species are continually hovering about the wall-plates and eaves of my house in search of their prey, which they hunt out with most praiseworthy perseverance. Both are thin-bodied, but one is half as long again and has a larger body, as also broader black rings than the other. A few remarks may be of interest to your readers.

These wasps build variously-shaped mud nests, which are met with hanging from twigs in the bush or stuck on walls in houses and under overhanging rocks. Some species use a red sandy loam, others common mud.

The nest is divided into compartments, each of which contains an ovum, and is filled with spiders, on which the larvæ feed.

For a long time I was under the impression that these spiders were killed outright, and was puzzled to find them perfectly fresh and juicy after a lapse of a fortnight, with a thermometer ranging up to 118° in the shade; but a few days ago I broke open a large nest, and was astonished to notice a constant movement in the legs of half-a-dozen spiders which were contained in one cell.

I have since then examined several nests, and invariably with the same result.

Mrs. Hubbard's explanation of the tracking described by Mr. Cecil (NATURE, vol. xvii. p. 402) is no doubt correct as far as it goes, but the wasps here seem to "go in" for every and all species of spiders, with only one exception.

My house abounds with a podgy black spider having a bright vermilion patch on the medial line of the body and two bright spots above this patch. This insect is a most venomous and dangerous neighbour; its bite is highly poisonous and inflicts excruciating agony on the person or animal bitten. I have seen a terrier succumb to its effects in eight hours, and one young man went mad in 1868 from the effects of a bite received in the wrist. Several persons have been bitten here during the last two years, and only the immediate use of ammonia and spirits saved them from serious injury. This spider is carefully avoided by the wasp, who immediately retreats on discovering that the occupant of a web belongs to this species.

Several small hunting spiders are to be found in the wasps nests which do not seem to leave any line behind them, and whose mode of life is very nomadic. I quite believe from observation that wasps can and do run these insects down by scent, although it is difficult to obtain direct proof of the fact. They seem quite capable of discerning between their prey and enemies, as shown above; and granting this, it does not seem a great stretch to a further development of instinct having the propagation of the species in view.

Can any of your readers explain to me in what manner the spiders are stupefied and not killed?

I have lately noticed cases of protective mimicry in birds which I think are worth recording.

While following a small wood-swallow (*Artamus minor*) a few evenings back, they suddenly disappeared near a large leaning gum-tree. Walking up to the spot they suddenly flew out of the trunk, which I found had been hollowed out by fire, leaving the inside charred and black. The birds had chosen this black surface as their roost, and when followed in the day-time invariably flew into this refuge. It was impossible to detect the birds when clinging to the charred surface, with which their plumage matched.

Artamus albiventris, I find, in like manner chooses a greyish back as a roost against which they are less liable to be detected. *Erythrina vespertilio* is a favourite tree with this species.

I have on several occasions taken *Podargus giganteus* and other species alive by hand in broad daylight. These birds sit perfectly still on a limb of an iron-bark or acacia, whose bark resembles their plumage in colour. W. E. ARMIT

Georgetown, Queensland, July 19

Agricultural Ants

I HAVE lately discovered a colony of agricultural ants near Georgetown. The species is very small and red.

My attention was first directed to these tiny harvesters by noticing heaps of chaff and hulls in a bare spot situated in a grove of young acacia trees.

The formicaries are entirely subterranean, being entered by a funnel-shaped tube.

Roads diverge from this gate in four or five directions, and during working hours are alive with what appear like white insects, the little ants being covered by their load. Some of these ants seem to clean the grain and carry out the husks, which form a heap round the opening to the nest. The clear space round each opening is small, certainly not more than eighteen inches in circumference, and a small mound not more than six inches in height is formed with the earth excavated in forming the nest. The only species of grain harvested is the seed of *Perotis rara*, whch is light when quite ripe. I cannot give the generic name of these little fellows, never having devoted any special study to the family, but shall be happy to furnish specimens in spirits to any naturalist who will forward his address. WILLIAM E. ARMIT

Dunrobin, Georgetown, July 19

Meteor

AT about 5.50 P.M. to-day I saw a most brilliant meteor fall quite close to the moon, which was shining brightly at the time: it was in full daylight, shortly after the sun had gone down. Its direction was nearly perpendicular, but inclined a little from north to south as it fell. It was of a bright green colour; its motion rapid, its path long, and the time during which it was visible about two seconds, and it left no visible trace behind it.

Harlton, Cambridge, October 8 O. P. FISHER

JANSSEN'S NEW METHOD OF SOLAR PHOTOGRAPHY

IN two papers published respectively in the *Comptes Rendus* for December, 1877, and in the *Annuaire* of the *Bureau des Longitudes* for the current year, M. Janssen has described a new method of photographing the solar disc, which he has successfully carried out at the Meudon Observatory, during the past twelve months; and he has also drawn attention to some striking features in the constitution of the photosphere, disclosed for the first time in the beautiful pictures which are among the first results of his process. These may be regarded as only the forerunners of further important discoveries. Through the courteous liberality of M. Janssen, I have lately had the advantage of studying the process in the Meudon Observatory, and a description of its distinctive features, and a brief notice of such of the results as M. Janssen has already published, will certainly be acceptable to many who are interested in the recent developments of solar physics, and have not ready access to the original papers.[1]

M. Janssen's pictures of the solar disc are distinguished from all those previously obtained with the photoheliograph, not only by their great size (30·5 ctm. diameter), but more especially by the remarkable sharpness and definition with which they display the details of photospheric structure, which are such that, for the purpose of their more effective study, it is found advantageous to enlarge the original pictures to three and even nine times their original linear dimensions. The greatly extended means of research which M. Janssen's invention places in the hands of the solar physicist will be obvious, when we consider the difficulties which attend any prolonged ocular inspection of the solar disc, hitherto the only practicable method of examining its detailed structure. "In spite of the interposition of coloured glasses, helioscopes, &c., the eye must seize on the details in a dazzling field, and perform its functions under conditions which are quite unfamiliar. The true variations of luminous intensity in different parts of the image can no longer be appreciated, and the impressions produced do not correspond to reality. Thus may be explained the diversity of the opinions which have been put forward respecting the forms and dimensions of the granulations, and of the constituent parts of the solar surface"—diversities well illustrated by the old controversy of the "willow-leaf" and "rice-grain" bodies in the photosphere. In M. Janssen's pictures the forms, sizes, and arrangement of the bodies described under these appellations are exhibited in the most satisfactory manner, and as, in a favourable state of the atmosphere, the pictures may be repeated at as short intervals as the operator pleases, the movements and changes of form exhibited by these bodies may be studied with the utmost ease in a register which preserves the most fugitive phases of their appearance, and is available for leisurely re-examination at any future time.

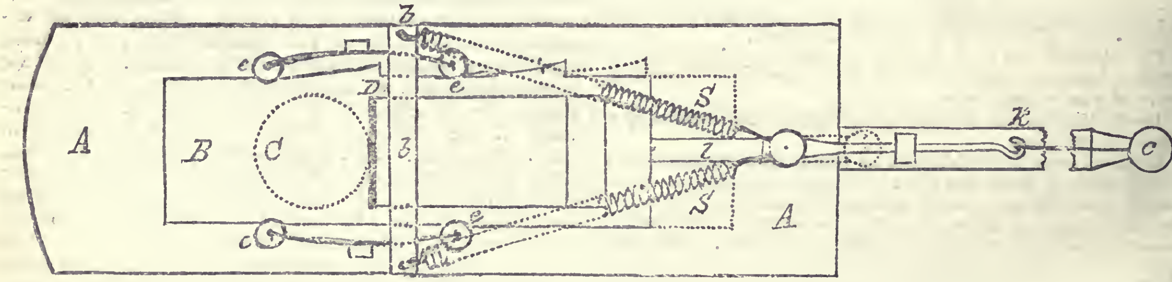
Before noticing the novel facts which M. Janssen has thus brought to light, I will briefly describe the principles of the process itself. The main difficulty to be surmounted in order to obtain a sharply defined photographic picture of the details of the solar disc is presented by the phenomenon known as photographic irradiation, in virtue of which a brilliantly illuminated surface occupies, on the negative picture, a proportionally exaggerated space; its borders being extended over the darker objects around. This phenomenon, M. Janssen remarks, "is very striking in all the photographs of total eclipses which have been obtained since 1860, which exhibit the images of the protuberances encroaching on the lunar disc, to the extent, in some cases, of ten and twenty seconds of arc, and even more." The granulations (to employ M. Janssen's terminology) visible on the solar disc have a mean diameter of not more than one second of arc, and in ordinary photoheliograms their very existence is therefore completely masked by irradiation.

[1] The present notice contains a more detailed description of the process than M. Janssen's original papers, and is published with his full approval and authority. With characteristic liberality M. Janssen writes:—"Quant au procédé lui-même, il est evident que toute personne un peu au courant de la photographie astronomique pourra l'appliquer et obtenir bientôt des photographies semblables à celles que nous obtenons. Je serai aussi très probablement devancé sur plusieurs points de la constitution du soleil que ces procédés peuvent révéler. L'inconvenient n'est que pour moi, et je crois qu'il vaut mieux dès aujourd'hui livrer la méthode au public scientifique. Les progrès de la science y gagneront certainement."

The simple and beautiful contrivance by which M. Janssen has succeeded in getting rid of irradiation, is to restrict the photographic action to one small sheaf of rays of the spectrum, viz., those which extend from the line G a short distance towards H. In a series of experimental photographs of the spectrum which M. Janssen took in my presence (with calc-spar prisms and a rock-crystal lens) and in which the time of exposure was varied successively from two-thirds of a second to three minutes, those obtained with the shortest exposure represented only that part of the spectrum immediately contiguous to G and extending a short distance towards H; and in this, the spectral lines were exhibited with extreme sharpness. With a more prolonged exposure the range of the image was greatly extended in both directions, accompanied by intensified action in the G–H region, which impaired the local definition.[1] Taking advantage of this fact, in the new process, the collodionised plate is exposed to the sun's action only so long as to allow of the action of the most actively photographic rays, and this is the cardinal condition of success. In practice, the duration of the exposure is restricted to between $\frac{1}{2000}$ and $\frac{1}{3000}$ of a second, in summer, being varied according to the season and to the time of day. The means by which this delicate adjustment is effected and verified will be described presently.

A second condition is, so to adjust the distance of the sensitised surface from the lens of the instrument, that it shall exactly coincide with the focus of the G rays. The necessity of this precaution will be readily understood when it is borne in mind that no lens is perfectly achromatic, and that, in virtue of the first condition, the rays in the vicinity of G alone produce the image. The remaining conditions are, the adoption of as large a plate as can be readily manipulated and some improvements in the process of preparing and developing the plates, whereby a very perfect surface is insured for the reception of the image, and a graduated development after exposure.[1]

The photoheliograph employed was constructed specially for the Meudon Observatory by M. Prazmouski of Paris. It has an object-glass of five inches diameter, and a reversing ocular, giving an erect image on the sensitized plate. The finding telescope casts an image on a disk of ground glass; by observing which, the operator can judge the exact instant for releasing the sliding screen, which causes the instantaneous exposure of the sensitised plate. At present, the position of the telescope is adjusted by means of winches worked by hand. The construction of the sliding screen, on the accurate adjustment and working of which the success of the operation mainly depends, will be understood on reference to the accompanying rough diagram (which, however, is not drawn to scale, and in which, to avoid confusion, some of the minor details are omitted). A A is an oblong brass plate which serves as a frame for the mechanism, and is introduced through a slit in the side of the telescope, exactly at the spot where the real image is formed by the object-glass. At the spot where the image falls, the plate is pierced with a circular aperture somewhat larger than



the image, shown by the dotted circle C. But this is completely covered by the sliding screen B, excepting such portion as is momentarily uncovered by the transverse slit D, in its passage across the image. The width of the sl t D can be varied by means of a micrometer screw, which is omitted in the diagram. The sliding screen works between four small grooved wheels *e e*, fixed to curved springs, which press them against the edges of the screen-plate; and one of these edges is shaped in the manner shown in the diagram, so that the pressure is increased from the instant at which the slit D reaches the margin of the circular aperture C, neutralising the acceleration of the movement by the continued action of the springs, and rendering it uniform during the passage of the slit across the image.[2] Thus the image is allowed to fall on the sensitised plate, not as a whole, but in successive slices, and the width of the slit is so adjusted to the rate of motion, that each slice is exposed during from $\frac{1}{2000}$ to $\frac{1}{3000}$ of a second only. The motion of the screen is effected by three spiral springs, two of which S S, are shown in the diagram (the central spring being omitted). The fixed ends are attached to a bar *b b* screwed to the slide A A; the free ends to a stud on the bar *l* which projects from the proximal end of B, is bent twice at right-angles and terminates in the hook *k*. In setting the screen before operating, a loop of twine is passed over the hook, which is then drawn towards the clip *c*, extending the springs S S, and bringing the screen into the position shown by a dotted line on the diagram. The twine is made fast in the clip *c* and all is in readiness for the operation. At the critical moment, the retaining string is cut and the slit D is rapidly drawn by the tension of the springs across the field, till checked by a stop not shown in the diagram. The movement of the screen is generally horizontal, but a gravity compensation is attached which can be employed when the movement is vertical.

The rate of the movement and the uniformity of the motion are determined by attaching, by means of wax to any part of the sliding screen, a glass slip coated with lamp-black. A tuning fork with an attached bristle, being made to vibrate transversely to the movement of the screen, the latter is released as in the actual operation of photographing, and the length of the wave marked by the bristle on the carbonised surface, multiplied by the width of the slit D, and divided by the number of vibrations of the fork per second, gives the duration of the exposure; while the uniformity of the movement is tested by the equality of the wave-lengths, which correspond to the passage of the slit D across the circular aperture C. By this means, the uniformity of exposure can be regulated to $\frac{1}{10000}$ of a second.

In order to obtain the exact position of the sun's axis on the plate, the instant of the exposure is noted by the

[1] Owing probably to spherical aberration.

[2] This, M. Janssen suggests, may be otherwise effected by an arrangement which will arrest the action of the springs at the instant when the slit reaches the margin of C.

[1] In this part of the process, M. Janssen acknowledges the assistance of M. Arents, to whom the writer is also indebted for much information.

chronometer, and the exact level of the slide containing the sensitized plate is observed with an accurate clinometer before removing it from the camera.

As M. Janssen has pointed out, the chemical preparation and development of the plate require very great care, in order to obtain the requisite sharpness of detail. The gun-cotton for the collodion is prepared at a high temperature (70° C.), and numerous precautions are taken to ensure that the collodion film shall be perfectly even, and free from the smallest speck of foreign matter. The image is developed gradually, beginning with a solution of ferrous sulphate, and after thorough washing, completing with a solution of pyrogallic acid, after which the image is strengthened with a mixture of pyrogallic and silver nitrate solutions.

In a favourable state of the atmosphere, the pictures thus obtained leave nothing to be desired in point of sharpness and definition of detail. But, as a matter of course, all states of the atmosphere do not permit of equal success, the process being subject to the same atmospheric contingencies as in all astronomical work with the telescope. The best results were obtained in the late autumn, and during this last spring.

The character of the photospheric surface as displayed in the new photographs, will be best described in a translation of M. Janssen's own words: "The photographs show that the solar surface is covered everywhere with a fine granulation. The forms, dimensions, and arrangement of the granular elements are very varied. Their size varies from some tenths of a second to three or four seconds. The shapes are circular or elliptical, and more or less elongated; but often these regular forms are more or less distorted. The granulation is exhibited everywhere; and, at first sight, it does not appear to present a different constitution towards the polar regions. But this is a point to be further investigated. The illuminating power of the granular elements, taken separately, varies much; they appear to be situated at different depths in the photospheric layer. The most luminous of them, those which more especially contribute to the luminosity of the photosphere, occupy but a small fraction of the surface of the sun.

"But the most remarkable result yet obtained, and which is exclusively due to the employment of the photographic method, is the discovery of the photospheric network (*réseau photosphérique*). An attentive examination of the photographs shows that the photosphere has not an uniform structure throughout, but is divided into a series of figures, more or less distant from one another, and exhibiting a special constitution. These figures generally have rounded contours, but also often rectilinear and sometimes polygonal. Their dimensions are very variable, and they sometimes attain to a minute or more in diameter. While, in the intervals between these figures, the grains are distinct and definitely bounded, although of very variable size; in their interior, the granules are half obliterated, drawn out and confused; most frequently, indeed, they have disappeared, giving place to trains of matter which replace the granulations. Everything indicates that, in these spaces, the photospheric substance is subject to violent movements which have confounded the granular elements. . . . This fact enlightens us as to the forms taken by solar activity, and shows that this activity is always very great in the photosphere, even though there be no spot visible on the surface. I will further draw attention to this very important fact, of which very distinct evidence is furnished by certain photographs, viz., that numerous very dark points appear in the regularly granulated tracts, indicating that the photospheric layer can have but a very small thickness."

In another paper, Mr. Janssen deduces some further conclusions of interest. He observes:—

"If the solar layer which forms the photosphere were in a state of repose and perfect equilibrium, it would result from the fact of its fluidity, that it would form a continuous envelope around the solar nucleus. The granular elements would be confounded together, and the lustre of the sun would be uniform in all its parts. But the ascending gaseous currents do not admit of this state of perfect equilibrium. They break up and divide the fluid layer, escaping at a great number of points. Hence results the formation of the granular elements, which are but so many fractions of the photospheric envelope, and which tend to take a spherical form, in virtue of the gravity of their constituent parts. . . . But even this state of equilibrium of the individual parts is but rarely realised; in numerous points, the currents drag along with them the granular elements, and these latter lose their spherical form, and eventually become no longer recognisable where the movements are most violent. . . . Moreover, in the regions of relative calm, the movements of the photospheric medium do not allow the granular elements to arrange themselves in an even layer, whence results the greater or less immersion of the grains beneath the surface, and consequently, owing to the great absorptive action of the medium, the great differences of their lustre shown in the photographic pictures. . . . We may further conclude, from the fact of the relative rarity of the most luminous grains in the photographs, that the illuminating power of the sun is due principally to that of a small number of points on his surface. In other words, if the solar surface were completely covered with granular elements of equal brilliancy with these, its illuminating power, according to a first approximate estimate, would be from ten to twenty times greater than it is." It will be interesting to ascertain, at the next epoch of sun-spot maximum, whether the brilliant granules occupy a relatively larger proportion of the solar disc than at the present time. The direct evidence which such an observation will afford on the important question of the periodic variation of solar radiative intensity, a question on which much diversity of opinion still exists, will be of the highest value.

H. F. BLANFORD

BIOLOGICAL NOTES

THE ANATOMY AND AFFINITIES OF THE AYE-AYE.—Dr. Alix having recently dissected a young male Aye-aye (*Chiromys Madagascariensis*), communicates, through Prof. Gervais, to the Academy of Sciences of Paris (*Comptes Rendus*, July 29, p. 219), some notes on certain points in its anatomy which bear upon the much-vexed question of the position of this curious animal in the mammalian series. It seems that his observations confirm in all points the opinion of all those eminent naturalists who, in accordance with De Blainville, and contrary to Gmelin and Cuvier, have held that the Aye-aye must be approximated to the lemurs and separated from the rodents, fresh facts being brought forward in support of this view. First, as regards its myology. The extensor communis hallucis, which in rodents is attached to the outer condyle of the femur, arises in the Aye-aye from the tibia. The biceps brachialis, which has only one head in the majority of rodents, has two in the Aye-aye. The supinator longus, which is generally absent in the rodents, has in the Aye-aye a good development. The common extensor of the digits, to those of the hand or foot, is composed of two distinct fascicles, of which one furnishes the tendons of the second and third digits, the other those of the fourth and fifth, from which it results that the Aye-aye, like the other Lemurina, possesses a paired digital system, and resembles in this feature the cloven-hoofed Pachyderms and the Ruminants, while the other mammals have, under all relations, an unpaired digital system. Dr. Alix has, moreover, verified the presence of a rotator muscle of the fibula, previously men-

tioned by Dr. Murie and Mr. Mivart in their paper upon the anatomy of the Aye-aye, published in the *Proceedings* of the Zoological Society. In examining the nervous system of the cervical region arrangements were discovered quite different from those seen in rodents. For example, the trunk of the great sympathetic nerve, which is otherwise separated from the pneumogastric in the whole extent of this region, has no middle cervical ganglion, but only an inferior one, excessively reduced in bulk. The superior cervical ganglion, situated immediately above the bifurcation of the common carotid, adheres by its fibrous sheath to the pneumogastric; and it is at this spot that the superior laryngeal nerve detaches itself from the pneumogastric, crossing the ganglion with which it enters into connection. On the left side there is no indication of a nervous filament answering to a depressor nerve, while on the right there may be seen to detach themselves from the superior laryngeal nerve two filaments of excessive tenuity which go to rejoin the trunk of the great sympathetic nerve. Nothing in this arrangement suggests resemblance to the nervous cord so distinct among the rodents, and above all among the Leporidæ, which, from this very circumstance, have furnished physiologists with the opportunity of making experiments of the greatest value. This character distinguishes the Aye-aye also from the opossums, which were placed by Illiger with the apes and lemurs in his order "Pollicata." The nervous arrangements, in short, confirm the results arrived at by the study of the muscles, viscera, and organs of generation, of the external form, skeleton, and dentition. J. C. G.

PROF. CARUEL'S CLASSIFICATION OF THE VEGETABLE KINGDOM.—At the close of his recent work, "La Morfologia Vegetale," Prof. Caruel of Pisa proposes a classification of the vegetable kingdom which has not so much of novelty in its principles of classification as in its terminology and the salient characters of the groups. He makes the following five primary groups, viz.:—1. PHANEROGAMIA. Every individual is trimorphic. The first form is neutral, and is capable of indefinite development, and of organic reproduction, principally by means of buds. This organic form gives rise, through the medium of the flower, to the two other (sexual) forms, male and female, which have only a definite development. The male form or pollen is thalloid; the female form or gemmule (ovule) is cormoid; this last produces, first, a pro-embryo as the result of the fecundation by the fovilla of the pollen, of an oosphere contained in a closed oogonium, and finally, the embryo of the neutral form, which develops at the extremity of the pro-embryo and in the same direction. In the subdivision of Phanerogamia, Caruel discards the distinction between Gymnospermia and Angiospermia, retaining, as the two primary classes, Monocotyledones and Dicotyledones, and giving the higher rank to the former. 2. SCHISTOGAMIA, including Characeæ only. These are also trimorphic; but the male sexual form consists of vermiform phytozoa (antherozoids) instead of pollen-grains, formed in an antherocyst (antheridium) differing in structure from the anther; the female form consists of an oogemma (archegonium) comparable to a gemmule, but naked; the neutral form springs directly from the oospore, which, on germinating, produces the embryo transversely. 3. PROTHALLOGAMIA, or Vascular Cryptogams. These are also trimorphic The neutral form does not produce the two sexual forms, but spores; these, on germinating, are transformed into sexual prothallia, with archegonia and naked oospheres, and vermiform phytozoa contained in antheridia; the oospore gives rise transversely to the embryo of the neutral form. The Prothallogamia are divided into Heterosporæ and Isosporæ. 4. BRYOGAMIA (synonymous with Muscineæ). The distinguishing character of this group is the indefinite power of development of the female individual, together with the definite development of the neutral form or sporogonium. A consequence of this is the continued and repeated fecundation of which the female form is capable, which distinguishes the Bryogamia from the three preceding groups. The embryo springs directly from the oospore; the male forms are phytozoa. The group is divided into Musci and Hepaticæ. 5. GYMNOGAMIA (Thallophyta or Cellular Cryptogams). The simplest Gymnogamia possess only a single form, which is reproduced agamically by fission, by conidia and sporidia, or by gamogenesis, but without any sexual differentiation. In others there is sexual differentiation into male and female forms; a few have also a third neutral form, when the oospore produces zoospores, instead of passing directly into the female form. They resemble the Bryogamia in the definite development of the neutral form and the indefinite development of the female form, but differ in the zoospore-like form of the phytozoa, and in the structure of the oogonium, which is isolated and naked, and does not form part of an archegonium. Prof. Caruel altogether discards the old classification of Thallophytes into Algæ, Fungi, and Lichens, but does not propose any other in its place, thinking it probable that, as our knowledge of some of its forms increases, it will be broken up into several primary groups He thinks it would be an advantage if the term Cryptogamia were altogether disused.

TRANSITION FORMS OF CRINOIDS IN AMERICAN PALÆOZOIC ROCKS.—Messrs. C. Wachsmuth and F. Springer have carefully studied the crinoids of the subcarboniferous rocks of the Mississippi valley, especially the Burlington and Keokuk limestones. There is probably no region in the world which exhibits, within the same limited geographical extent, so great and uninterrupted a range of crinoidal deposits in geological succession, almost unaltered. These observers conclude that extravagant forms and developments are not perpetuated, and that types mostly cease to exist when they reach a culmination in anatomical features. A large proportion of the genera become extinct in the formations above mentioned. The extinction of specific forms was not coincident with the close of the respective epochs of limestone deposits, but most of the changes were made by a series of slow and gradual modifications of specific characters, which correspond in a striking manner with the changes in individual life by growth. The smaller and less conspicuous forms were generally persistent, and ranged through the whole crinoidal formations with comparatively little change.

GEOGRAPHICAL NOTES

ACCORDING to present arrangements, we believe that Mr. Keith Johnston, the leader of the expedition which the Committee of the African Exploration Fund are about to despatch from the East Coast of Africa to Lake Nyassa, will leave England on November 14 for Zanzibar, together with his second in command, Mr. Thomson, whose more especial function it will be to study the geology of the country traversed. Mr. Thomson, we believe, has had an excellent training as a geologist, and it is expected that he will make important contributions to our knowledge of the geology of the region to be visited. The expedition will not actually start for the interior till next spring, and the interval will no doubt be utilised in making short journeys on the mainland, and in procuring all information possible in regard to the inhabitants, language, &c., of the region which is about to be thoroughly and scientifically explored. We sincerely trust that Mr. Johnston may not meet with the same trouble in the matter of porters as has so long retarded the progress of the Belgian and one or two other expeditions, but we do not hear that the Royal Geographical Society have formally given in their adhesion to the most recent

suggestion for facilitating African travel by the purchase of one or more Indian elephants.

THE *Moniteur Belge*, of October 12, publishes late intelligence respecting the International Association's East African Expedition, gathered from communications dated Zanzibar, September 17. The seventy-one porters engaged by M. Greffulhe were at Bigviro on August 22, and 200 others had been collected through the exertions of Père Etienne, Superior of the Mission at Bagamoyo. It was expected that they would have joined M. Wautier at Mwoméro on September 18. The *personnel* of the expedition is, therefore, now complete, and the travellers will have started by this time to rejoin M. Cambier with the baggage abandoned by the deserters. We further learn that Dr. Dutrieux wrote from Mpwapwa on August 26 to M. Greffulhe, stating that M. Cambier was pursuing his journey and was then at Kididimo, about 400 kilometres from the coast. His letter reached Zanzibar on September 4, and since then no fresh news has been received of the movements of the members of the expedition. There seems to be no truth in the rumour that the members of the Belgian expedition have been assassinated.

THE Dutch vessel *William Barends* has returned to Amsterdam from her voyage to the Arctic regions. It is stated that the voyage has been very successful from a scientific point of view, and it is expected that a full account of the discoveries made will shortly be published.

PETERMANN'S *Mittheilungen* contains an article on Turkey as it stands according to the Berlin Treaty, with a map. Dr. Emin Effendi's narrative of his journey in the African Lake Region is concluded, and an accompanying map shows the present position of the question as to the Muta Nzige, according to the data furnished by recent explorers. The south end of the Albert Nyanza lies to the north of 1° N. lat., and to the south is another Muta Nzige, of which Stanley's Beatrice Gulf is a southern extension. The Victoria Nile, between 1° and 2° N., and on the 33rd degree of E. long., broadens out into two lakelets, Gitansege and Codscha. Herr Kanitz contributes an interesting geological and physical paper on the Balkans. Perhaps the most interesting paper is one by Prof. H. Fritz on the periodical changes in the length of glaciers. The author has collected a large number of historical data to prove that there has been something like regularity in the changes of the Alpine glaciers. Important factors in seeking for the causes of change in the length of glaciers are temperature, snowfall, atmospheric moisture, clouds, direction of the wind, and atmospheric pressure. Prof. Fritz seems also of opinion that a connection may be traced between sun-spots and changes in the length of glaciers. The subject is of importance in various directions, and we trust that Prof. Fritz will continue the observations which he has so well begun.

RECENT advices, up to the beginning of September, have been received from Prof. Hayden, in the geyser regions of the Yellowstone, where the work of the survey was being prosecuted with great energy. Shoshone has been thoroughly mapped and explored, and the mammoth hot spring was next to be visited. Mr. Jackson has made large numbers of superb photographs of the geysers and other points of interest. Investigations into the temperature, composition, and other features of the hot springs are being carefully made, and a map is in progress on the scale of one inch to the mile. The survey of Point of Rocks, on the Union Pacific Railroad, along the west side of the Wind River Mountain, will next be attempted; thence he will proceed by Snake River on the east side of the Teton range to the sources of the Snake River. Dr. Hayden expected, after spending the month of September in the Park, to return along the east side of the Wind River Mountains *viâ* Camp Brown to Rawlins, on the Union Pacific Railroad, thence to Cheynene, which point it was hoped to reach on October 20.

WE have already noticed from time to time the movements of Mr. F. A. Ober, who has been engaged for two years under the auspices of the Smithsonian Institution in exploring the natural history of the West Indian Islands. His researches relate more particularly to the birds, his object being to secure the typical forms of all the islands, so that the West Indian fauna can be studied as a whole, his attention not being confined to one or two only of the islands. He has been extremely successful in his work, and has sent in numerous collections of much interest, embracing many new species, together with quite a series known previously by single specimens only. Last advices were dated Martinique, August 18. He expected to start for Guadeloupe on August 20, and to proceed thence to Marie Galant.

DR. E. TIETZE has just published in the *Jahrbuch* of the Vienna Geological Society an important paper on Mount Demavend, with a map of the mountains of the surrounding region. Demavend, according to the author, is a volcano in the solfatara condition, whose activity has waned within the memory of man. Its highest point, which even now gives out hot gases, contains a small crater, and stands inside the ruins of an older crater wall of larger diameter.

WE have received the first volume of Dr. Lenz's account of his explorations in West Africa. The second volume will appear in the beginning of next year, and Dr. Lenz hopes to publish during the present winter the geological results of his African travels, with a geological map of Guinea, and several plates of fossils.

WE have received from Mr. Stanford two new maps bearing on the present political difficulties in Asia: one is a map of "The Indian and Afghan Frontiers," the other, "A map of Western Asia." In the former the physical features are boldly drawn; here we may see how the present political boundary of India, having already passed beyond the broad and unbridged Indus, has yet stopped short of the next natural frontier, ending apparently at the foot of the mountains, but nowhere in particular. And what mountains! Commencing at the southern extremity of the map with the modest elevation of 5,390 feet, they stretch northwards at an ever-increasing altitude, until at the northern extremity we read 18,900 feet. The marking of many altitudes is a commendable feature in this map. The political colouring is somewhat startling; we did not expect to find that there is so broad an interval generally between the Indian and Afghan frontiers; they appear to be coterminous only at Thul in the Kuram Valley. The other map embraces, as its name implies, a much wider area, and shows by colour the latest extension of the Russian frontier in the direction of the mountains of Central Asia. As the British frontier has advanced to the foot-hills on the southern side of this great mountain mass, so the Russian frontier has advanced, or is rapidly advancing, to the northern foot-hills. This mass, with a breadth of about 200 miles, and an average elevation of more than 10,000 feet, with sundry passes of 12,000 and 13,000 feet, is all (!) that intervenes between the British and Russian frontiers. The new Indian frontier railways are correctly shown on these maps; with the exception of a break at Sakkur, on the Indus, and about 100 miles yet to make from Rawul Pindi to Peshawur, we have railway communication all along the landward frontiers of India, from Kurrachee to Calcutta.

NEWS from Munich states that Dr. Eugene Forel is about to start on a scientific exploring tour to New Granada.

ON THE NATURE OF VIBRATORY MOTIONS[1]

III.

Experiments by which Compound Sounds are analysed by viewing in a Rotating Mirror the Vibrations of König's Manometric Flames.

TAKE a piece of pine board, A, Fig. 15, 1 inch (25 millimetres) thick, $1\frac{1}{2}$ inch (38 millimetres) wide, and 9 inches (22·8 centimetres) long. One inch from its top bore with an inch centre-bit a shallow hole $\frac{1}{8}$ inch deep. Bore a like shallow hole in the block B, which is $\frac{3}{4}$ inch thick, $1\frac{1}{2}$ inch wide, and 2 inches (51 millimetres) long. Place a $\frac{1}{2}$-inch centre-bit in the centre of the shallow hole in A and bore with it a hole through the wood. Into this fit a glass or metal-tube, as shown at E. Bore a $\frac{3}{16}$-inch (5 millimetres) hole obliquely into the shallow hole in B, and into this fit the glass tube C. Then bore another $\frac{3}{16}$-inch hole directly into the shallow hole in B. Put a glass tube in a gas or spirit flame and heat it red-hot at a place about two inches from its end. Then draw the tube out at this place into a narrow neck. Make a cut with the edge of a file across this narrow neck, and the tube will readily snap asunder at this mark. Then heat a place on the tube in a flame, and here bend it into a right angle, as shown at D, Fig. 15. Now fit this tube into the hole just made, as shown at D. These tubes may be firmly and tightly fitted by wrapping their ends with paper coated with glue before they are forced into their holes.

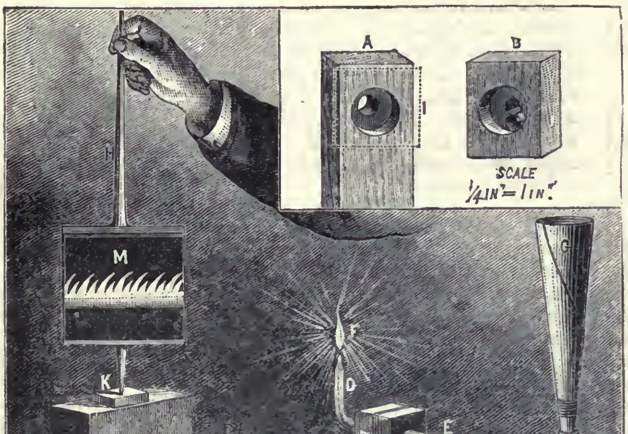


FIG. 15.

Get a small piece of the thinnest sheet rubber you can find, or a piece of thin linen paper, and, having rubbed glue on the board A around the shallow hole, stretch the thin rubber, or paper, over this hole and glue it there. Then rub glue on the block B, and place the shallow hole in this block directly over the shallow hole in A, and fasten B to A by wrapping twine around these blocks. Thus you will have made a little box divided into two compartments by a partition of thin rubber. Fasten the rod A to the side of a small board, so that it may stand upright.

Attach a piece of large-sized rubber tube to the glass tube E, and into the other end of the tube stick a cone, made by rolling up a piece of cardboard so as to form a cone 8 inches long and with a mouth 2 inches (51 millimetres) in diameter.

Now get a piece of wood 1 foot long, 4 inches wide, and $\frac{1}{4}$ inch thick. Out of this cut the square, with the two rods projecting from it, as shown at M. The lower of these rods is short, the one above the square is long. Cut the end of the shorter rod to a blunt point, and with this point make a very shallow pit in the piece of flat wood K for the rod and square to twirl in. Glue the piece of wood K on the end of a brick, L. Get two pieces of thin silvered glass, each 4 inches square, and, placing one on each side of the square M, fasten them there by winding twine around the top and bottom borders of the mirrors.

Experiment 16.—Through a rubber tube lead gas to C. It will go into the left-hand partition of the box and will come out at F, where you will light it. Now place the mirror-rod in the shallow pit in K, and hold the mirror upright so that you may see the flame F reflected from its centre.

Hold the rod upright and twirl it slowly between the thumb and forefinger, which should point downward and not horizontally, as shown in the figure. The flame appears in the mirror drawn out into a band of light with a smooth top-border. While twirling the mirror put the cone to your mouth and sing into it. The sonorous vibrations enter the side A of the box, and, striking on the thin rubber, force this in and out. When it goes in a puff of gas is driven out of the other partition, B, of the box, and the flame F jumps up. When the sheet of rubber vibrates outward it sucks the gas into the box B, and the flame F jumps down. Therefore, on singing into the funnel, you will see in the mirror the smooth top-border of the luminous band broken up into little tongues or teeth of flame, each tooth standing for one vibration of the voice on the rubber partition.

Place a lamp-chimney around the flame, should the wind from the twirling mirror agitate it, and be careful not to have the flame too high.

Experiment 17.—Another way of showing the vibrations of the flame is to burn the jet of gas at the end of a glass tube stuck into the end of a rubber tube attached to F. Now sling the tube round in a vertical circle, and you have an unbroken luminous ring; but as soon as you sing into the cone this ring breaks up into a circle of beads of light, or sometimes changes into a wreath of beautiful little luminous flowers, like forget-me-nots. To make this experiment you will be obliged to have a tube with a larger opening than that at F.

This instrument will afford you many an hour of instruction and amusement. We have only space to show you a few experiments. Others will suggest themselves whenever you use it.

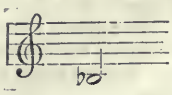
Experiment 18.—Sing into the funnel the sound of *oo* as in p*oo*l. After a few trials you will get a pure simple sound, and the flame will appear as in Fig. 16. Some voices get this figure more readily by singing E.

Experiment 19.—Twirling the mirror with the same velocity, gradually lower the pitch of the *oo* sound till your voice falls to its lower octave, when the flame will

[1] From a forthcoming work on "Sound: a Series of Simple, Entertaining, and Inexpensive Experiments in the Phenomena of Sound, for the Use of Students of every Age." By Alfred Marshall Mayer, Professor of Physics in the Stevens Institute of Technology. Communicated by the author. (Continued from p. 596.)

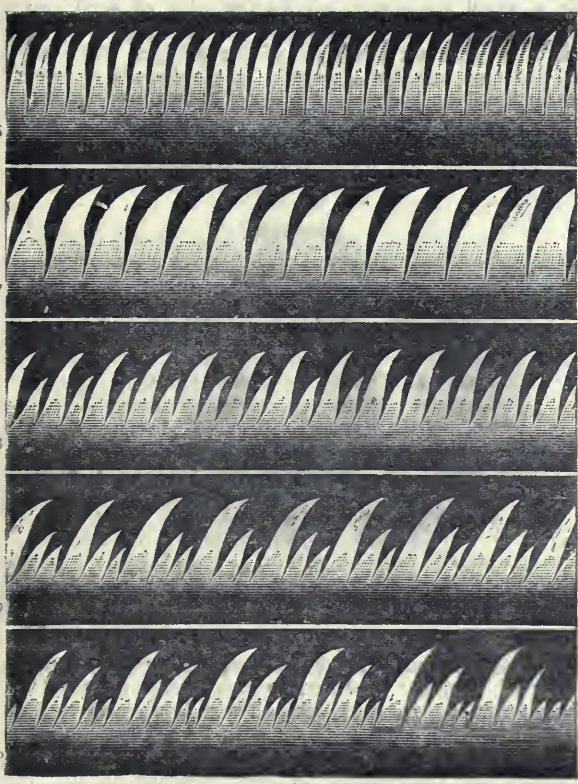
appear as in Fig. 17, with half the number of teeth in Fig. 16, because the lower octave of a sound is given by half the number of vibrations.

Experiment 20.—Sing the vowel-sound *o* on the note



and you will see Fig. 18 in the mirror. This evidently is not the figure that would have been made by a *simple* vibration. It shows that this *o* sound is compound, and formed of two simple sounds, one the octave of the other. The larger teeth are made by every alternate vibration of the higher simple sound acting with a vibration of the lower, and thus making the flame jump higher by their combined action on the membrane.

Experiment 21.—Fig. 19 appears on the mirror when we sing the English vowel *a* on the note f.



FIGS. 16, 17, 18, 19, 20.

Experiment 22.—Fig. 19 appears on the mirror when we sing the English vowel *a* on the note c.

Examine attentively Fig. 19. This shows that the English vowel *a* sung on f is made up of two combined simple vibrations. One of these alone would make the long tongues of flame, but with this simple vibration exists another of three times its frequency; that is, the vibration of greater frequency is the 3rd harmonic of the slower. As the slower vibration, making the long tongues of flame, is f, the higher must be c'' of the second octave above f. Each third vibration of this higher harmonic coincides with each vibration of f; hence each third tongue of flame is higher than the others.

Experiment 23.—In like manner the student must analyse Fig. 20 into its simple sonorous elements. Then he should, with the vibrating flame, examine the peculiarities of the various voices of his friends, and make neat and accurate drawings of the flames corresponding to them, so that he may analyse them at his leisure.

Experiment 24.—Blow your toy trumpet into the paper cone gently, and then strongly, and observe that the sound given by the trumpet is a complex one. Try if you cannot get a flame somewhat like the trumpet gives by singing *ah*, through your nose, into the cone.

The student will soon find that different persons, in singing the same note, as nearly alike as they can, will produce flames of very different forms. This is because the voices differ in the number and relative intensities of the simple sounds which form them.

Another cause of the different forms of flame obtained by different experimenters is due to the fact that they have used different lengths of tube leading from the cone to the membrane.

Experiment 25.—The fact can be readily shown by singing the same compound sound through different lengths of tube leading from the cone G to the membrane.

Terquem's Experiment, in which König's Flame is used instead of the Ear, and thus the Motions of a Vibrating Disk are made Visible.

The method of analysing the motions of a vibrating plate with the paper cone and tube applied to the ear, which has been used by us for a long time, has quite recently been adapted to M. König's flame by Prof. Terquem, of Lille, who has thus made these motions visible to the student, and has given us a charming experiment.

On how we Speak, and on the Talking Machines of Faber and Edison.—How we Speak.

The little musical instrument with which we sing and speak is formed of two flexible membranes stretched side by side across a short tubular box placed on the top of the windpipe. This box is made of plates of cartilage,

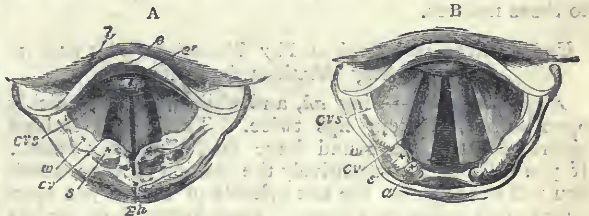


FIG. 21.

FIGS. A and B.—Views of the human larynx from above as actually seen by the aid of the instrument called the laryngoscope. Fig. A.—In the condition when voice is being produced. Fig. B.—At rest, when no voice is produced. *e*, epiglottis (foreshortened); *cv*, the vocal chords; *cvs*, the so-called false vocal chords, folds of mucous membrane lying above the real vocal chords; *a*, elevation caused by the arytenoid cartilages; *s*, *w*, elevations caused by small cartilages connected with the arytenoids; *l*, root of the tongue.

movable on each other, and bound together with muscles and membranes.

The top of the windpipe is formed of a large ring of cartilage, called the *cricoid* (ring-shaped) cartilage. Jointed to this is a broad plate of gristle, called the *thyroid* (shield-shaped) cartilage. This cartilage is bent into the shape of a V. The legs of this V straddle over the cricoid and are jointed to its outer sides. The peak of the V stands up and points toward the front of your throat. You can feel it, as it is the "Adam's apple." On the back of the upper edge of the cricoid ring are jointed two small pointed cartilages, known as the *arytenoid* (funnel-shaped) cartilages. Stretching from these to the inner sides of the legs of the V of the thyroid are two membranes, one to each leg. These are the *vocal chords.*

When the point of the thyroid is not pulled down these membranes are lax, and the breath from the windpipe passes freely between them and does not make them vibrate. (*See* B of Fig. 21.)

But when the peak of the thyroid is pulled down by

its muscles the vocal chords are stretched. At the same time the arytenoid cartilages move nearer each other, and the thin, sharply-cut edges of the vocal chords are brought parallel and quite close to each other, as is shown in A of Fig. 21. If the air is now forced through this narrow slit (called the *glottis*), the vocal chords vibrate just like the tongue in our toy trumpet, or like the reed in any reed-pipe. A puff of air passes between them; they separate; immediately afterward they come close together and the current of air is stopped. They again open, another puff goes into the cavity of the mouth, and then they close together again. Thus the glottis opens and closes with a frequency depending on the degree of stretch on the vocal chords.

Our experiments with König's flame have shown how composite are the sounds of the human voice. The quality of a voice depends on the number and relative intensities of the simple sounds which build it up.

SPEECH is voice modified and modulated by the movements of the parts of the cavity of the mouth, of the tongue, and lips.

The oral cavity is made larger or smaller, longer or shorter, and thus, resounding to some lower or higher harmonic of the voice, makes the others feebly heard.

Experiment 26.—If you form your speaking organs to say *o*, and then take your vibrating A-fork and hold it before your lips, you will hear the cavity of the mouth resounding to this sound. On changing the vocal organs to say *e* the resonance ceases.

All the vowel sounds are formed by a steady voice modified by the resonance of the different sizes and shapes given to the oral cavity.

The consonants are made by obstructions placed at the beginning or end of the oral sounds, by the movements of the tongue and lips; but, as this is a book of experiments, I leave you to inform yourself by experiments as to these matters.

Experiments in which a Toy Trumpet Talks and a Speaking Machine is Made.

Experiment 27.—Sing *ah*, and while doing so quickly open and shut your lips twice. These two sudden obstructions to the sound have made you say *mama*. If you will observe attentively the motion of your mouth you will see that for the last syllable of mama you open your mouth wider and keep it open longer than for the first syllable.

Experiment 28.—This is all we have to know to make our toy trumpet talk. You already have seen that its sounds,

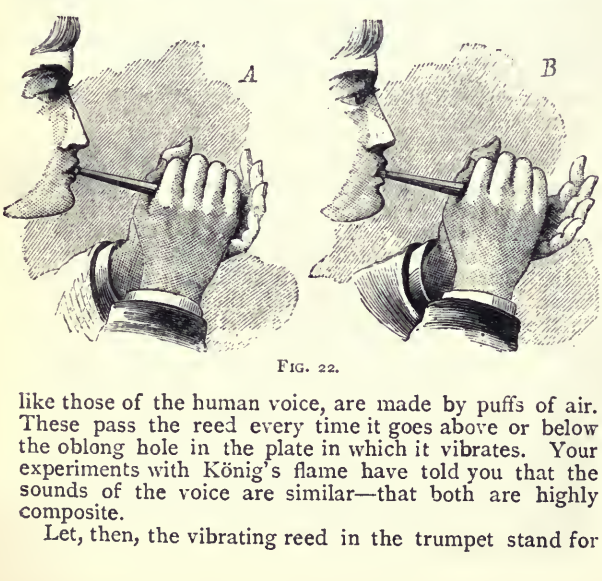


FIG. 22.

like those of the human voice, are made by puffs of air. These pass the reed every time it goes above or below the oblong hole in the plate in which it vibrates. Your experiments with König's flame have told you that the sounds of the voice are similar—that both are highly composite.

Let, then, the vibrating reed in the trumpet stand for your *vocal chords*. To get a resonant cavity like the *mouth* make one between your two hands, as shown in A of Fig. 22. The funnel of the trumpet is placed inside this cavity with the tube coming out in the crotch between the thumb and forefinger. The lips we will form of the fingers of one hand. By raising these together, more or less, from the other hand we can make a larger or smaller opening into the cavity between the palms of the hands, and thus get articulation.

Now blow into the trumpet as though you were speaking *mama* into it, so that you may make it sound twice, each sound lasting just as long as the sounds in mă and mā. While making the first sound, raise the fingers as high as is shown in A; while making the second, raise them as high as is shown in B. The trumpet talks and says *mama* quite plainly.

Experiment 28.—Let us make a talking machine. Get an orange with a thick skin and cut it in halves. With a sharp dinner-knife cut and scrape out its soft inside. You have thus made two hemispherical cups. Cut a small semicircle out of the edge of each cup. Place these over each other, and you have a hole for the tube of the trumpet to go out of the orange. Now sew the two cups together, except a length directly opposite the trumpet, for here are the *lips*. A pea-nut makes a good enough nose for a baby, and black beans make "perfectly lovely" eyes. Take the baby's cap and place it on the orange, and try if you can make it say

FIG. 23.

Faber's Talking Machine.

These simple experiments show the principles followed in the construction of the celebrated talking machine of Faber of Vienna. A vibrating ivory reed, of variable pitch, forms its vocal chords. There is an oral cavity whose size and shape can be rapidly changed by depressing the keys on a key-board. Rubber, tongue, and lips make the consonants. A little windmill turning in its throat rolls the *r*, and a tube is attached to its nose when it speaks French. This is the anatomy of this really wonderful piece of mechanism.

ELECTRIC LIGHTING

ALL the papers are still much occupied with correspondence about the electric light, the extent to which it may be brought into public use, and its possible effect upon the use of gas and upon gas companies. Many writers display much ignorance both as to the scientific and economical bearings of the matter; the most important communication on the subject that has appeared is the letter from Dr. C. W. Siemens in the *Times* of the 12th inst. Dr. Siemens writes as follows :—

The intelligence flashed through the Atlantic cable a few days since to the effect that Mr. Edison, the ingenious inventor of the phonograph, &c., had succeeded in dividing electric currents

indefinitely, for the distribution of light and power appears to have taken the public by surprise, and has exercised a most depressing influence upon the holders of gas shares. Having given close attention to the question of electric lighting ever since 1867—when, following the researches of my brother, Dr. Werner Siemens, I presented a paper to the Royal Society describing the dynamo-electric principle—I may be allowed perhaps to make a few remarks upon the novelty and probable effect of Mr. Edison's startling announcement.

In passing an electric circuit from a main conductor into several or any number of branches, the current divides itself between those branches, according to the well-known law of Ohm, in the exact inverse ratio of the electrical resistance presented by each branch. A current may thus be divided, for instance, into ten separate currents of precisely equal force, if each branch is made to consist of a wire of the same length and conductivity; but if one of these wires was again to be slit into ten wires, presenting in the aggregate the same conductivity, each of these wires would only convey 100th part of the total current. In the same way one of the minor wires might again be subdivided into branches, each of which would convey an amount of electric current which would be accurately expressed by the relative resistance of the branch in question, divided by the total resistance of all the branches put together. It would thus seem that nothing could be more easy than to divide a powerful electric current among as many branches of varying relative importance as might be desired; but in the case of electric lighting a difficulty arises in consequence of the varying resistance of each electric light or candle, due to the necessarily somewhat varying distance of the carbon points from each other, upon which the length of the luminous arc depends. In order to work a number of lights upon different branches of the same current, it is necessary to furnish each branch with a regulator so contrived that an increase of current corresponding to too near an approach of the carbon points will produce automatically an increased resistance in that branch circuit, whereas an accidental increase in the distance between the carbon points of any lamp will cause the regulator to reduce the extraneous resistance of the circuit to a *minimum*. Such a mode of regulating currents was present in my mind when, in addressing the Iron and Steel Institute in March, 1877, I ventured to express my conviction that natural forces, such as represented by large waterfalls, could be utilised for the production of motive power and electric light, in towns at a distance even of thirty miles from such source, by means of a large electric conductor. This suggestion gave rise to a good deal of discussion and criticism, especially in the United States; but I replied to some of these criticisms in delivering one of the Science Lectures at Glasgow in March last, having already referred to the matter in a discussion that was held before the Institution of Civil Engineers on January 29 last. Having in the meantime perfected the regulator, I showed it in operation at the *soirée* of the Royal Society on June 19, and have only been waiting to get experimental data complete in order to bring the whole subject before one of the scientific bodies. The arrangement may be said to consist simply of a thin strip of copper or silver, say six inches long and half an inch broad, stretched horizontally between two supports with a weight or spring exerting a certain pressure in the middle. The branch current to be regulated is passed through this strip of metal, which is thereby heated to a certain moderate extent, depending upon the amount of current passing, and upon the rate of radiation of the heat produced in the strip to surrounding objects. Suppose that when the normal condition of things obtains, the strip of metal is maintained at the temperature of, say, 100° Fahrenheit, and suppose that by an accidental approach of the carbons of a lamp the resistance of the circuit is suddenly decreased, an almost instantaneous increase of temperature of the thin strip will ensue, which will cause it to elongate slightly, and allow the weight resting in the middle to descend, which in its turn causes an increase in the resistance of a small rheostat, through which the branch current in question has to flow.

It will thus be seen that it is not so much the novelty of the announcement made by Mr. Edison as the manner in which it has been conveyed to us that has alarmed a portion of the British public, and I hold that such startling announcements as these should be deprecated, as being unworthy of science and mischievous to its true progress.

Although I am strongly of opinion that electricity will gradually replace gas in many of its most important applications as being both cheaper and more brilliant, I still hold the opinion, quoted by Mr. Northover in his letter to you of yesterday, that its application will be limited, at least during our generation, to such larger purposes as the lighting of our coasts, to naval and military signalling, to harbours, quays, warehouses, and public buildings, including perhaps picture galleries and drawing-rooms, where the objections to gas are already felt to the extent of banishing that means of lighting to the passages, offices, and bed-rooms. I am, however, of opinion that a revolution even to the extent indicated must be the work of time, and that while gas will undoubtedly in due course be supplanted by its more brilliant rival for the purposes just indicated, the consumption of gas will be maintained by the increasing area of application resulting from increase of towns, and by additional applications for cooking and for heating purposes, for which gas will supplant the use of solid fuel, and thus confer a new benefit upon mankind by doing away with the nuisance of smoke and ashes. If gas companies only rightly understood their interests they would themselves take up electric lighting for those purposes for which it has the decided preference, and at the same time promote the application of gas for heating, in doing which they would clearly increase their business as lighting companies, while benefiting the public by providing them with the very best sources of heat and light.

NOTES

AT the request of the Chemical Society Prof. Ad. Wurtz, of Paris, has accepted the office of Faraday lecturer for this year. The subject of his lecture is "La Constitution de la Matière à l'État gazeux." The lecture is to be delivered on Tuesday, November 12, in the Theatre of the Royal Institution, Albemarle Street. On the following day the Fellows of the Chemical Society propose entertaining Prof. Wurtz at a dinner to be held at Willis's Rooms.

THOSE interested in the progress of natural science at our old universities should take notice of the fact that, after considerable opposition of the "Board of Studies of the Natural Science School," the majority of that Board (chiefly by the aid of the examiners, who are London, and not Oxford, men) have carried a series of resolutions which provide that "candidates for honours in biology" may be examined in experimental physiology. The necessary encouragement to the study of this subject, viz., examination in it as an "honour subject" now existing, we may hope to see as the result some activity in the physiological laboratory of Magdalen College. Similarly we have to notice the recognition of the morphology and physiology of the vegetable kingdom as a necessary part of the study and examination of the Oxford student who is a candidate for "honours in biology." Botany was long resisted and sneered at in Oxford. External pressure has, however, reinstated botany in the Oxford school of natural science, and it rests with the examiners in future to maintain the study of this subject in the direction indicated by Sachs' admirable treatise on Botany published by the University press.

THE following changes are proposed to be made in the Council of the London Mathematical Society for the ensuing session:—Mr. C. W. Merrifield, F.R.S., to be president in the room of Lord Rayleigh, F.R.S., who is proposed for the office of vice-president in conjunction with Prof. Cayley, F.R.S.; Messrs. J. Hopkinson, F.R.S., and H. M. Taylor to be ordinary members of council in the room of Prof. Clerk Maxwell, F.R.S., and Mr. T. Cotterell. The valedictory presidential address will probably be delivered at the annual meeting (November 14).

THE new specimen of *Archæopteryx lithographicus* of Solenhofen, the discovery of which was announced some time back, has been purchased by Dr. Otto Folger, President of the Freie Deutsche Hochstift, for the sum of 35,000 marks (1,750*l.*), and

will no doubt be placed in the hands of some competent German palæontologist for description. It is said to be in several respects more perfect than the first, and hitherto unique, specimen in the British Museum, which has been the subject of the labours of Prof. Owen and Prof. Huxley on this most remarkable of extinct birds.

THE death is announced of M. Leymarie, Professor of Geology at Toulouse, and the author of the first geological map of France.

THE death, at the age of seventy, of Prof. William Monroe Davis, is announced as having taken place at Cleveland, Ohio, U.S., on July 21 last. When quite advanced in life Prof. Davis took up the study of astronomy, and has long been known as an original thinker and labourer in this field. After the resignation by Prof. Mitchell, as director of the Cincinnati Observatory, he was succeeded by Prof. Davis, who remained in charge for some considerable time. Of late years he has not been actively employed in any work, with the exception of the construction of a telescope, which has done excellent service in his hands.

THE Society of Arts announce that their opening meeting will be held on November 20, when the chairman's inaugural address will be delivered and the following medals presented :—The Albert Medal (gold), for "Distinguished Merit in Promoting Arts, Manufactures, or Commerce," to Sir William G. Armstrong, C.B., F.R.S., D.C.L. The council also announce that the following papers will be read :—On November 27, "The Land of Midian," by Capt. Burton; December 4, "The Electric Light," fully illustrated with experiments, by T. N. Shoolbred; December 11, "The Route to India, with especial Reference to the Euphrates Valley Railway," by Hyde Clarke; December 18, "Science Teaching in Elementary Schools," by Dr. Gladstone, F.R.S.; and the first course (six in number) of lectures by W. M. Williams on "The Manufacture of Mathematical Instruments."

A CORRESPONDENT makes the interesting suggestion that the microphone might be used to detect if insects have any audible means of communicating with each other, and if so, what is its nature in different classes of insects.

THE fourth annual conference of the Cryptogamic Society of Scotland was held in the Royal Botanic Garden, Edinburgh, on the 9th, 10th, and 11th inst., under the presidency of Prof. Balfour, and was a success in every way. The business meeting was held in the lecture-hall, and in addition to the president's address a number of papers were read relating to recent discoveries—both in species and in morphology—in cryptogamic botany. An excursion was made to the Penicuick woods, where about 170 species of fungi, including one or two new and several rare species, were noted. The public show was held in the winter garden and herbarium of the Royal Botanic Garden, and was visited by a great many people, who appeared to be much interested in the exhibition. A considerable number of hymenomycetes, &c., were arranged in classified order and named. Many distinguished botanists, both English and Scottish, attended the meeting, and were very hospitably entertained by the president and other members of the local committee. A notice of the scientific results of the conference will be given in the *Scottish Naturalist*. Next year's conference is to take place at Forres. Arrangements may be learnt from the secretary of the Society, Dr. Buchanan White, Perth.

MR. T. MUIR, M.A., of the High School, Glasgow, has forwarded to the London Mathematical Society a verification of Pervouchine's first result regarding the divisibility of $2^{2^{12}} + 1$ by $7 \cdot 2^{14} + 1$ (NATURE, vol. xviii. p. 104). The mode of verification will be understood from the following question and solution :—Is $11 \cdot 2^4 + 1$ a factor of any number of the form $2^m + 1$? $11 \cdot 2^4 + 1 = 2^7 + 2^6 + 2^4 + 1 = 10110001$ (radix 2). The question thus is—"Is there any number which when multiplied by 10110001 will produce a number of the form 1000...0001? Now knowing that the last digit of the multiplier and product is 1, we infer that the last digit of the multiplicand must be 1. Taking it as such and performing the multiplication we have—

```
    .......1
    10110001
    --------
       .....1
     ....1000
    ....1
   ....0
  ....1
```

whence, in order that the result of the addition may be of the form ....0001, we see that the second, third, fourth, and fifth digits of the multiplicand must be 0, 0, 0, 1 respectively. Prefixing these to the first digit and continuing the multiplication we have—

```
      ....10001
       10110001
    -----------
      ....10001
   ....10001
  ....10001
 ..10001
 --------------
```

from which on addition we deduce other four digits of the multiplicand; and so on. When we have got in all twenty-two digits the figuring is as follows :—

```
       1011100100100001010001
                     10110001
    --------------------------
       1011100100100001010001
   1011100100100001010001
  1011100100100001010001
1011100100100001010001
------------------------------
```

and we find that addition then gives a product of the required form; and thus we have the result—

$$2^{29} + 1 \text{ is divisible by } 11 \cdot 2^4 + 1.$$

When there are few significant figures in the multiplier, as here, it will readily be seen that a very considerable lessening of labour is possible, that, in fact, the digits of the multiplicand can be written down at a steady pace without any auxiliary figuring at all. This is what was actually done in Mr. Muir's verification of Pervouchine's first case. With reference to the editorial query as to how the trial divisors came to be thought of Mr. Muir refers to the *Turin Transactions* for the present year, where there is a paper by M. E. Lucas, which almost entitles him to the merit of being a sharer in the discovery.

THE excavations made in Carniola under the direction of Herr von Hochstätter, on spots of palæontological and prehistoric interest, have hitherto been crowned with every success. The Kreuzberg Cave, near Laas, in the district of Zirknitz, proved to be an exceedingly interesting bear cave. The investigations made in this district, at St. Michael, near Adelsberg, and at Klenke, near Waatsch, have furnished undeniable proofs of the existence of prehistoric colonies and burial grounds at these places. Another interesting discovery has just been made at the Laibacher Moor, the well-known pile-dwelling ground. A peat digger found six silver coins of the size of a florin, which all bear the inscription of the Roman Emperor, Augustus Claudius. The discovery has been secured for scientific purposes.

THE Paris Academy of Arts has recently acquired an Egyptian papyrus which is particularly remarkable on account of its reputed age, which is estimated at over 4,000 years. It is perfectly preserved; its height is 8·30 metres, and its width 43 centimetres.

It contains a description of the death and the burial celebration of the mother of King Herod, from the first dynasty of Egyptian kings. The price paid by the Academy was 4,000 francs (160*l.*); the papyrus is now in the Exhibition.

IN the August number of the *Moniteur Scientifique* M. J. Laurent, of Marseilles, cautions the scientific world generally, and chemists in particular, against the use of de la Bastie's toughened glass. He considers the objects and utensils made of this substance to be no better than so many Prince Rupert's drops or Bologna flasks, from which they differ only by their shape. M. Laurent adduces an instance where a dish made of toughened glass was used at a stearine factory at Marseilles; it suddenly broke into thousands of fragments upon being placed on the metal scale of a balance. It was then in a state of cooling down from 110° C., at which temperature it had been maintained for some time; but it had previously been in use for about a month, and its sudden destruction was entirely inexplicable, save by the theory above mentioned.

WITH reference to Vesuvius the *Libertà* publishes the following letter from Prof. Palmieri, dated October 6:—"The phase of minor activity of the crater continues, nor is there sign of any speedy increase. Little smoke, very little lava, and a certain lesser activity in the eruptions of the new cone represent the phases of decreasing dynamism. According to some, I have announced augmenting force with the growth of the moon. I must state that I only said that if there was to be any increase, it would be verified towards the time of the full moon, according to the laws I have noted since Vesuvius has been my study, and confirmed by irrefutable documents from which I have drawn the history of our volcano. But, however it may be, this eruptive period, long foreseen, appears to require time to reach the evolution of the major phases it will ultimately attain." Telegrams from Naples on the 13th announced greatly increased activity in the volcanic action of Vesuvius.

MESSRS. MACMILLAN AND CO. have in preparation a textbook, systematic and practical, on the "Physiological Chemistry of Animal Bodies, and on the Changes which their Tissues and Fluids undergo in Disease," by Prof. Arthur Gamgee, M.D., F.R.S. The author seeks to fill up an important gap at present existing in English medical and scientific literature by preparing a succinct, though complete, account of the chemical processes of the organism, and of the methods of studying them. The work will primarily be a didactic and systematic treatise, and, though in no respect a servile imitation, will be constructedon the same plan as Prof. Kühne's most admirable, though now necessarily almost obsolete, "Lehrbuch der physiologischen Chemie:" Leipzig, 1866. It will differ, however, even in plan from that book, in containing elaborate descriptions of methods of research and directions for the performance of analyses, which will in part be introduced into the systematic portion of the text, and in part be added as appendices to each section. These appendices will be so detailed and complete as to render superfluous a separate laboratory treatise on Chemico-Physiological Analysis, such as the excellent books of Hoppe-Seyler, and Gorup-Besancy. It is the object of the author to prepare a work which will not only be useful to specialists in physiology, but to physicians, by whose researches the most important facts in the chemical history of the body have been discovered in the past, as they doubtless will be in the future.

FOUR shocks of earthquake were felt at Mineo, in Sicily, early on the morning of the 5th.

THE severe thunderstorms of October 7, 8, 9, 10, which raged in several parts of France with an almost unprecedented fury, were preceded by strong siroccos in Algeria, where the heat had been quite oppressive. The thunderstorms advanced in France from the south northwards, and even in the British Channel strong south gales were felt. The perturbation lasted during five days, when the magnificent weather which had marked the beginning of October returned.

THE large balloon, the *Crusader*, which escaped from the Royal Arsenal, Woolwich, on Monday afternoon last, descended in the Port Meadow, near Oxford, at 7 o'clock the same evening.

AN interesting account of the annual fungus foray of the Woolhope Club will be found in the *Gardeners' Chronicle* of October 12.

THE *Courrier de Bone* says that a singular phenemenon was observed at Clousel, in the vicinity of Hammam Mex Kontine, one of the most celebrated thermal bathing-places in Algiers. After an earthquake which took place in the beginning of September, an immense rock was precipitated from the mountain. Some inhabitants visiting the place found the opening of a grotto at the bottom of which a lake was discovered. The water is quite fresh and almost at zero Centigrade.

ACTIVE preparations are being made for the meeting of the Social Science Congress at Cheltenham on the 23rd instant.

CAPTAIN PATTERSON, Superintendent of the U.S. Coast Survey, has lately initiated a very important undertaking in connection with the work of the Survey, namely, in determining the extent and position of the oyster beds of the Chesapeake Bay, primarily with reference to the formation of oyster reefs, and their interference with navigation, but broad enough in its scope to serve as the basis of a critical investigation of the whole subject of the oyster fisheries and oyster culture in the United States. It is somewhat curious that the best article upon the statistics and distribution of the oyster in America is from the pen of Capt. Broca, a French officer sent over some years ago by his government to investigate this subject. The work is being prosecuted in the Chesapeake Bay by the Coast Survey steamer *Palinurus*, Mr. H. J. Rice, formerly of Johns Hopkins University, looking more particularly after the natural history features, such as the embryology and development of the oyster, &c. After the survey and investigation have been completed in Chesapeake Bay, the exploration will be extended to other points on the coast. For the better purpose of furnishing the required data for a critical investigation of the subject, the party, in addition to determining the depth of water in which the beds are situated, will secure samples of the water itself, with specimens of the oyster, and the temperature and currents will be observed, the whole work being conducted in accordance with the best principles of modern research.

M. THIERS' long-talked-of work on philosophy will soon be published; it is in the hands of copyists, who will finish their work this week. The work will include scientific subjects. It will be published in three volumes, but the first is the only one which has been completed. The last two volumes have not been revised by the author. M. Thiers began the work in 1864, after having received lessons in astronomy from Leverrier and in chemistry from Sainte Claire Deville. It was interrupted from 1870 to May 24, 1874, when Thiers was obliged to resign the presidentship of the French republic. But it underwent some interruption from January, 1877, when the illustrious author was appointed by the Chamber of Deputies President of the Commission for the Reorganisation of the Army. At St. Germain, when he suddenly died, he was busy re-writing his second volume.

MR. HORMUZD RASSAM, we learn from the *Times*, who returned to England in July last, bringing with him a rich collection of Assyrian antiquities, the result of his last expedition to explore the ruins of Ancient Nineveh, is about to start upon a second and much extended tour of exploration. The expeditions of the late George Smith and other explorers

during the last few years have been greatly impeded by the restricted nature of the firmans granted, and constant disputes were arising as to the area over which the firman extended. Mr. Rassam has succeeded in obtaining a series of sufficiently open permits to enable the new expedition to assume the nature of a roving exploring party. The new firman includes the whole of Mesopotamia, embracing the region around Mosul—that is, the sites of Nineveh, Kalakh, and the ancient city of Assur, the site of which is marked by the mounds of Kileh Shergat. A special firman has been obtained to enable Mr. Rassam to commence explorations in a hitherto untouched field—the districts of North-Eastern Syria. This region, the country which once formed the seat of the powerful Hittite kingdom, having for its capital the city of Carchemish, is as yet a *terra incognita* to explorers, and as its annals when discovered will form an important link in the chain of history which binds Assyria to the West, great results may be expected from Mr. Rassam's explorations in this district.

WE have on our table the following books:—"Pleasant Ways in Science," R. A. Proctor (Chatto and Windus); "Ancient History from the Monuments of Sinai," S. H. Palmer (S.P.C.K.); "Crystallography," H. P. Gurney (S.P.C.K.); "Bluthendiagramma," 1st and 2nd Parts, Dr. A. W. Eichler (Engelmann); "Studies from the Physiological Laboratory of University of Cambridge" (University Press).—London Science Class-Books (Longmans):—"Botany, Morphology, and Physiology," W. R. McNab; "Botany: Classification of Plants," W. R. McNab; "Hydrostatics and Pneumatics," P. Magnus; "Invertebrata and Vertebrata," Prof. Macalister; "Motion of the Moon," Dr. S. Newcomb (Washington); "Physical, Geological, and Geographical Map of Great Britain," Prof. Ramsay (Standford); "Meteorology of the Bombay Presidency," Charles Chambers; "Karl Ernst von Baer," Dr. Stieda; "Karl Friedrich Gauss, Hauptmann (E. J. Brill); "Report on Iron and Steel as Manufactured by Messrs. Jones and Laughlins," R. H. Thurston; "On the Equilibrium of Heterogeneous Substances," Parts 1 and 2, J. W. Gibbs; "Skizzen aus West Afrika," Oskar Lenz (A. Hofmann); "Les Produits de la Nature," A. J. C. Geertz (C. Lèvy); "La Prévision du Temps," W. de Fonvielle.

THE additions to the Zoological Society's Gardens during the past week include a Cross Fox (*Canis fulvus*) from Colorado, presented by Mr. Wilfred G. Marshall; a Common Otter (*Lutra vulgaris*), European, presented by Mr. W. H. Baylis; a Brown Mynah (*Acridotheres fuscus*), a Pied Mynah (*Sternopastor contra*) from India, an Indranee Owl (*Syrnium indranee*) from Ceylon, presented by Capt. J. Murray; four Egyptian Geese (*Chenalopex ægyptiaca*) from the Cape of Good Hope, presented by Mr. Justice Denyssen; two Leopard Tortoises (*Testudo pardalis*) from the Cape of Good Hope, presented by the Rev. G. H. R. Fisk, C.M.Z.S.; a Collared Peccary (*Dicotyles tajaçu*) from South America, deposited; a Red-Sided Eclectus (*Eclectus polychlorus*) from Malacca, a Black-Footed Penguin (*Spheniscus demersus*) from the Cape of Good Hope, four Chinese Turtle-Doves (*Turtur chinensis*) from China, purchased; a Hybrid Mandrill Monkey (between *C. mormon* ♀ and *M. cynomolgus* ♂), an Indian Muntjac (*Cervulus muntjac*), born in the Gardens.

ON THE PRESENCE OF DARK LINES IN THE SOLAR SPECTRUM WHICH CORRESPOND CLOSELY TO THE LINES OF THE SPECTRUM OF OXYGEN[1]

THE measurement of the wave-lengths of the dark lines of the solar spectrum obtained by photographs, and the construction of a chart of the same, has for many years occupied my leisure time. As a result of the investigations connected with this work, I have arrived at the belief that oxygen as well as other non-metallic gaseous elements are represented in the solar spectrum by dark lines in the same manner as metallic substances. The lines in the case of oxygen are, however, very faint when compared with those produced by metals in the vaporous state.

The apparatus employed in these investigations may be briefly described as follows: 1st, a spectroscope for photographing the normal solar spectrum. As my purpose was to obtain photographs in which the positions of the lines should be as true as possible, I resorted entirely to the process by reflection, *and at no time did the solar rays pass through glass*; all error that might arise during refraction was thus avoided. The mirrors of the heliostat were of flat glass silvered, the silver-surface being polished served as the reflector. The surface of the concave-mirror employed to bring the image of the slit to a focus, was also silvered and polished. Gratings of 4,800 and 9,600 lines to the English inch, ruled on glass by a machine constructed by myself and by my assistant Mr. Sickels, and also an admirable one of 17,280 lines to the inch, for which I am indebted to Mr. Rutherfurd, were used. These were silvered with a thin coating, and the unpolished silver surface employed to give spectra by reflection. With the 4,800 line gratings the photographs were in the 1st and 3rd orders; with these of 9,600 lines in the 3rd order, and with 17,280 in the 1st and 2nd orders. The accuracy of the gratings was tested with satisfactory results by taking photographs in equivalent orders of spectra on each side of the normal. The photographs for the determination of the wave-lengths of the solar spectrum were in sections of eighty to one hundred and fifty wave-lengths. The gratings were adjusted to the line of no deviation for the centre of each section of the spectrum, as it was photographed.

The wave-lengths of the lines of the spectrum were carefully measured on the original photographs, by projecting them upon a scale of wave-lengths, each wave-length being five millimetres in extent. The scale was drawn upon slips of paper, which had been glued to strips of well-seasoned pine wood cut with the grain. The lantern used for projection was that described in this journal for April, 1878. The distance of the lantern from the scale, and the consequent magnifying power, was so adjusted as to make the leading lines of the photograph coincide with the same lines of Ångström, drawn in their proper position below the scale as is shown in the diagram given later on. Thus the positions of the lines in each section of one hundred or more wave-lengths were all made visible at once, and the errors in Ångström's chart corrected. From 4045 to O in the ultra-violet the leading lines of Cornu were employed. Among the advantages presented by this method of studying and measuring the lines of the spectrum we may mention the opportunity offered for several persons to inspect at the same time the details of the section under examination, and submit them to intelligent discussion. To this we may add the facilities offered for comparing many photographs with each other by marking below the scale the peculiarities of one, and then projecting the others in order upon the marks made. In this way the effects of duration of exposure and manner of development of the image, together with the variation in the size of the slit and focal distance may be investigated, and their action on the details of the picture determined. Pictures may even be placed face to face, one a little above the other, and examined in that position by projection. From the measures thus obtained a chart of the spectrum was constructed, which extended from E in the green to P in the ultra-violet. The values assigned to the wave-lengths in this chart are those of Ångström, and it is my purpose to present the positions and characters of certain of these lines in this communication.

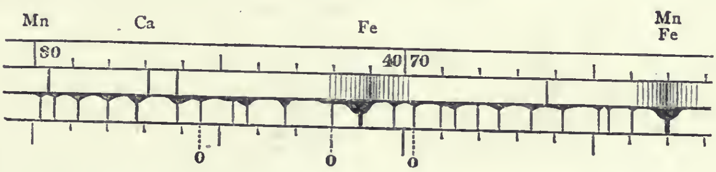
The great increase in the number of lines in the chart made from photographs by Mr. Rutherfurd's grating, compared with that of Ångström, led me to collect all the measurements of spectrum lines of elements that I could find, for the purpose of determining the character of the newly-measured lines. On comparing the lines of the spectra of oxygen, nitrogen, and air, as given in Watt's index of spectra, from the researches of Thalén, Huggins, and Plücker, I was struck with the number of approximate coincidences between the wave-lengths of oxygen lines and those of dark lines in my map. Attempting to make a close comparison of the oxygen with the solar lines I was confronted by the following difficulties, viz.: the measurements

[1] By John Christopher Draper, M.D., LL.D., Professor of Natural History in the College of the City of New York. From the *American Journal* for October.

of Thalén, Huggins, and Plücker were given in wave-lengths only, fractions being omitted altogether. Error amounting to half a wave-length could therefore exist in the position of a line, according as it fell on one side or the other of a figure on the scale expressing a wave-length. In the values given to the air-lines by Ångström in his chart, this difficulty did not exist; I therefore attempted the use of Ångström's values, employing the work of Huggins and Plücker, to separate as far as possible the oxygen from the nitrogen lines. This operation was, however, quickly discarded; because of the great differences existing between these authorities regarding the wave-lengths of a number of oxygen and air-lines. To obviate this trouble, I made photographic measurements of the lines of the electric spectrum of oxygen by the method detailed below.

The apparatus employed consisted of a spectroscope with two flint glass prisms of 60°, adjusted to the minimum deviation of D′. Collimator and telescope objectives, achromatics of ten inches focus. This was used to make photographs of the spectra given by the condensed electric spark in oxygen, in air, and in nitrogen. When so employed the eye-piece of the telescope was removed, and a camera placed in its stead. The slit was sometimes made as narrow as was possible. The induction-coil was one of Ritchie's, giving a ten-inch spark, and having a hammer current-breaker driven by clock-work. The battery was three two-gallon bichromate cells, the elements were large, but just touched the fluid when the battery was in operation. The large mass of fluid in proportion to the immersed area of the elements served to supply a very uniform primary current. The condenser on the secondary current consisted of ten glass plates, each having a tin-foil coating of thirty-six square inches. One or more of these plates could be thrown into the circuit as occasion required. By this arrangement a number of photographs of the electric spark, between platinum and iron points, in atmospheres of oxygen, and of oxygen and nitrogen, were made. The positions of the iron and oxygen lines in these were measured, as in the solar photographs, by projection under a suitable magnifying power. The centre of each line was the portion from which measures were taken in all cases. The wave-lengths of the oxygen-lines were then determined by means of a curve, which from λ3864 to λ4414·75 was based on the iron lines of the same spectrum. In all forty-seven iron lines in this extent of the spectrum, or about one to every eleven wave-lengths were used. The values assigned to the iron lines were those obtained in my chart of the solar spectrum. By this method of measurement errors arising from maladjustment of two spectra were avoided. From λ4414·75 to λ4705 the iron lines did not photograph; I was therefore obliged to construct this portion of the curve from the wave-lengths of oxygen and air-lines already given by various authorities, selecting those values in which they agreed. From 3864 to 4414·75 the results are therefore accurate. From 4414·75 to 4705, though they are approximate, the error, if any exists, must be very small. The measurements to fractions of a wave-length were obtained by constructing the curve on a scale of sufficient magnitude.

In illustration of the great number of lines presented by my chart of the solar spectrum, as compared with that of Ångström, I give a small section extending from 4062 to 4080, within which the oxygen group 4069·80—4072·10—and 4075·50 falls. On the upper part of the diagram the symbols of the elements are placed, to which, according to Ångström, corresponding lines are found in the solar spectrum. On the first space below the line is the scale of wave-lengths, each wave-length being five millimetres in extent. In the second space the lines of Ångström's chart are given. In the third space the lines measured on the photographs, the vertical portion of each symbol giving the position, and the horizontal portion the width, and also the darkness on an arbitrary scale of one to ten. The darkest lines encroach most on the vertical portion of the symbol. The value 10 is expressed in the symbol of the Mn Fe line 4063, and the value 1 in that of the line 4068·05. Other features of the lines are shown by the manner in which the upper part of each of these symbols is drawn. Beneath the spectrum-lines the scale is repeated, and the position of the



oxygen lines indicated. In addition to the lines already mentioned as being in Ångström's chart, lines of the following elements appear in this space, viz., Fe 4063, Pb 4066, Sr 4078, Bi 4080. The correspondence between these values and the wave-lengths of the lines in the photographic spectrum is as close as could be expected, seeing that the authorities do not give fractions of wave-lengths. The Te line is represented in the spectrum by the Mn Fe line 4063, the Pb line by the spectrum line 4065·7, the Sr line by the line 4077·9, and the Bi line by the line 4079·8.

Inspection of the diagram also shows that, while the Mn Fe 4063 lines are coincident in both charts, the Fe line 4071 of Ångström reads 4071·25 in the photographic chart, and the unassigned line 4076·25 of Ångström reads about 4076·20, in my chart, the two lines being nearly coincident. In the photographic chart the relations of the lines to each other as regards position are accurately presented, and where these differences occur the positions given in the photographic chart must be correct. The total number of lines in the two charts is also worthy of notice. In the eighteen wave-lengths represented in the diagram Ångström gives six lines, while the photographic chart gives twenty-four. Of Ångström's lines five are assigned to different metals, if we give the line 4066·25 to Pb, and one is unassigned. In the photographic chart these lines also appear, and in addition the lines of Bi and Sr, together with the three oxygen lines. Out of the twenty-four lines ten are assigned to various elements, leaving fourteen to be accounted for, and many of these are moderately strong lines. The oxygen lines represented in the diagram are among the strongest in the electric spectrum of oxygen, yet the equivalent lines in the solar spectrum are faint when compared with the lines of Ca and Fe. This would seem to indicate a low absorbing power in the gaseous non-metallic elements, as compared with the same power in the case of metals in the vapourous state. The existence of a difference like this would explain why it is that many of the lines in the solar spectrum which represent oxygen have been overlooked. Some of these lines have, however, been observed, Ångström himself giving in his chart a number of lines not assigned by him nor any one else to other elements, which are very nearly coincident with the oxygen lines, as determined by the photographic method, as will be seen in the table at the close of this article.

As it is not possible, in the space to which we are limited, to give diagrams of all the portions of the solar spectrum which contain oxygen lines, we present in the following table the positions in that spectrum of all the oxygen lines that were obtained in the photographs of the electric spark in that gas. The first column contains the wave-lengths of certain lines in the chart made from photographs of the solar spectrum; the second the wave-lengths of the lines of the condensed electric spark in oxygen; the third Plücker's lines of oxygen, which are much more numerous than those of Huggins, which are presented in the fourth column, while the fifth column gives the lines of Ångström's air spectrum, which may be credited to oxygen. The term free in the first column is used to indicate the fact that no element has heretofore been found to give a line within two or three tenths of a wave-length of that position. It is therefore free to be assigned as an oxygen line. The chemical symbols on the other lines show that the element indicated has been assigned to that line, and shares it with oxygen. The number of lines of greater wave-length than 3961·60, which are free from other elements, and which are assignable to oxygen, is good evidence of its presence in the solar envelopes.

DRAPER. Lines of photographic chart of solar spectrum, with their condition.	DRAPER. Lines of electric spark in oxygen.	PLÜCKER. Lines of oxygen.	HUGGINS. Lines of oxygen.	ÅNGSTRÖM. Lines of spark in air attributed to oxygen.
3864·50^{2} free.	3864·75^{1}			
3882·30^{9} ,,	3882·30^{3}			
3907·90^{2} ,,	3908·00^{1}			
3912·25^{3} ,,	3912·35^{3}			
3919·75^{3} ,,	3919·50^{2}			
3945·10^{1} ,,	3945·10^{3}			
3954·60^{3} ,,	3954·70^{7}			
3961·60^{4} ,,	3961·60^{2}			
3973·40^{3} ,,	3973·50^{10}			
3982·75^{1} ,,	3982·70^{3}			
3995·50^{3} ,,	3995·50^{5}			
4069·80^{2} ,,	4069·50^{10}	4069·00^{3}	4069·00^{3}	4069·50
4072·10^{3} ,,	4071·90^{10}	4072·00^{3}	4073·00^{3}	{4071·65 4073·65
4075·50^{2} ,,	4075·45^{10}			4075·50
4084·70^{4} ,,	4084·80^{5}	4085·00^{4}		
4088·00^{1} ,,	4087·80^{4}	4086·00^{2}		
4093·20^{1} ,,	4093·10^{4}	4094·00^{2}		
4104·40^{2} ,,	4104·50^{6}	4104·00^{2}		4103·00
4111·00^{2} ,,	4111·10^{4}			
4118·00^{7} Fe.	4118·20^{10}	4117·00^{2}	4117·00^{2}	
4121·20^{3} ,,	4121·20^{6}			
4133·00^{3} free.	4132·90^{6}	4126·00^{6}		
4142·90^{7} Fe.	4142·90^{6}	4135·00^{6}		
4145·30^{2} free.	4145·50^{7}	4147·00^{2}	4149·00^{2}	
4152·90^{1} ,,	4153·00^{8}			
4155·60^{1} ,,	4155·75^{4}	4158·00^{4}		4155·00
4168·20^{1} S.	4168·40^{4}	4171·00^{2}		
4184·90^{1} free.	4185·00^{8}		4183·00^{5}	4184·50
4189·90^{1} C.	4190·00^{10}	4190·00^{5}		4189·60
4254·30^{1} free.	4254·50^{3}			
4274·80^{4} CrCa.	4275·00^{6}			
4278·10^{3} Pb.	4278·10^{6}			
4303·00^{5} free.	4303·00^{4}			
4316·60^{2} ,,	4316·50^{8}			
4320·00^{4} TiC.	4319·75^{8}			
4325·10^{10} Fe.	4325·20^{6}	4327·00^{2}		
4328·10^{1} Bi.	4328·20^{4}			
4331·00^{2} free.	4331·20^{4}			
4336·34^{4} SCr.	4336·00^{6}	4334·00^{2}		
4345·15^{2} free.	4345·20^{9}	4341·00^{6}		4345·80
4348·20^{2} ,,	4348·30^{10}	{4347·00^{10} 4348·00^{10}	4347·00	4347·50
4353·00^{2} ,,	4553·10^{8}			
4365·00^{1} BrCe.	4365·20^{8}		4364·00^{4}	
4369·10^{4} CrFeAs.	4369·20^{4}	4369·00^{4}		
4394·50^{3} free.	4394·50^{4}			
4413·20^{2} ,,	4413·20^{10}	4414·00^{8}	4414·00^{8}	4414·60
4417·85^{3} ,,	4418·00^{10}	4418·00^{8}	4416·00^{8}	4418·30
4445·00^{2} ,,	4445·00^{6}	4443·00^{4}		
4450·00^{2} Mn.	4450·00^{3}	4450·00^{4}		
4463·00^{2} Ce.	4463·00^{8}	4457·00^{4}		
4467·30^{1} Ce?	4467·20^{8}	4468·00^{10}	4467·00^{10}	
4483·80^{1} Fe.	4483·75^{3}	4474·00^{10}		
4572·10^{3} Be.	4572·20^{1}			
4577·75^{6} Ce.	4577·55^{1}			
4582·10^{2} FeCe.	4582·10^{1}			
4589·30^{4} free.	4589·50^{10}		4588·00^{6}	4590·80
4595·40^{3} ,,	4595·50^{10}	4593·00^{6}	4596·00^{6}	4595·90
4599·80^{3} 4600·15^{4} } Sb. C.P. Cr.	4600·00^{3}	4600·00^{6}		
4629·60^{3} free.	4629·60^{4}	4639·00^{10}		
4640·50^{3} ,,	4640·20^{10}	4640·00^{10}	4640·00^{6}	4640·25
4648·15^{4} Cr.	4648·15^{10}	4649·00^{8}	4648·00^{8}	
4661·50^{4} free.	4661·50^{8}	4662·00^{7}	4662·00^{7}	
4674·90^{1} CSe?	4675·00^{8}	4675·00^{7}	4677·00^{7}	4674·75
4698·65^{3} free.	4698·50^{10}	4698·00^{7}	4699·00^{7}	4698·00
4704·65^{1} Ba.	4705·00^{10}	4705·00^{7}	4706·00^{7}	4706·50

The table presents what may be called the oxygen region of the spectrum, only a few oxygen lines lying outside of its limits. As this also happens to be the region in which our photographic apparatus and chemicals were most sensitive, we are enabled to present measurements of the majority of the lines of oxygen. It will be noticed that though the oxygen lines of greater wave-length than 4704·65 are wanting, on account of their lack of photographic power, this loss is partly made up by the extension of the measurements into the ultra-violet region, where as yet no exact measurements of oxygen lines have been made that I am aware of.

That there should be no error regarding the nature of the chemical element producing the lines, every precaution was taken to have the oxygen as pure as possible. Photographs of the spark in oxygen, between points of the purest platinum that I could procure, were also made. These were compared with the measured photographs of the spark between an iron and a platinum terminal, and provision was thus made for the detection of any error that might have arisen from impurity in the iron used in the terminal. As these photographs of the spark between platinum terminals in pure oxygen presented all the lines given in the table, these lines may be regarded as true oxygen lines. In addition to the oxygen lines given, the following feeble lines were observed, regarding the nature of which I was not quite satisfied, as they did not pass entirely across the spectrum, viz., 4490·30—4505·80—4525·50—4548·75. In the space extending from 4254·30 to 4345·15, many of the oxygen lines are assigned to wave-lengths occupied by other elements; for example, Cr, Ca, Sb, Ti, C, Bi. As other lines of these elements did not present themselves in the measured photographs, and as the lines in question were also found in the photographs of the spark between platinum terminals, they are to be regarded as true oxygen lines, although they are not given by other authorities. In some of the instances in which elements in addition to oxygen are assigned to a weak line in the solar spectrum, it is very possible that such assignments are in error, because of a lack of fractions in the determinations of the wave-lengths of these additional elements. Apparent discrepancies regarding wave-lengths in my determinations, and those of the other authorities, are sometimes explained by the fact that a line which is recorded as single in one case, is given as two lines in the other. It is also worthy of remark that in almost every instance in which a line is presented by one authority and omitted by the others, it is to be found in the column containing the photographic determinations, and is an evidence of the superiority of this method of recording the existence and positions of spectrum lines throughout the region over which it can act.

Examination of the table shows that the differences between the wave-lengths obtained for the lines of the electric spectrum in oxygen and the lines of the solar spectrum are very small. Out of the sixty-five lines of the solar spectrum which are, as we have seen, assignable to oxygen, in seventeen the coincidences are absolute; in four the difference is only five one-hundredths of a wave-length; in twenty-two, ten one-hundredths of a wave-length; in four, fifteen one-hundredths of a wave-length; in eleven, twenty-one one-hundredths of a wave-length, and in the remainder the greatest difference is only thirty-five one-hundredths of a wave-length, or about that which Ångström has made in different measurements of the same line in the solar spectrum.

The small figure attached as a power to each wave-length of the electric and solar spectra in the table is a proximate expression of the photographic strength of that particular line in each spectrum, and an examination of these upholds the statement made in a preceding paragraph that the oxygen lines of the solar spectrum are very weak when no other element furnishes a line which falls on the same wave-length. Of course photographic must not be compared with visual intensities, for as the one diminishes in the less refrangible regions of the prismatic spectrum the other increases. An example of coincidence in the lines of different elements, and consequent increment in strength, occurs in the line 4118, and probably in the line 4303 also, though it is supposed to be free.

In conclusion, I give a list of certain lines in Ångström's chart which have not as yet been assigned to any element, together with the wave-lengths of the same lines in my solar and electric spectra. From this table it will be seen that Ångström himself observed a number of lines, the relations of which to elementary bodies no one has as yet demonstrated, and which I believe represent the oxygen in the solar envelopes.

Table of Free Lines in Ångström's Solar Spectrum which may be attributed to Oxygen.

Draper's electric spectrum of oxygen.	Draper's solar spectrum.	Ångström's solar spectrum.
$4132{\cdot}90^{6}$	$4133{\cdot}00^{3}$	$4133{\cdot}20^{2}$
$4155{\cdot}75^{4}$	$4155{\cdot}60^{1}$	$4155{\cdot}80^{2}$
$4254{\cdot}50^{3}$	$4254{\cdot}30^{1}$	$4254{\cdot}55^{3}$
$4303{\cdot}00^{4}$	$4303{\cdot}00^{5}$	$4303{\cdot}00^{2}$
$4316{\cdot}50^{8}$	$4316{\cdot}60^{2}$	$4316{\cdot}50^{2}$
$4348{\cdot}30^{10}$	$4348{\cdot}20^{2}$	$4347{\cdot}95^{1}$
$4394{\cdot}50^{4}$	$4394{\cdot}50^{3}$	$4394{\cdot}45^{2}$
$4595{\cdot}50^{10}$	$4595{\cdot}40^{3}$	$4595{\cdot}20^{2}$
$4648{\cdot}15^{10}$	$4648{\cdot}15^{4}$	$4648{\cdot}75^{4}$
$4661{\cdot}50^{8}$	$4661{\cdot}50^{4}$	$4661{\cdot}70^{2}$

The subjects presented in this communication may be briefly summed up as follows :—

1. The resort to the process of reflection in producing and photographing solar spectra, and thereby avoiding certain errors, and the employment of the silvered surface itself of a glass grating.

2. The extension of the measurement of oxygen lines into the ultra-violet region.

3. The measurement in the region of less refrangibility than H, of lines of oxygen not heretofore recorded, and the use of projection as a method of measurement.

4. The establishment of a close relationship in position between certain lines in the solar spectrum and the lines of oxygen; the slight differences that exist being assignable to the experimental difficulties in the way of making accurate measures of the oxygen lines, and falling within the limits of error of experiment.

5. The evolution of the fact that the lines of the solar spectrum which appear to correspond to the lines of oxygen are weak, or faint, and show that that gas possesses a feeble absorbent power when compared with metallic vapours or gases like H, Fe, Ca.

6. The demonstration that in Ångström's chart there are many lines not assignable to any elementary body, and that these lines occupy very closely the positions of certain oxygen lines.

7. The suggestion that the proof of the presence in the solar envelopes of oxygen, and other substances giving faint lines, is a problem not to be solved by the comparison of two spectra of small dispersion. The solar spectrum in certain parts is so crowded with lines presenting all kinds of details, that the only satisfactory way is to make measures of the positions of these lines on a large scale, and as truly as possible, and then compare with these the most accurate measures of oxygen lines that can be made.

CYON'S RESEARCHES ON THE EAR[1]

II.

HAVING now described, at what we hope our readers will not consider inordinate length, the history of the subject up to the time when Dr. de Cyon commenced his second series of experiments, a history which he gives in the first part of his thesis in a very clear and impartial manner, we shall now give a short account of the new matter contained in the second part. This may be arranged under two heads—(1) experiments undertaken chiefly with the view of testing the kinetic theory, and (2) the statement of his own theory and arguments in support of it.

The experiments have obviously been made with extraordinary care and skill. Dr. de Cyon succeeded in producing the lesions which he intended to produce, without injuring any other part, and in most cases with scarcely any loss of blood; we can thus observe the effects of any particular operation without the slightest complication from concurrent injury or inflammation of the cerebellum. He has established in the most convincing way, (1) the fact observed by Flourens that the movements of the head always take place "in the plane of the divided canal," or, as we should express it, about an axis at right angles to that plane. (2) That the movements are much more violent, and that the loss of equilibrium is much more persistent, when the corresponding canals of both sides are cut, than when one only, or two dissimilar canals are divided. (3) That when all six canals are destroyed very violent and complex convulsions occur and continue for several days. If the animal survive this stage it gradually attains a condition in which its movements are effected with great deliberation, and in which the sense of sight is absolutely necessary to enable it to direct itself. These experiments were made upon pigeons, upon rabbits, and upon lampreys, the latter animals being especially interesting as possessing only four canals, two on each side.

So far, the results of the new experiments confirm and render more precise the knowledge derived from previous investigations, and they are in perfect accordance with the kinetic theory. One point, however, requires special notice.

Dr. de Cyon points out that the first movement made by an animal on the section of a canal, takes place in a direction "from the divided canal." It is not quite easy to make out the precise meaning of this phrase. It may, and probably does mean, that when the *left* horizontal canal is cut the pigeon moves its beak to the right; but, as the operator is at the back of the bird, it may also mean that the *back* of the head moves to the right and away from the divided canal. Judging, however, from the experiments upon the vertical canals, it is most probable that Dr. de Cyon means that the first movement takes place in such a manner that the ampulla of the divided canal precedes the canal. If this be the case, and if, as seems reasonable, we assume that the first effect of the division is stimulation and not paralysis, and that the movement is a compensatory one—that is, the result of an effort to preserve the same position—we are forced to the conclusion that the canal is affected by a rotation in which the ampulla *follows* the canal, contrary to the view somewhat hesitatingly expressed by Brown and Mach.

Dr. de Cyon has, however, made several experiments, the results of which cannot so easily be harmonised with the kinetic theory. These experiments were made expressly to test the truth of this theory, and in his opinion their results render it untenable. As Mach holds that change of pressure in the ampullæ excites the ampullary nerves and produces a sensation of rotation, Dr. de Cyon devised and executed a series of experiments so arranged that the pressure in the ampullæ should be changed, without injury to the membranous canals. 1. He opened the bony canal and allowed the perilymph to escape. 2. He opened the utricle and allowed the endolymph to escape, and observed that the whole membranous labyrinth collapsed. 3. He introduced into the space containing the perilymph small rods of dried laminaria; these rods slowly swelled by imbibing moisture, and must have considerably increased the pressure in the interior of the cavity. In none of these cases did he observe any trace of the Flourens phenomena. 4. He replaced the perilymph by a lukewarm solution of gelatine, which solidified, and inclosed the membranous labyrinth in an approximately rigid case. Still no Flourens phenomena were observed, but these at once occurred on pricking the membranous canals through the solid gelatine.

Dr. de Cyon further mentions as an argument against Mach's view, the fact that periodic changes of pressure occur in the contents of the labyrinth, synchronous with the heart's beat, and evidently connected with the change of arterial pressure. This, he thinks should, on Mach's hypothesis, produce irritation of the nerves and sensation. It must, however, be observed, that this change of pressure is produced simultaneously in all the six ampullæ, and that therefore the resultant rotation perceived would be zero.

But by far the most important evidence in opposition to the kinetic theory is derived from the section of the whole auditory nerve. Dr. de Cyon succeeded in performing this operation without serious injury to any other part, and found that rabbits, in which both of the auditory nerves had been divided, and in which, therefore, all nervous connection between the semicircular canals and the brain had been cut off, showed, after being subjected to rotation, the same symptoms of vertigo observed by Mach in the case of normal rabbits. It is unfortunate that Dr. de Cyon has not given further details of this most important experiment.

External irritations which, when small, are perceived only by the organs specially fitted for their perception, as a rule act, when very intense, upon other organs. Thus a feeble sound can

[1] Recherches expérimentales sur les Fonctions des Canaux semi-circulaires et sur leur Rôle dans la Formation de la Notion de l'Espace. Par Elie de Cyon, M.D., &c., Lauréat de l'Institut de France. Continued from p. 635.

only be heard, a small quantity of an odoriferous substance can only be smelled, a dilute solution of a sapid substance can only be tasted, but a very loud sound can be felt by producing vibrations sensible to the nerves of the skin; ammonia gas attacks the mucous membrane of the nose, and mustard bites the tongue. These are not sensations of hearing, smell, or taste, but they are sensations produced by external irritations which, when feebler, are perceptible by these special senses only. It is not, then, unreasonable to suppose that sudden and violent changes of rate of rotation should be perceived by the shock communicated to all the soft and movable parts of the body, although slighter changes may be perceptible only by the special organ of the sense of rotation. The experiment just mentioned is undoubtedly a crucial one, but, in order to obtain from it a decisive answer, it would be necessary to make a series of comparative trials, with varied rates of rotation, upon normal rabbits, and upon rabbits whose auditory nerves had been divided; if no difference is observed, even with moderate change of rate, the kinetic theory must be abandoned.

A great deal of valuable information might be obtained by carefully testing the delicacy and accuracy of the sense of rotation in deaf-mutes. Many deaf-mutes have not only the cochlea, but the whole internal ear, destroyed; if, then, the inmates of deaf and dumb establishments were systematically tested by means of such experiments as Mach and Brown made upon themselves, experiments which would, no doubt, greatly interest and amuse them, and if the condition of the internal ear were, in each case of *post-mortem* examination of a deaf-mute, accurately noted, we should soon obtain a mass of information which would do more to clear up the relation between the sense of rotation and the semicircular canals than any number of experiments on animals unable to describe to us their sensations.

We cannot pass from this criticism of the kinetic theory without noticing a passage in Dr. de Cyon's thesis which seems to show that he has not fully appreciated the bearing of this theory:—"Quelques mots seulement pour mieux faire ressortir l'invraisemblance *à priori* de la théorie de MM. Mach, Crum Brown et d'autres. Comment admettre que les canaux semi-circulaires servent à nous informer sur la rotation de la tête, quand nous voyons les mêmes organes parfaitement bien développés chez les animaux qui, comme les grenouilles ou les poissons, ont la tête presque immobile et qui d'ailleurs, pas plus que les autres animaux, n'exécutent pas habituellement des mouvements de rotation?

"Pourquoi justement la présence d'un organe des sens pour un mouvement peu habituel et pas pour beaucoup d'autres, pour les mouvements que les animaux exécutent continuellement?"—P. 47.

No doubt a frog or a fish cannot move its head freely without at the same time moving its body, but head and body together move and perform frequent and rapid rotations. Whenever a fish or other animal changes the direction of its motion, rotation takes place, and a knowledge of the amount and of the axis of this rotation is necessary if the animal is to retain any sense of its orientation. Of all animals a fish, moving and turning with great rapidity and sharpness in a dense medium often affected by complicated currents, seems to have most need of such an organ which serves the same purpose as a ship's compass, an instrument surely not useless because a ship is rigid and does not habitually perform movements of rotation.

We have already alluded to some of the phenomena of optical vertigo. This subject is discussed at considerable length by Dr. de Cyon, and it is therefore right that we should here explain somewhat fully the opinions held in reference to it by various experimentalists.

The phenomena themselves are, in the main, well known. If we rotate about a vertical axis either actively (that is turning ourselves) or passively (that is being turned round on a movable chair or platform by an assistant) we are at first fully aware that we are turning and that external objects are at rest; gradually external objects seem to move round us in a sense opposite to that of our real motion. If at this stage we stop we not only feel that we are being turned round in the opposite sense to that of the previous real motion, but we see, or think we see, external objects turning round. These two imaginary rotations, viz., that of our body which we feel, and that of external objects which we see, take place about the same axis, in the same sense and at the same rate. The axis is parallel to the line in the head which was the axis of the original real rotation, and the sense is, as already explained, contrary to that of the original rotation. These facts are well known and were fully described by Darwin in the "Zoonomia" and by Purkinje. Another phenomenon closely connected with them was first noticed by Purkinje, and has since been investigated by Breuer and by de Cyon. When a real rotation of the body takes place the eyes do not at first perfectly follow the movement of the head. While the head moves uniformly the eyes move by jerks. Thus, in the diagram, Fig. 3, where the abscissæ indicate time and the ordinates the angle described, the straight line $a\ b$ represents the continuous rotatory motion of the head and the dotted line the discontinuous motion of the eye.

Here it will be seen that the eye looks in a fixed direction for a short time, represented by one of the horizontal portions of the dotted line $a\ b$, and then very quickly follows the motion of the head, remains fixed for another short time, and so on. After the rotation has continued for some time the motion of the eye gradually changes to that represented by the dotted line $c\ d$ in Fig. 4. The eye now never remains fixed, but moves for a short time more slowly than the head, then quickly makes up to it, then falls behind, and so on. At last the discontinuity of the motion of the eye disappears, and the eye and head move together.

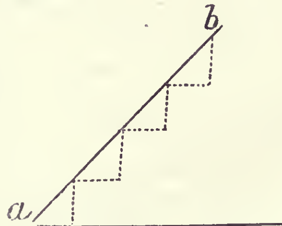


Fig. 3.

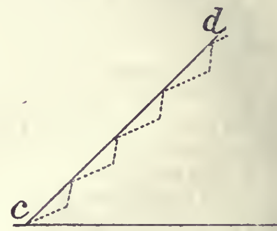


Fig. 4.

If now the rotation of the head be stopped (of course the body stops also) the discontinuous movements of the eyeballs recommence. They may now be represented by the dotted line in Fig. 5.

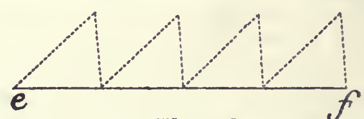


Fig. 5

Fig. 6.

The intermittent motion of the eyes gradually becomes less, passing through a condition such as that shown by the dotted line in Fig. 6, and at last ceases. The consideration of these oscillatory motions is greatly simplified if we draw diagrams similar to the above, but in which $\frac{dy}{dx}$ is made proportional to the *apparent* rate of rotation, that is, to the rate of rotation as perceived by the observer, instead of to the real rate as measured or inferred from external observations.

The *apparent* motion of the head and of the eyeballs is shown in Fig. 7 when uniform real rotation is kept up until it ceases to be perceived, and is then suddenly stopped.[1] The accented letters a', b', &c., correspond to a, b, &c., in Figs. 3, 4, 5, and 6.

We now see what is the real nature of the oscillatory movements. The eye remains for a short time in an apparently fixed

[1] In order to represent accurately the phenomena, the oscillations should be much more numerous and of course smaller. As five or six complete turns are required before all sense of rotation is abolished, and as Dr. Breuer finds at least ten oscillations in one complete turn, there should be at least fifty between a' and e'. These could not be represented without making the figure either very large or very indistinct. Such diagrams are not used by any of the physiologists who have investigated the subject, and must not be interpreted too rigidly, as both the duration and the extent of the oscillations vary considerably. The figures, however, represent the general nature of the phenomena sufficiently for our purpose.

direction, that is, in a direction which the experimenter, judging from his sensation of rotation, concludes to be fixed, then rapidly returns to its original position relatively to the head, again for a short time looks in an apparently fixed direction, and so on.

Some physiologists have considered these oscillatory movements as the cause; others—and notably Dr. de Cyon—regard them as the effect, of the visual vertigo. The latter opinion seems the more reasonable; and indeed both visual and tactile vertigo seem to be matters of judgment rather than of sensation. When the real rotation stops the experimenter perceives, by his sense of rotation, that his head is turning round; he *feels* that his body and the chair on which he sits are at rest relatively to his head; he *sees* that external objects are at rest relatively to his head; and he concludes that, as his head is turning round, his body, his chair, and all external objects must be turning round also—his eyes oscillate precisely as they would do if all these imaginary rotations were real.

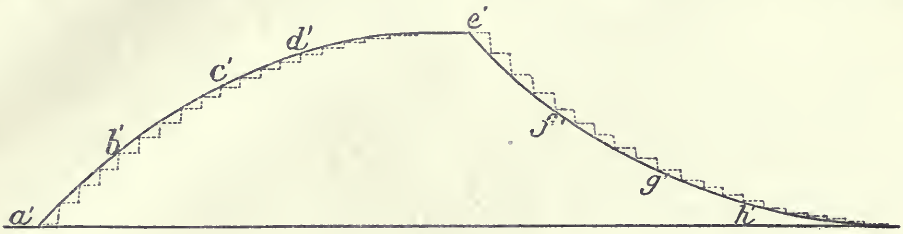


Fig. 7.

Similar oscillatory movements of the eyeballs are described by Dr. de Cyon as resulting from the section or mechanical irritation of the semicircular canals in rabbits, and are by him referred to reflex action dependent on intimate anatomical connection between the roots of the nerves of the ampullæ and those of the motor nerves of the eye. The points of chief importance noticed by him are:—

1. At the moment of irritation the spasm of the muscles of the eyeballs has a tetanic character; immediately afterwards the oscillations commence, and last for a variable time, depending on the intensity of the irritation, and rarely exceeding half-an-hour. The frequency of the oscillations varies from 20 to 150 per minute.

2. The direction of the oscillations depends on the particular canal cut or irritated. Dr. de Cyon describes, somewhat minutely, the direction in each case, but states that he finds it difficult, on account of the peculiar position of the eyes in the rabbit, to give a precise determination of these directions. From his descriptions we can, however, gather, with considerable probability, that the eyeballs oscillate about an axis perpendicular to the plane of the divided or irritated canal.

3. The oscillations, caused by the irritation or section of any one canal, cease when the opposite auditory nerve is divided; new irritations of the canal then produce only tetanic contractions.

The direction of the initial tetanic spasm is given by Dr. de Cyon, and appears to coincide with what we should expect if rotation with the ampulla *preceding* be supposed to excite the ampullary nerves; this result leads to the suspicion that we may have misinterpreted the author's account of the initial movement of the head on section of one canal in the pigeon.

We have now only to state and examine Dr. de Cyon's theory of the function of the canals and of the part which they take in the formation of the idea of space.

This theory is stated in a very general form, and it is difficult by a single quotation to do full justice to it. We quote, however, two passages in which it is distinctly enunciated, and refer the reader for its full discussion to the thesis itself:—

"Les canaux semi-circulaires sont les organes périphériques du sens de l'espace; c'est-à-dire que les sensations provoquées par l'excitation des terminaisons nerveuses dans les ampoules de ces canaux servent à former nos notions sur les trois dimensions de l'espace. Les sensations de chaque canal correspondent à une de ces dimensions.

"A l'aide de ces sensations, il peut se former dans notre cerveau la représentation d'un espace idéal sur lequel seront rapportées toutes les perceptions de nos autres sens qui concernent la disposition des objets qui nous entourent et la position de notre corps parmi ces objets."—P. 64.

"La disposition des nerfs, dans trois plans perpendiculairs l'un à l'autre, se prête à merveille pour une pareille fonction. Nous pouvons très bien nous figurer comment les sensations d'etendue, dans trois plans, dont la disposition, chez tous les vertébrés, répond exactement aux trois co-ordonnées de l'espace, peuvent être utilisées par notre intelligence pour la construction d'une notion de l'espace.

"Je dirais plus: aucun autre sens ne présente une relation aussi facile à saisir entre la représentation et la sensation, que le sens d'espace, d'après ma manière de voir."—P. 73.

This is not inconsistent with the kinetic theory as explained above. The difference is that that theory does, and Dr. de Cyon's does not, explain what the sensations of the canals are and how they contribute to our ideas of direction or orientation in reference to three rectangular axes.

There are two ways in which we may investigate the action of an organ of sense:—We may examine and compare the information we obtain by means of the sense under a great variety of conditions; this is the way in which our knowledge of physiological optics has been chiefly obtained: or we may study the effects of injuries of the organ, either occurring naturally or intentionally produced. A detailed theory has, in the case before us, been sooner attained by the first of these ways than by the second. This theory must be tested by experiments carried out in every appropriate way, and, if necessary, must be modified in accordance with their results. We may thus expect to obtain a knowledge of the mechanism of the sense of orientation as complete as that which we have of the mechanism of the sense of sight. Dr. de Cyon's thesis contains the record of a very considerable step in this direction.

ALEX. CRUM BROWN

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

THE President and Fellows of St. John's College, Oxford have just passed the following resolution *nem. com.*: "That independently of the granting of Fellowships for the assistance of research, the college shall from time to time make money grants for that purpose." The granting of Fellowships alludes to a clause in the new Statutes which has just been drafted, in which a Fellowship is set apart for this purpose.

PROF. HUXLEY will lecture at a general meeting of the Working Men's College, Upper Kennington Lane, to be held on the 24th, at 8.30 P.M., to inaugurate the new premises of the College, near Vauxhall Station, one part of which has been fitted up as a chemical laboratory, with all the appliances needed for the study of practical chemistry.

THE New South Wales correspondent of the *Colonies* states that, in consideration of the necessity which is now felt for extending the curriculum of Sydney University and augmenting its teaching powers, the Colonial Government have consented to ask Parliament for an additional annual grant of 5,000*l*. This will enable the Senate to make the following additions to the present course of study:—(1) Mental philosophy, law, history, and English literature; (2) all the education necessary for the medical profession; (3) a complete course of natural philosophy, coupled with mechanics and engineering; (4) the addition of organic chemistry and metallurgy to the chemical school; and (5) biology, including animal and vegetable physiology. The Senate will also be in a position to establish a faculty of science, and to confer the degrees of Bachelor and Doctor of Science, and also degrees in medicine, on those who have received their education in Sydney.

SCIENTIFIC SERIALS

Verhandlungen der k.k. zoologisch-botanischen Gesellschaft in Wien (vol. i. 1878).—This volume is in every respect equal to its predecessors, both for variety as well as scientific interest of its contents. The papers which deserve special praise are: Lichenological excursions in the Tyrol, by F. Arnold.—On the metamorphosis of several species of *Tipulidæ*, by Th. Beling.—Remarks on the metamorphosis of insects in the spirit of the theory of descent, by Dr. Fr. Brauer.—Researches on *Phytoptocecidiæ*, by Dr. Franz Löw.—Notes on some new *Cecidomyidæ*, by the same.—On some new exotic *Hesperidæ*, by H. B. Möschler.—Ennumeratio Ichneumonidum, exhibens species in alpibus Tiroliæ captas, by Aug. E. Holmgren.—Other papers of interest are: On the mollusc-fauna of Galicia, by J. Król.—Ornithological notes, by P. Hanf.—On the birds of Ecuador, by A. von Pelzeln.—On ear-shaped grass panicles, by E. Hackel.—On the fungi of Carniola, by Wilhelm Voss.—Note on a new mollusc, by M. Folin.—On some new *Cucujidæ* in the Royal Museum at Berlin, by Edmund Reitter.—Researches on *Lyctidæ*, by the same.—Analytical classification of the species in the genera *Sphindus* and *Aspidophorus*, by the same.—On the influence of changes in the conditions of vegetation upon the forms of the organs of plants, by Otto Stapf.—On a remarkable form of Lenticels, by the same.—On some species of the *Chalcidiæ* genus *Eurytoma*, obtained by artificial breeding, by Dr. G. Mayr.—On the flora of Fiume, by A. M. Smith.

SOCIETIES AND ACADEMIES

London

Royal Microscopical Society, October 9.—H. J. Slack, president, in the chair.—This was the first meeting of the present session. Numerous presents were announced and acknowledged, and Major Festing and John Borland were elected Fellows of the Society.—The president called attention to a specimen of the perforating proboscis of a moth which had been received from Colombo, and compared its structure with that of a species which had been the subject of discussion at a former meeting.—An interesting paper was read by Prof. Owen upon certain fossils found in the middle Purbeck, to which he had given the name of *Granicones*, and which, after careful comparative examination, he had decided to be the dermal scutes of a lacertian closely resembling the now existent Australian species, *Moloch destructor*. He pointed out that the remains found in these rocks were chiefly those of marsupials, and that in the mesozoic strata both animals, plants, and shells had now their only living representatives at the antipodes.—Communications were read from Col. Woodward on the modification of the illuminator for balsam-mounted objects, also from the American Microscopical Congress recommending the adoption of the $\frac{1}{100}$ millimetre as the standard for microscopical measurements.—A discussion ensued, in the course of which it was suggested that much advantage would arise to microscopists from the more careful adherence on the part of makers to the Society's standard screw, and also greater uniformity as to size of tube, eye-pieces, and other mechanical details.

Paris

Academy of Sciences, October 7.—M. Fizeau in the chair.—The following papers were read:—On the irreducible covariants of the quantic of the seventh order, by Prof Sylvester.—Observations on M. Gruey's recent communication on a gyroscopic apparatus, by M. Hirn. He considers the new apparatus presents a special case of phenomena analysed in his own memoir on the gyroscope.—On a singular case of heating of a bar of iron, by M. Hirn. A workman holding a cylindrical iron bar (about 1 m. long and 0·08 in diameter) on another piece so as to be struck with a hammer on the free end, said he felt the bar at each stroke greatly heated and then as quickly cooled. M. Hirn verified this with surprise. He estimated at 35° the sudden variation of temperature. For best observation one should come very near the bar and seize the iron about 0·01 m. from the end struck (a position requiring some faith in the address of the workman!). M. Hirn thinks the phenomenon quite subjective, *i.e.*, one of sensation. In certain conditions, sonorous vibrations affecting the sensitive nerves, may cause at the periphery of the body a sensation of heat, just as, *e.g.*, pressure or a blow on the eyes may awaken in these organs the sensation of light. This view he gives with reserve, and desires physicists to test a bar, in such circumstances, with a Melloni thermometer.—Observations on M. Bouillaud's note inserted in last week's *Comptes Rendus*, by M. du Moncel.—Discovery of two small planets at Clinton (New York), by Mr. Peters.—Second letter of Mr. Watson on the discovery of intra-Mercurial planets. M. Mouchez thought that while the American observations have rendered very probable, or almost certain, the existence of intra-Mercurial planets, they have not sensibly improved the knowledge of their orbit.—Two remarks on the general relation between pressure and temperature, determined by M. Levy, by Herr Weber.—On the manner in which is distributed, among its points of application, the weight of a hard body placed on a polished, horizontal, and elastic ground; identity of this mode of distribution for a plane and horizontal supporting base with that of an electric charge in equilibrium in a thin plate of the same form, by M. Boussinesq.—On the resolution, in whole numbers, of the equation (1) $ax^4 + by^4 = cz^2$, by M. Desboves.—On a new gyroscopic pendulum, by M. Gruey.—Revision of the flora of the Malouines (Falkland Isles), by M. Crie. At present there are about 394 species (Hooker enumerated 368). The compositæ count more individuals than all the twenty-seven other families of dicotyledons combined. The gramineæ occupy the second place. As in most arctic flora, the most numerous are the cryptogams. Algæ have nearly 100 representatives. The author adds nine new species of muscineæ to those described by Hooker.—Researches on the urea of organs, by M. Picard. During digestion, urea is formed in the muscles, the brain, and the liver; these have all more of the substance than an equal weight of blood. During fasting, urea seems to be formed only in the brain and the muscles.—Note on M. Perez' work on the buzzing of insects, by M. Jousset de Bellesme. This is the substance of a paper read to the French Association in August, and giving much the same results as those of M. Perez, communicated to the Academy in September.—On *Trichodonopsis paradoxa* (Clap), by M. Schneider.—Structure and botanical affinities of the cordaïtes, by M. Renault. The order of Cordaïteæ is more nearly related to the Cycadeæ than any other family of Gymnosperms, and the Cycadeæ, including Sigillarineæ, must have reached an immense development at the coal epoch.—On the atmosphere of planetary bodies and the terrestrial atmosphere in particular; remarks on Mr. Sterry-Hunt's recent paper, by M. Meunier.

CONTENTS

THURSDAY, OCTOBER 24, 1878

THE CONSERVATION OF RIVERS

THE question of the control of rivers has during the last two years occupied more attention than had previously been bestowed upon it for a considerable period. The disastrous floods in South Wales and other parts of the country in the summer of 1875 caused a great outcry at the time, and this had hardly been forgotten when the evil recurred with still greater intensity, and in some cases more damaging effects in the winter of 1876-77. The immense amount of damage caused by floods in these two years, coupled with the fact that, in the opinion of a great many, they occurred at more frequent intervals than in former years, at length drew the attention of Government to the subject. Consequently in 1877 a Select Committee of the House of Lords was appointed to inquire into the operation of existing statutes in regard to the formation of, and proceedings by, Commissioners of Sewers, and Conservancy, Drainage, and River Navigation Boards; to consider by what means they could be more inexpensively constituted and their powers enlarged so as to provide more efficiently for the storage of water and the prevention of floods. At the same time two other committees of the House of Commons were sitting on the same subject, but with reference only to the River Thames, one dealing with the question of the prevention of floods within the metropolitan district and the other within the whole valley of the Thames. A vast amount of information was collected from the numerous witnesses examined by these committees. Considerable difference of opinion was, as might be expected, found to exist in respect of the causes and best means of prevention of the severe floods experienced of late years in our different river basins, but by far the greater majority of the witnesses agreed that the floods were yearly getting worse than formerly, that the river channels were getting into a worse and worse state of neglect, and, what is a most important point in the practical bearing of the question, that the damage caused was so great that the carrying out of extensive remedial measures could in most instances be made to pay. It would appear at first sight a comparatively simple matter, given the drainage area of the river basin, the intensity and duration of the maximum observed rainfall, and the hydraulic inclination of the river, to calculate the sectional area of channel requisite for the maximum discharge; this is doubtless the case, but unfortunately in the early history of the world the formation of rivers was left entirely to the forces of nature without the professional assistance of a competent engineer. If an engineer of suitable experience were called upon to design a river from source to the sea, he could doubtless successfully accomplish the work, but the problem the legislature is now called upon to deal with is of far greater complexity. Not only have the rivers themselves been doing all they could to make things difficult, meandering in graceful curves through plains where they ought to have gone in straight lines, and silting up where greater depth of channel was necessary, but the inhabitants of the country most interested have followed the same course now for some centuries by constituting themselves under Acts of Parliament into innumerable small drainage boards with certain definite powers within their own districts, but unable and unwilling to join together and act for their general good.

The Select Committee on the Prevention of Floods within the Metropolitan District had a comparatively simple question to deal with. Accurate and long-continued observations on the levels of high water were produced, showing the result of the various works of improvement that have been carried out since it was decided to remove old London Bridge. The tides in the Thames are the resultant of two tidal waves, one reaching the mouth of the river by the North Sea, the other by the English Channel, the former arriving at the river usually about three hours before the latter. But certain conditions of the wind on some occasions bring these two tidal waves together; thus a south-westerly gale accelerates the English Channel wave and retards the North Sea wave, while again the latter may be increased by a north-west wind off the coast of Scotland blowing at the same time. The highest tide on record in the Thames was that of November 15, 1875, which rose to the height of 4 feet 7 inches above Trinity high-water, or 3 feet 2 inches above the predicted height; that of January 2, 1877, exceeded the predicted height by 3 feet 4 inches, and reached a height of 4 feet above Trinity high-water. The conditions producing these results were nearly the same: in the south a south-westerly gale, in the north a north-westerly with an easterly wind blowing up the Thames, combined with a low barometric pressure over the river valley and continued heavy rains. Almost the worst possible concurrence of circumstances having on previous occasions occurred, it will not be too much to predict their recurrence at some future time, and possibly with much more disastrous effects; for whereas the highest tide on record, that on November 15, 1875, was only 3 feet 2 inches above its calculated height, that of December 12, 1845, rose to 5 feet 7 inches above its predicted height, showing that a suitable concurrence of conditions might produce a tidal wave upwards of 2 feet at least above the highest on record. In the face of these facts and the repeated inundations of low-lying parts of the metropolis, it is simply astounding that so little should be done not only to obviate the recurrence of a well-known evil, but to prevent a much worse one from happening. Bearing in mind the great and costly works carried out by the Metropolitan Board of Works in the main drainage of London, the Thames embankments and the numerous new streets, and, finally, also in their last great work of buying up and freeing the bridges, it would appear a small thing to raise the height of the river banks at the few places necessary to prevent the periodical inundations; but though there is no difference of opinion as to the character and extent of the works required, the Select Committee and the Board do not seem to have agreed on the question of how the improvements are to be paid for, and the matter appears again to have gone to sleep.

The general question of the control of the entire river channels is one of much greater complexity than the prevention of the periodic overflows of the Thames

in the Metropolitan District. It was, therefore, with great pleasure that we saw that the President of the Mechanical Section of the British Association, Mr. Edward Easton, did so much to further the consideration of the question by making it the subject of his opening address at the recent Dublin meeting, and the number of papers by various well-known authorities that followed on the different branches of the same subject, with the discussions thereon, still more enhanced its value. The scope of the whole question involves, as well as the prevention of floods, the following considerations, as pointed out by the President:—the supply of pure and wholesome water for domestic and sanitary wants, the supply of water of proper quality and sufficient quantity for industrial purposes, the proper development of water power, land drainage, and irrigation, navigation, and the preservation of fish. It has long been found in most districts that many of these interests are mutually antagonistic, that the use of water for manufacturing purposes and sanitary arrangements interferes with the supply of pure water for domestic purposes in the places lower down the stream; that the utilisation of water-power by the erection of mill-dams and weirs impedes the passage of floods and renders efficient land drainage more difficult, while on most navigable rivers the navigation and drainage interests seem to be constantly in opposition. Before two of the Select Committees referred to much useful information was given by the different witnesses examined, but the absence of discussion left much useful work to be done at the Mechanical Section at Dublin.

The facts most prominently brought forward and the remedies advocated may now be considered. The passage to the sea of the water falling as rain may not only be too much retarded, causing loss to the producing power of the country by submerging large tracts of valuable land for a long period; but it may easily be too much accelerated, causing great loss and inconvenience from drought in summer. Notwithstanding the persistence of rainy weather, so much complained of in our climate, most of our rivers would sink to most insignificant dimensions were not some provision made for impounding the water in times of drought. In the fen country admirable provision for floods was made by the early Dutch engineers employed, who, finding the natural river channels, though adapted for the ordinary flow, quite incapable of containing the flood waters, raised banks a considerable distance back from the natural bed of the river. Long tracts of country were thus inclosed and formed into a channel for the passage of flood waters; and, had the works been completed to the outfalls of the river, or had those outfalls been artificially deepened and straightened in those days to the extent they have since, there is little doubt that the result would have been completely satisfactory. As it is, with the extension of works at the outfall of the rivers, rendered necessary by increasing quantity and size of shipping, the original works have been allowed to fall into decay, or have been so altered by the erection of dams and sluices as no longer to fulfil their original uses. What is required in these rivers, then, is to a great extent merely to restore them to the state they were in when left by their original improvers, continuing the improvement of the outfalls, and removing the shoals and other obstructions that have grown up since that time. To remove the whole of the weirs and sluices would be to take away almost the entire water supply, in time of drought, for agricultural purposes. The weirs will therefore have to be made movable, rendering it an easy matter to retain the water supply in summer, while, by lifting the sluices in good time, on the approach of floods, the channel will be freed of all obstructions. In the case of all the rivers considered—the Thames, the Severn, the Shannon, the Great Ouse, Nene, Witham, and others—it was shown that the channels had been suffering continued deterioration from the formation of shoals and the growth of weeds, and that the dams and weirs erected for the maintenance of the water-level in dry weather were not provided with sufficient sectional area for the flood discharge. The engineering difficulties in the way of a complete improvement of our principal rivers are not at all insurmountable, but the legislation necessary has proved abortive. Each river basin is cut up into a number of drainage districts, formed under Acts of Parliament obtained at various times, and wholly without reference to each other. One district is unable to obtain powers to make improvements in its portion of the river without encountering a determined opposition from the districts below, who find that their works would be imperilled by the alterations; and thus a great proportion of the cost of any improvements is spent in parliamentary contests. There is now an almost general agreement in the opinion that the difficulties in the management of rivers are due to divided authority, and that the question could be satisfactorily dealt with if entire river-basins were placed under the control of one body of conservators. The conclusion arrived at is that each river-basin must be placed under the care of a single Conservancy Board, who shall have to decide on and execute any general scheme for the improvement of the river; that this Board shall consist of members chosen from the various districts Boards, which would have the power of executing minor works in their own districts, not affecting the general *régime* of the river, but subject at all times to the control of the combined Board; that the powers of rating conferred on the combined Board shall extend over the whole river basins, but that a special additional rate shall be levied on those districts most directly benefited by the improvements.

It has been proposed, also, that a Central Board should be established in the metropolis, presided over by a Cabinet Minister, to which appeal from the decisions of the various Conservancy Boards can be made, and that this Central Board shall have powers analogous to the Local Government Board, of issuing provisional orders, to be subsequently confirmed by Act of Parliament, thus greatly diminishing the cost to the local ratepayers of the improvements they desire. At the conclusion of the Dublin meeting a recommendation was made by the Mechanical Section that a Committee of the British Association should be appointed to ascertain what steps should be urged on Government to bring about the undivided control of river basins in this country. As it appears very uncertain what action Government may take on the Reports of the Select Committees of

both Houses on this question, the Council of the British Association may be able to do a work of great national importance by pointing out clearly and definitely what must be done, and by the publication of an annual report on the progress made preventing the subject again being allowed to drop.

STELLAR OBJECTS SEEN DURING THE ECLIPSE OF 1869

It will be remembered by the reader who has interested himself in the published reports of observations of recent total eclipses of the sun, that during the totality on August 7, 1869, at a point in Iowa, called St. Paul's Junction, several observers attached to a party organised by Mr. W. S. Gilman, of New York, remarked below the sun what they termed "a little brilliant," and that one of the number using a small telescope, reported having seen just before the sun disappeared and as he came out again a minute crescent, in a similar direction from the moon. Commodore Sands, then Superintendent of the Naval Observatory, Washington, expressed his regret that these objects should not have been seen by Mr. Gilman himself, who had experience of the use of the telescope and was using a larger instrument than the others who had optical aid—but his "plan of operations" did not permit of it. The facts are thus stated:—A few moments after the corona formed, a small but exceedingly bright point, like a star, was noted independently by four of the party, two of whom it is mentioned were observing without telescopes; it appeared near the limits of the corona, below the moon's disc, and with one exception the observers located it a little to the right of an "anvil-shaped" prominence, or "at about 230° from the north point, reckoning by the east," and it is added that each of the observers felt quite positive that what he saw was truly a star. With respect to the small crescent Mr. Gilman reports that about half a minute preceding totality another member of the party, Mr. Vincent, came to him exclaiming that he saw a miniature-crescent-shaped star under the moon, and asking him to verify the observation, but, interested in his own work, he did not at the moment do so; on Mr. Vincent returning more urgent than ever, Mr. Gilman says he did look in a hurried manner but saw nothing in the few seconds he gave to the search; he afterwards states, however, that he does not think he looked so far away from the moon as the crescent was located in a drawing made immediately after the eclipse by Mr. Vincent; in this drawing it was placed "at one and a half times the moon's diameter from its limb, and to the left of a perpendicular down to the horizon." Mr. Gilman adds he could not connect this crescent with the small star of the other observers, indeed Mr. Vincent estimated the object seen by him at three times as far removed from the moon's limb as the small star, which would assign for the latter a distance of about half a degree, corresponding very well to the expression used by the four observers who noted it, that it was near the limits of the corona. Dr. B. A. Gould, now Director of the Observatory at Cordoba, who observed this eclipse at a different station, gave some attention to a search for any object near the sun which might be an intra-Mercurial planet, and he states he saw the star π Cancri, but did not meet with any other stellar body. This star being at the time in a similar direction from the moon's centre, to "the little brilliant" of the Iowa observers, there has been a pretty general opinion that it was the object remarked by them, and, in conversation with Dr. Gould several years since, I found him tolerably well satisfied that he had thus sufficiently explained their observations. But the discovery, or rather discoveries, of Prof. Watson, lend a new interest to them, and a more strict examination of the circumstances may not be out of place here. The position of St. Paul's Junction is stated to be in latitude 42° 47′ 30″ N., and longitude 19° 5′ 45″ W. of Washington. The totality was observed to commence at 5h. 48m. 4·6s., ending at 5h. 51m. 34s. M.T. at Washington, so that the middle occurred at 10h. 58m. 22s. M.T. at Greenwich, which agrees exactly with a calculation made with the *Nautical Almanac* elements. We will assume 10h. 59m. G.M.T. as the time to which the observations of the brilliant point apply. Correcting the moon's place for the effect of parallax, we find her apparent position at this time to be in right ascension, 9h. 11m. 26·7s., and north declination 16° 13′ 58″; her augmented semi-diameter was 16′ 37″. We must assume that both star-like object and crescent were on an angle of 230°, the latter one-and-a-half times the moon's diameter from her limb, and the former at one-third of this distance, whence, referring to the moon's centre, we have, for the crescent, $\Delta\alpha = -$ 3m. 32s., $\Delta\delta = -42'\cdot7$, and for the bright little star, $\Delta\alpha = -$ 1m. 46s., $\Delta\delta = -21'\cdot4$; and thus,

	R.A. h. m. s.	Decl. ° ′
Stellar object	9 9 41	+ 15 52·6
Crescent	9 7 55	+ 15 31·2

The former was therefore 28′ and the latter 56′ south of the ecliptic.

Now with regard to the star, the presence of which has been supposed to explain the observation of the four observers who noted "the little brilliant," there has been some slight confusion. In a note inserted in the last "Annual Report" of the Royal Astronomical Society it is stated that the object seen "has been satisfactorily identified as the star π^1 Cancri," which is assuredly a mistake. π^1, according to Argelander, is only a seventh magnitude, which is hardly to be glimpsed with the most acute sight in the darkest winter sky. For π^1 no doubt we must read π^2, or 82 Cancri. But this star, also, is of a degree of brightness wholly insufficient to allow of it being possible to discern it at all so near the sun's place without some optical aid in the still illuminated sky-ground, much less to be caught up as a brilliant point of light, with the naked eye; the "Durchmusterung" estimate is 5·8m., which is confirmed by the careful estimations of the second Radcliffe Catalogue, where we find it rated 5·9m., or, in round magnitudes, a sixth. It should be mentioned that the apparent place of 82 Cancri was in right ascension 9h. 7m. 59·5s., declination 15° 28′ 56″, agreeing nearly with that we have found for the minute crescent, but 33′ from the small star. It appears probable, in view of Prof. Watson's discovery, that Dr. Gould may have mistaken an intra-Mercurial planet for π^2 Cancri, and if the statements of the four observers at St. Paul's Junction (one of them, by the way, a lady) are accepted, it can hardly be doubted that they also were

attracted by an unknown object, since in that part of the sky there is no star which could be visible as they describe it. It is significant that Regulus, upwards of 12° from the sun's place, was only noticed as "a glimpse-star" at St. Paul's Junction. J. R. HIND

LAKE DWELLINGS

The Lake Dwellings of Switzerland and other Parts of Europe. By Dr. Ferdinand Keller. Second edition, greatly enlarged. Translated and Arranged by John Edward Lee, F.S.A., F.G.S. Two vols. (London: Longmans. 1878.)

IT is ten years since the first edition of Dr. Keller's valuable work was published, and since that time vast additions have been made to a knowledge of the subject of which it treats. Mr. Lee, besides incorporating in the present edition the whole of Dr. Keller's Seventh Report, gives short accounts of every lake settlement that has been investigated; and an idea of the progress that has been made in this department may be learned from the fact that in the first edition the number of objects drawn and described numbered about 1,500, whereas in the present edition they number between two and three thousand.

The second volume, a thick one, is entirely occupied with illustrations of the portions of lake dwellings that have been found, of the situations in which the remains exist, and of the multitude of objects that have been collected *in situ*. As a frontispiece to the first volume is given an illustration of a restored lake dwelling, which nestling near the shore of a Swiss lake, in a picturesque situation, backed by trees and overtopped by lofty mountains, makes a very pleasant picture. Over the fence which surrounds the edge of the platform we see the sloping roofs of the huts built thereon, and out in the lake two boats fishing by means of nets. There is some difficulty, it seems, as to the question of windows, Mr. Lee contending that the huts must have contained these, as the inhabitants would often have to work at their flint implements by the side of the fire. But we scarcely think windows necessary on this account; we have frequently seen the peasants in Scotland, in the long winter evenings, carry on knitting and other even more delicate work by the light of the fire or at most with the addition of a lighted spill of resinous pine. On the whole, however, Mr. Lee's restoration is likely to be a pretty close approximation to reality, to judge from the mass of data collected in Dr. Keller's work.

After an introduction describing the various forms of lake dwellings, the methods used in collecting the relics, on the original discovery of the dwellings, and a few sensible remarks on the ages of stone, bronze, and iron, each of the lakes in which dwellings have been found is treated of separately, the nature of the remains described, and an attempt made to realise the actual nature of the original work. Sections, for there are no chapters, are devoted to the discussion of special objects found and of special points and circumstances connected with the structures, the life and habits of the builders, and the environment generally. One of the most interesting sections is that on the Geographical Distribution of Lake Dwellings, from which we see that they have been found in many other places besides Switzerland—in Germany, Austria, France, Ireland, Scotland, and Wales. Mr. Lee rightly includes under the general head of Lake Dwellings the Crannoges of Ireland, Scotland, and Wales; for although those of Ireland at least were in several important respects different from the structures found on the Continent, still they have several strong points of resemblance. The reason for the choice of site was no doubt pretty much the same—security from sudden attack. Remains of lake-dwellings have even been found in the heart of London, in the peaty marsh where Finsbury now stands, and where, "in Romano-British times, some outcast natives lived." Gen. Lane Fox describes these remains in the *Anthropological Review* for April, 1867. Dr. Keller has always been adverse to the idea of lake-dwellings having been circular; but just when the English work was completed, Mr. Lee tells us that the author sent him word of remains of an undoubted circular dwelling having been found at Fang, in the Lake of Morat. Very interesting are the sections by Prof. Oswald Heer on the plants of the lake-dwellings, and by Prof. Rütimeyer on the animals.

From the mass of data which has been collected, Dr. Keller gives a very clear account of the general form of these lake settlements, and of the different varieties under which they may be classed. As to the substructure, first of all of pile dwellings, which are by far the most numerous in the lakes of Switzerland and Upper Italy. Piles of various kinds of wood sometimes split, but in general mere stems with the bark on, sharpened sometimes by fire, sometimes by stone hatchets or celts, and in later times by tools of bronze and probably of iron, were driven into the shallows of the lakes, provided they were not rocky, at various distances from the shores. Sometimes the piles were close together, sometimes wide apart. On the level tops of the piles were laid the beams, which were sometimes fastened by wooden pins and sometimes let into mortises cut in the heads of the piles. Sometimes the vertical piles were strengthened by cross timbers below the platform beams. Generally the platform itself consisted of one or two parallel layers, and was of very rude structure, though sometimes the stems were split and joined together with some approach to accuracy. The distance from shore seems to have varied considerably; but it is curious that when a lake dwelling had been inhabited both in the stone and bronze ages, that part evidently used in the bronze age is frequently further from the shore and deeper in the lake than that which belongs to the age of stone. Otherwise, nearly the same mode of construction prevailed in the pile-dwellings during the ages of stone, bronze, and iron. In some cases, at least, there is evidence that the dwellings were connected with the shore by a narrow platform or bridge, formed also of piles. In certain cases, also, it is seen that artificial elevations were made on the bed of the lake by piles of stones brought in boats; one of these boats, still loaded with stones, is to be seen at Peters Island, on the Lake of Bienne. The stones must have been put down after the piles had been driven more or less deeply into the mud.

Certain forms, known as frame-pile dwellings, have been found in the Lake of Zürich. In this form the piles, instead of having been driven into the mud of the

lake, had been fixed by a mortise and tenon arrangement into split trunks, lying horizontally on the bottom of the lake, evidently when the mud was more than usually soft.

Another form of these ancient habitations has been designated fascine dwellings. Instead of a platform supported on a series of piles, Dr. Keller tells us these erections consisted of layers of sticks, or small stems of trees built up from the bottom of the lake, till the structure reached above the water-mark, and on this series of layers the main platform for the huts were placed. In these dwellings upright posts were used as stays or guides for the great mass of sticks reaching down to the bottom of the lake. Fascine dwellings occur chiefly in the smaller lakes, and belong to the stone age.

Another form of lake-dwelling which has been long known, are the Crannoges or "wooden islands," found in Ireland and Scotland; one has also been found in North Wales. "The crannoges, at least in Ireland, were frequently but not exclusively placed on natural islands, or on shallows which approached to this character; sometimes they were built up from the bottom of the lake on the soft mud, exactly in the manner of the fascine dwellings of Switzerland. They are surrounded by a stockade of piles driven into the bed of the lake, so as to inclose either a circle or an oval; the diameter varies from 60 to 130 feet. These piles are usually in a single row, but sometimes the rows are double and even treble. Occasionally the piles are boards, not round stems. The lowest bed within this inclosure is commonly a mass of ferns, branches, and other vegetable matter, generally covered over with a layer of round logs, cut into lengths of from four to six feet, over which is usually found a quantity of clay, gravel, and stones." Although it is probable that both these crannoges and the Swiss lake-dwellings, which it will be seen had much in common with regard to structure, were erected in lakes greatly for the sake of security, still the lake-dwellings were evidently places of permanent habitation, while the Scotch crannoges, at least, are believed by good authorities to have been chieftains' forts and fastnesses for occasional retreat. The crannoges were actually used far into the age of iron, while the Swiss lake-dwellings belong almost exclusively to the age of stone, and disappeared, Dr. Keller tells us, as far as we at present know, about the first century.

With regard to the superstructure of these pile-dwellings, it appears that on the platform was laid and beaten down firmly a bed of mud, loam, and gravel. The framework of the huts consisted of small piles or stakes, and sometimes of the upper or projecting parts of piles, longer than those on which the platform was built. Round the bottom, at least, of the hut a board or skirting was fitted, and the walls or sides were in a great measure made of a wattle or hurdle-work of small branches, woven in between the upright piles, and covered with a considerable thickness of loam or clay. The huts seem in nearly all cases to have been rectangular, though in at least one instance, referred to above, the circular form has been found. As to whether there were internal divisions in the huts no evidence has yet been found, though it seems certain they were thatched with straw or reeds. "Every hut had its hearth, consisting of three or four large slabs of stone; and it is probable from the almost universal prevalence of clay weights for weaving, that most, if not all, of them were furnished with a loom."

Such was, as far as can be gathered from the mine of information contained in Dr. Keller's volumes, the nature of these curious structures belonging to a remote age and a primitive people. But that the builders of these dwellings were considerably advanced beyond the lowest stage of civilisation is evident, not only from the structures themselves, but from the many articles found in connection with them, and which are so copiously figured in Dr. Keller's second volume. Implements, weapons, and ornaments, mostly in stone, but not infrequently in bronze and even in iron, have been found, of elaborate and finished structure. Beautifully wrought and ornamented textures, showing considerable skill not only in weaving, but in embroidery. Fishing-nets, fish-hooks, and boats, these lake-dwellers had, domestic animals and agricultural implements, all showing that, whoever they were, they were well on the way to a fairly high civilisation; they were fishers, hunters, shepherds and agriculturists, and to no small extent manufacturers. "The endeavours of the settlers to live together in permanent abodes and in a sociable manner, is a positive proof that they had long known the advantages of a settled mode of life, such as applies to the lake-dwellings, and that we have to look upon them not as wandering pastoral tribes, still less as a mere hunting and fishing people. A settled union of a great number of men in the same place, and of hundreds of families in the neighbouring bays, would never have taken place if there had not been a regular supply of provisions at all times of the year, and some beginning of social order."

To all interested in the progress of our race, the two fine volumes of Dr. Keller and Mr. Lee are well worthy careful study; they enable the student to put together with wonderful fulness a picture of a form of society that must have had an early beginning, and the dwellings, and implements, and manners and customs of which are full of interest. Considerable light is thrown on the subject of Dr. Keller's work by what we know of existing pile-dwellings in various parts of the world, not the least interesting of which are those found in Lake Mohyra, in Central Africa, by Commander Cameron.

OUR BOOK SHELF

Annual Record of Science and Industry for 1877. Edited by Spencer F. Baird. (New York: Harper Brothers. London: Trübner. 1878.)

THE high opinion which we have previously expressed concerning this excellent annual is sustained by the present volume, which, however, is smaller than its predecessors, owing to a change that has been made in its character. Hitherto the "Annual Record" has consisted of two distinct parts, a summary of scientific progress made during the year, and a series of abstracts of the more important papers and articles in the scientific journals. This dual character it has been found impossible to sustain, owing to the rapid increase in the number of scientific papers, and also probably to the larger range taken in by the contributors: hence the abstracts have been abolished and the summary alone retained. The change is a useful one, placing more space at the disposal of the editor and embarrassing the reader less. At the same time we regret the absence of references to

the papers themselves, which might be added as footnotes, or incorporated in the text; and perhaps more distinction might be drawn between the longer researches, or more valuable memoirs of the year, and mere passing scientific observations. However, it is easier to criticise than to compile a work like the one before us. Our readers will form some idea of the comprehensive nature of this "annual record" by the following summary of its table of contents:—Astronomy, together with reports of the American observatories, contributed by Mr. Holden, of the United States Naval Observatory, Washington. Physics of the globe, followed by general physics, written by Prof. Barber, who also contributes the next section on chemistry. Mineralogy by Dr. Dana, and geology by Dr. Sterry Hunt. Hydrography and geography follow, the geography of North America being specially full. Microscopy, anthropology, zoology, and botany are contributed by able men in each department. Agriculture, engineering, technology, and industrial statistics are less full, and some of the abstracts given in technology would, we think, have found a better place under the head of physics, such, for example, as the telephone, phonograph, &c.

The observatory reports are a feature of the present volume, information being given concerning the *personnel* of each observatory, its principal instruments, the subjects of special observation during the past year, and those to be taken up during the coming year, and lastly the principal publications emanating from each observatory during the past year.

The bibliography at the end of the annual, giving the list of works on science published during 1877, seems most thoroughly and ably done, and so also is the index to the whole volume, and the concise and useful necrology of scientific workers. W. F. B.

Choice and Chance. An Elementary Treatise on Permutations, Combinations, and Probability. With 300 exercises. By W. A. Whitworth, M.A. Third edition, revised and enlarged. (Deighton, Bell and Co., Cambridge.)

We have all three editions before us, and so are able to mark the growth of this work, which has been very considerable. The number of pages in the last edition is ten less than that of the second edition, but the volume is much thicker, and much of the matter is in smaller type. The work had already attained the position of a standard one on the subjects of which it treats, and it maintains and even improves its position in the present edition. Here, even in the elementary parts are to be found many propositions of great utility which are not to be met with, so far as we know, in any form elsewhere. We do not mean to say that they are not known to mathematicians, but writers have not introduced them into the text-books. Besides chapters on Permutations and Combinations, we have a chapter on *Distribution*, that is the separation of a series of elements into a series of classes, and one on *Derangements* (if a series of elements have been arranged, or if they have a proper order of their own, and we place them in some other order, we *derange* them). Under the head of *chance* we have a full treatment of that part of Probability which usually finds a place in algebraical treatises. Remarks "On the Disadvantages of Gambling," which formed an appendix to the last edition, here forms part of a chapter which also has a few paragraphs to show that insurance is the reverse of gambling, and discusses the effect of the repetition first of a fair wager, secondly of a wager at odds, thirdly of a fair wager on a scale proportioned to the speculator's means, the general case of a lottery with prizes of different value, and closes with a fairly exhaustive account of the Petersburg Problem. The novelty of this edition is a chapter on the geometrical representation of chances. We shall hope to see this chapter considerably enlarged in a future edition. The whole treatment may be said to assume nothing but what a well-primed algebraical student should be able to master. What is much wanted is a general treatise on the subject of Probability for English students. Mr. Todhunter's history of the theory down to the time of Laplace is a most interesting and able one, but it does not fill up the gap. In this branch, as in many others, we are dependent upon French writers, and still must have recourse to the works of Laplace, Poisson, and Liagre.

Pine Plantations on the Sand-Wastes of France. Compiled by John Croumbie Brown, LL.D., &c., &c. (Edinburgh: Oliver and Boyd. London: Simpkin Marshall and Co., 1878.)

The subject of the reclamation of sand-wastes by the planting of coniferous trees or of grasses, shrubs, or other plants is one always of much importance. The extension of pine plantations has a two-fold interest over and above the primary cause of planting, namely, that arising from the general improvement in the appearance of the country, as the plants make growth and develop themselves into goodly forms, and that which is more utilitarian, but withal equally important—in the production of timber. Anything that can be done towards reducing the desolation of these French sand-wastes is a point gained not only, as Dr. Brown points out, for the benefit of France herself, but as indicating that what has been accomplished there may also legitimately be expected elsewhere, "not necessarily by the same means, but by means as appropriate, if they can be discovered." As will be seen from the title, the book does not claim originality, it professes to be a compilation, and the copious extracts, with the usual inverted commas, extending often over continuous pages makes this announcement unnecessary. Nevertheless a good work has been done in bringing together in a convenient form a great deal of valuable matter, scattered about in various books inaccessible for the most part to readers for whom the present work is intended, and amongst whom it will, no doubt, chiefly circulate; containing as it does detailed information on every branch of coniferous culture, from a consideration of the soils most suitable to satisfactory culture, to the collecting of the resin, and other economic products, and the diseases and injurious influences to which the plants are subject.

From the range of country under consideration, it will be understood that the pines treated of are limited to very few species, such as *Pinus sylvestris*, *P. maritima*, and *P. pinaster*. J. R. J.

La Morfologia vegetale. Esposta da T. Caruel. (Pisa, 1878.)

A new text-book of vegetable morphology, characterised by freshness both in the mode of treatment and in the illustrations, is an acquisition to botanical literature, even though written in a language which is unfortunately not familiar to most English readers. Prof. Caruel starts with the primary classification of all vegetable structures into the thallus, which displays no external differentiation, and the cormus, consisting of a central stipes (caulome), to which are attached appendages (phyllomes) more or less differing from the stipes. Under the head of the thallus he then discusses propaguli (of Muscineæ), conidia, sporidia (including zoospores), sporules (or spores, properly so-called), the pollen, and phytozoa (or spermatozoids). The general description of the cormus leads to an account of the various special forms which it assumes, viz., to the morphology of flowering plants and vascular cryptogams; and to the various modes of the reproduction of cormophytes by a process of impregnation, that is, the union of the contents of two dissimilar cells. Finally, Prof. Caruel discusses the various subjects connected with the genesis of species, and concludes with the

system of classification of the vegetable kingdom, an outline of which we have already given to our readers (vol. xviii. p. 646). The author brings to his work a mind trained to great accuracy in the use of terms and in the perception of morphological homologies. Great advantage would ensue by the introduction into vegetable morphology in this country of a similar scientific terminology. A. W. B.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

American Exploration [1]

North-western Wyoming and Yellowstone Park

DURING a portion of the period 1870-4, the writer was engaged in service as military engineer of the staff of the troops serving in the geographical department of the Platte, U.S.A. This business involved the accumulation of local geographical information for the use of the troops who were constantly in contact with hostile Indians. The most troublesome of these were the Sioux, whose prowess in battle has since been shown to the world in the story of the battle of the Little Big Horn—the Custer massacre.

Within the limits of this department were several dark spots on the map marked "unexplored." One of these was the north-western corner of Wyoming territory—a region of vital interest to geographers, comprising, as it did, the crown of the North American continent. There lay, wrapped in impenetrable mystery, the trackless forest country where, in native lore, the Great Spirit evidenced his eternal anger by spouting great columns of water and smoke into the air far above the highest tree-tops, by filling the air with strange rumbling sounds, and Bad Medicine smells; and by flinging the waters of the great river into the bottomless depths of the Cañon of the Yellow Stone. There lay the Lake Beautiful high on the mountain side, from whose borders the Great Spirit puffed great clouds of smoke. There were the strange mountains that no man had ever entered, and whose existence was only indicated on the best maps by a hazy line of feeble *hachures*. Somewhere in that black and forbidding mass of the Unknown Mountains were hidden the secrets of the sources of the Yellowstone, the largest feeder of the Missouri, as well as those of the Columbia and the Snake. They were literally the "Unknown" Mountains. They had been looked at in awe from a few spots on the west and north, and from the south-east by a few travellers whom they had turned back from the glittering prize just as it seemed within their grasp. They were unknown to the Indians who lived on their border, with the exception of a handfull of outcasts from the Crows and Shoshonees, who, driven from all intercourse with their fellows, were obliged to live in the mountains, of which even they had acquired but a limited and uncertain knowledge. Along their eastern border spread out the hunting-grounds beyond compare of the Sioux, Arrapahoes, Cheyennes, and Crows, from whence no white man or peaceful Indian could ever hope to return unless prepared to cope with fearful odds. The mildest geologist on the planet could not have entered that happy hunting-ground without finding his own—without leaving his bones to whiten in a lonely vale and his scalp to decorate the evening entertainment of some untutored child of nature.

In the winter of 1872-73 General Ord, commanding the department, informed me of his desire that what remained of this dark spot in his field of operations should be cleared up, and, if possible, that a passage-way be discovered between the sources of Yellowstone and Wind Rivers. This would give easy access to the recently-discovered Yellowstone Park region, and very much simplify the question of the shipment of supplies to some of the posts in Montana. This was the sole origin and animus of the expedition.

At that time there had been published concerning the Yellowstone Park region the following:—Hayden's "Geological Report of 1871," Barlow and Heap's "Reconnaissance," and Doane's "Narrative." Of these, Hayden's work was a rapid geological reconnaissance not based upon any topographical work worthy of mention; Barlow and Heap were two officers of engineers who made a military reconnaissance, in which astronomical and topographical instruments were used by trained observers; while Doane, also an officer of the army, simply recounted what he saw. It was also known that Hayden had spent the season of 1872 in the immediate Park region, but that he had not examined the country to the southward and eastward of it. Acting upon this information, and such as had come to me through considerable hard service in the neighbouring country, I decided to carry what explorers call a preliminary triangulation from the surveyed region of the Union Pacific Railroad northward into the Yellowstone Park, there connecting with the work of previous explorers. This route would take me through the region infested by hostile Sioux, thence through the unknown mountains from the eastward into the Park, and thence recrossing somewhere in the neighbourhood of the sources of the Yellowstone. I felt satisfied that, with a thoroughly efficient pack-train, this could be accomplished before the early snows rendered the mountains impassable.

The event proved that if those pack mules had not been handled with the utmost skill by the men in charge of them, and had they not had the agility of squirrels, we should have been turned back into a hornet's nest of redskins.

I had no particular intention of reduplicating anybody's work, but if such happened I am very glad of it. In the cause of science a little duplication and reduplication are things not to be sneered at. Dr. Hayden has been at work this very season reduplicating his own work in the Park and mine too, and I do hope and pray, if there be anything erroneous or incomplete in my work, that he may find and point it out. I am not afraid of truth and right, even though it lay me prone in the dust. It would be a pity indeed if, with the sum of $75,000 and upwards at his disposal, with an outfit that has been the growth of so many years of his own and other people's experience, and with the only dangerous Indians in the whole region completely quiescent and humbled to the dust—it would be a pity indeed if the sum of our knowledge of that wonderful region were not very largely increased and many former errors discovered.

Exploring is at best imperfect work, so far as the survey, which is its foundation feature, is concerned. Observations for longitude with any known portable instruments are painfully erratic unless there be abundant time; angles taken with a light shaky transit in a gale of wind from the summit of one mountain to the most pointed aspects of the summits of others in sight must make some very "holey" triangles; and yet this is the best that has been done or can be done unless there be time and money for a regular survey.

With such an expedition as mine it would have been a sad pity not to give trained scientists an opportunity to gather some of the treasures in our path, and so after careful selection and the advice of one of the most competent scientists in the country (Prof. O. C. Marsh), I took with me some specialist observers. They were excellent hard-working men, and have every reason for being proud of their work. The sum of money placed at my disposal for the work was $8,000.

Prof. Geikie has, I fear, been misled by the one-sided Report of a Congressional Committee.[1] This Report does not afford a fair idea of the issues with which it deals. It conveys the impression that the Engineer Department of the army had been making efforts to absorb the Hayden survey. I was at that time in a position to know that such was not the case.

It may be well to add that both by law and long-continued practice, a portion of the duties of the engineers of the American army comprises public explorations and surveys, and they have always given the greatest possible assistance to specialist observers, who can always do very much more and better work when they have no cares other than those of observation and reflection.

Of Dr. Hayden I would like to say that few men deserve more commendation for successful labour than he. Where others had always failed, he had succeeded in securing from Congress

[1] See NATURE, vol. xviii. p. 315.

[1] House Doc. Report 612, 43rd Cong. 1st Session.

annual appropriations of about $75,000. I know him to be an indomitable worker in the field, and well remember the day when his annual arrival in our department was hailed with the greatest interest, and was the signal for every possible act of kindness and assistance from one end of the command to the other.

Prof. Geikie's quotation, that the presence of an armed escort needlessly irritated the hostile Indians, is out of the pale of decent characterisation. In those days army men were of the opinion that no party having less than 100 long-range breech-loading rifles could safely pass into certain portions of the Sioux country, and that minimum-sized parties could just about take care of themselves on the defensive. That was the basis upon which my party was organised. That there was some error in this judgment was shown by the Custer massacre not long after. To the other portion of the quotation, that the geologists of the Interior Department were never molested by Indians, I will state, from personal knowledge, that they have always taken the most precious care not to operate in the neighbourhood of dangerous Indians, a very sensible proceeding for parties without armed escort. Through carelessness and lack of knowledge of the system of guarding camps, they have been stampeded once or twice by thieving Indians who were after plunder, but did not dare to kill anybody. This misfortune has recently befallen them in Yellowstone Park, where the commonest precautions would have made it impossible. Their presence alone, without armed escort, seems to have been "irritating to the Indians."

W. A. JONES

Geological Climate and Geological Time

I HAVE been much interested in Prof. Haughton's communication to NATURE, vol. xviii. p. 266, on the subject of geological climate and geological time. I fully agree with him that geological climates cannot be explained by any change in the position of the poles, even supposing such change possible, and for the reason assigned by him, viz., that we have no palæontological evidence of an arctic climate in any portion of the earth in any geological period previous to the glacial. But I have some objections to make to the data on which he bases his estimates of time, and therefore to his views as to the *cause* of geological climates.

I. He supposes aqueous agencies to commence operation, and therefore the archæan (azoic) era to commence when the earth surface had cooled to 212° F., evidently because, as he thinks, water could not exist as a liquid on the earth's surface at a higher temperature. But the writer forgets that with all the water of the ocean in the air as vapour, and the large quantity of carbonic acid now existing in the form of carbon and the carbonates also in the air, the pressure of the primeval atmosphere must have been many times—perhaps several hundred times—greater than now, and the boiling, or rather precipitating, point of water very much higher than 212°.

II. Again: Even with a surface temperature from internal causes of 212°, the crust of the earth must have been *very thin*, not more than 40–50 ft. (for increased atmospheric pressure, though greatly affecting the boiling-point of water, would not sensibly affect the fusing-point of rocks). Under these conditions—by the law of equilibrium—the inequalities constituting land-surfaces and ocean-bottoms could not possibly exist; the ocean would be universal, and therefore there would be no erosion and sedimentation. Therefore when dry land first appeared and erosive agencies commenced to act at the beginning of the azoic era, the surface temperature from internal causes must have been much less than 212°. For my part I believe that this temperature had already become very small, the surface had become substantially cool, and the crust very thick before land could exist, and the history recorded in stratified rocks could commence.

III. Therefore, though I agree with Prof. Haughton that all the evidence we have indicates uniform climates in early geological times, I would not, like him, attribute this to warm decrease of surface temperature from *internal* causes alone. I would attribute it almost wholly to *external* causes. Among these are:—1. The *constitution of the atmosphere*. The greater amount of carbonic acid and water in the atmosphere would shut in and accumulate the sun's heat on the earth surface according to the principle discovered by Tyndall, and applied to the explanation of geological climates by Sterry Hunt. 2. It is probable that the *heat received from the sun* was much greater then than now; for the sun is now cooling, and has been cooling throughout all geological times much faster than the earth. 3. The idea of Poisson, that in the journey of our system through the stellar universe it may be *now* in a region in which the *heat received from space* is exceptionally small, has been, perhaps, too much neglected in these speculations concerning geological climates. 4. The more *uniform distribution* of this greater surface-temperature from any or all these causes, would of course still farther increase the temperature of *high latitudes*. This more uniform distribution might be due to the position and shape of land masses, or to the less area and the less height of the then lands.

IV. Lastly: I think that a little reflection will show that while it may be allowable to roughly estimate the *relative lengths* of different *eras* by the *relative extreme thickness* of their strata; it will not at all do to estimate the *absolute length of geological times by the extreme thickness of all the strata*. For as the *measuring rod* is not the rate at which sediments are now accumulating at any one place, but the *average rate* over the whole bottom of the sea, so *the thing to be measured* is not the extreme thickness of the strata at any one place, much less the extreme thicknesses of different formations in different places piled one atop the other, but the *average thickness of the strata* over the whole earth surface.

Most of the points brought out here have already been discussed by me in my recently published "Elements of Geology."

JOSEPH LE CONTE

University of California, October 2

The Magnetic Storm of May 14, 1878

I WAS much interested in Mr. Perry's note inserted in NATURE, vol. xviii. p. 617, showing the simultaneity of the magnetic storm of May 14, 1878, in different parts of the earth's surface. As these magnetic disturbances are always accompanied by electric disturbances (earth currents) in telegraph wires, I was anxious to find out what effects were observed upon our wires in England on the same day, viz., May 14. I append an extract from the diary of the Relay Station at Haverfordwest, a very important station on the wires running from London to Valentia, and where very careful observations are made of all interferences with the regularity of the working of the wires:—

May 14.

P.M.

6.40.—First appearance of relays not closing automatic switch well on down side of 202.

7.10.—London finds some difficulty in reading on 199; no cause visible here.

7.40.—Up side of automatic switch on 202 rather unsteady.

9.0. —Variations in London's current on 200.

9.10.—Strong positive deflections (earth currents) on Cork wires.

9.15.—Cork complains of marks missing and running; no doubt the result of deflections.

9.30.—Great difficulty with automatic switches on Cork wires owing to continuous strong positive deflections which tend to opening of switches when battery currents are in opposite directions, and to close them when in same direction. Probably earth-currents influenced them earlier in evening.

9.40.—Deflections diminishing; wires going better.

9.45.—Deflections ceased.

10.0. —All wires going well; weather wet.

10.10.—*Aurora Borealis visible about this time.*

11.25.—Earth currents very strong again on all wires, causing much trouble with automatic switches. Almost full deflection on up side of 201 and 202.

10.40.—Deflection on 202 reversed, and not so strong as before.

10.45.—Full deflection on up side of 202.

10.48.—Deflection on 199 causes up automatic switch to remain depressed.

MIDN.—Earth currents disappearing.

May 15.

A.M.

12.40.—Electric storm seems to have spent itself. Weather fine; clear moonlit sky.

May 15.
A.M.
1.15.—Weather wet. High winds coming on.
7.0. —Nearly six hours' rain and wind.

The numbers 199, 200, 201, and 202 are those of the wires.

Mr. Ellis has also told us (p. 641) of what was observed at Greenwich. Although the magnetic disturbance commenced about 6 P.M. at Greenwich, it only reached a maximum at 9 P.M., when the electric disturbances were observed at Haverfordwest, though indifferent working, most probably due to them, commenced at 6.40. At 11.45 P.M. the north end of the needle at Greenwich moved sharply westward, and about the same time the currents were reversed at Haverfordwest. From midnight to 12.40 A.M. the currents gradually disappeared. Earth currents were also active at Greenwich, as indeed they were everywhere. Unfortunately the wires at the post-office were so fully occupied with press work during these hours, that no observations could be taken. It will be noticed that the aurora borealis was observed. W. H. PREECE

October 21

Giddiness

A TRUE theory of the cause of giddiness ought to explain the following extreme experience, which deserves record in print. The method was first told to me by a friend; I once tried it myself successfully in a mitigated form, and will assuredly not repeat the experiment, and I persuaded a philosophical friend to try it also, with much the same result. Stand in the middle of a soft field where you can't hurt yourself by tumbling on the ground or against anything; avoid having your best clothes on, and secure appreciative spectators. Then put both hands one above the other on the top of your umbrella or walking-stick, and bend down until your forehead rests on the back of your hands. Thus your legs will be vertical, your body will be more or less horizontal, and the umbrella will be vertical. Shut your eyes. Then get a friend, by touching your hips, so to guide you that you shall circle three times, with a sidelong gait, round the vertical umbrella as an axis. Finally raise your head quickly, and try to walk straight as though nothing particular had happened. What will occur is a frightful giddiness and feeling of sickness, a sense of the ground rising up tumultuously on all sides, a wild rush to save yourself, and a headlong tumble.

F. G.

OUR ASTRONOMICAL COLUMN

THE SATURNIAN SATELLITE HYPERION.—In a letter from Prof. Asaph Hall it is remarked that the ephemeris for September last, given in NATURE, requires correction by nearly two days, although it was deduced from the elements which he showed to closely represent the Washington observations in 1875 and 1876 (*Astron. Nach.*, No. 2,137). There appear to be great difficulties attending the satisfactory determination of the orbit of this satellite, doubtless arising in the main from the magnitude of the perturbations with which its motion is affected, but for this reason it is the more necessary that it should be regularly observed, and a rough indication of the position of so extremely faint an object is better than none.

If we take for a peri-saturnium passage September 24·8393 G.M.T. with Prof. Hall's other elements, the calculated and observed distances on September 27 agree, and the computed angle is one degree in excess of observation. On the same system the following angles and distances are found for 10h. G.M.T.:—

	Angle.	Distance.		Angle.	Distance.
	°	″		°	″
Oct. 25	271	137	Nov. 1	95	217
,, 26	268	82	,, 2	94	225
,, 27	250	23	,, 3	94	211
,, 28	107	41	,, 4	93	175
,, 29	99	99	,, 5	92	119
,, 30	97	150	,, 6	87	49
,, 31	96	191	,, 7	287	29

THE MEAN PARALLAX OF A STAR OF FIRST MAGNITUDE.—Prof. Gylden, director of the Observatory at Stockholm, has reported the result of a preliminary calculation bearing upon the mean distance of a star of the first magnitude. Remarking that in the actual state of our knowledge, when a general result is to be deduced from the parallaxes so far measured, we must not only take into account the apparent brightness of the stars concerned, but also their apparent proper motions, since the magnitude of the proper motion is to be viewed as at least as sure a criterion of a measurable parallax as the apparent brightness; as a hypothesis, it is then assumed that the actual parallax p of a star of nth magnitude with the apparent motion s, will be given by the formula

$$p = P \frac{s}{\sigma_n M_n}$$

where σ_n signifies the mean apparent motion of a star of nth magnitude and M_n the distance estimated according to its brightness. P is a constant, which for $M = 1$ indicates the mean parallax of a star of the first magnitude.

Prof. Gylden takes the following data, depending upon observation, for sixteen stars, of which the parallaxes are supposed to be known with the greatest degree of approximation:—

	mag.	p	s
		″	″
α Centauri	1	0·900	3·674
61 Cygni	5	0·511	5·221
L. 21185	7	0·501	4·734
34 Groombr.	8	0·307	2·801
L. 21258	8·5	0·260	4·403
Oelt. 17415	9·5	0·247	1·200
σ Draconis	5	0·222	1·925
Sirius	1	0·193	1·252
70 Ophiuchi	4	0·162	1·108
α Lyræ	1	0·153	0·349
1830 Gr.	7	0·147	7·053
ι Urs. maj.	3	0·133	0·525
α Bootis	1	0·127	2·258
γ Draconis	2	0·092	0·063
α Aurigæ	1	0·046	0·438
α Urs. min.	2	0·046	0·045

From the adopted values of p, s, together with the products $\sigma_n M_n$, 16 equations of condition can be formed, the solution of which by the method of least squares will furnish the value of P. But Prof. Gylden points out that this mode of treatment will not be found to answer the object in view, since the determination of the weights of the different equations, which can in no wise be considered equal, is attended with great difficulties. On the assumption of equal weight, the value of P comes out, 0″·048. As another mode of treatment, normal equations may be formed in various ways, each containing the unknown quantity P, and consequently each serving for a determination of the quantity sought. The sum of all the equations thus obtained gave P = 0″·062.

This value, however, is greatly influenced by several stars of the first magnitude with large proper motion. Omitting α Centauri, α Bootis, and Sirius, the remainder give P = 0″·086, or if all stars of the first magnitude are omitted, P = 0″·083. Again, if all stars with extreme motions are neglected, and a value of P derived from the nine stars which remain, with proper motions less than 2″ annually, it is found to be 0″·084. Prof. Gylden considers that the near agreement of values obtained from these two calculations, in which the extreme case of brightness and motion enter, affords some support to the inference that the relation indicated by the above formula between parallax, apparent motion, and apparent brightness may be taken as an approximation to the truth. It may be remarked that Prof. Peters found for the mean parallax of a first-magnitude star, 0″·102 ± 0″·026; the new value is within the limit of his probable error.

ON THE TIDES OF THE SOUTHERN HEMISPHERE AND OF THE MEDITERRANEAN[1]

On the coasts of the British Islands and generally on the European coasts of the North Atlantic and throughout the North Sea, the tides present in their main features an exceptional simplicity, two almost equally high high-waters and two almost equally low low-waters in the twenty-four hours, with the *regular* fortnightly inequality of spring tides and neap tides due to the alternately conspiring and opposing actions of the moon and sun, and with large *irregular* variations produced by wind. Careful observation detects a small "diurnal" inequality, (so called because it is due to tidal constituents having periods approximately equal to twenty-four hours lunar or solar) of which the most obvious manifestation is a difference at certain times of the month and of the year between the heights of the two high-waters of the twenty-four hours, and at intermediate times a difference between the heights of the two low-waters.

In the western part of the North Atlantic and in the North Sea, this diurnal inequality is so small in comparison with the familiar twelve-hourly or "semi-diurnal" tides that it is practically disregarded, and its very existence is scarcely a part of practical knowledge of the subject; but it is not so in other seas. There is probably no other *great* area of sea throughout which the diurnal tides are practically imperceptible and the semi-diurnal tides alone practically perceptible. In some places in the Pacific and in the China Sea it has long been remarked that there is but one high water in the twenty-four hours at certain times of the month, and in the Pacific, the China Sea, the Indian Ocean, the West Indies, and very generally wherever tides are known at all practically, except on the ocean coasts of Europe, they are known to be not "regular" according to the simple European rule, but to be complicated by large differences between the heights of consecutive high-waters and of consecutive low-waters, and by marked inequalities of the successive intervals of time between high-water and low-water.

On the coasts of the Mediterranean generally the tides are so small as to be not perceptible to ordinary observation, and nothing therefore has been hitherto generally known regarding their character. But a first case of application of the harmonic analysis to the accurate continuous register of a self-recording tide-gauge (published in the 1876 Report of the B.A. Tidal Committee) has shown for Toulon a diurnal tide amounting on an average of ordinary midsummer and mid-winter full and new moons to nearly $\frac{4}{5}$ of the semi-diurnal tides; and the present communication contains the results of analysis showing a similar result for Marseilles; but on the other hand for Malta, a diurnal tide (similarly reckoned), amounting to only $\frac{1}{4\frac{1}{2}}$ of the semi-diurnal tide. The semi-diurnal tide is nearly the same amount in the three places, being at full and new moon, about seven inches rise and fall.

The present investigation commenced in the Tidal Department of the Hydrographic Office, under the charge of Staff-Commander Harris, R.N., with an examination and careful practical analysis of a case greatly complicated by the diurnal inequality presented by tidal observations which had been made at Fremantle, Western Australia, in 1873-74, chiefly by Staff-Commander Archdeacon, R.N., the officer in charge of the Admiralty Survey of that Colony. The results disclosed very remarkable complications, the diurnal tides predominating over the semi-diurnal tides at some seasons of month and year, and at others almost disappearing and leaving only a small semi-diurnal tide of less than a foot rise and fall. These observations were also very interesting in respect to the great differences of mean level which they showed for different times of year, so great that the low-waters in March and April were generally higher than the high-waters in September and October. The observations were afterwards, under the direction of Capt. Evans and Sir William Thomson, submitted to a complete harmonic analysis worked out by Mr. E. Roberts. Not only on account of the interesting features presented by this first case of analysis of tides of the southern hemisphere, but because the south circumpolar ocean has been looked to on theoretical grounds as the origin of the tides, or of a large part of the tides, of the rest of the world, it seemed desirable to extend the investigation to other places of the southern hemisphere for which there are available data. Accordingly the records in the Hydrographic Office of tidal observations from all parts of the world were searched, but besides those of Fremantle, nothing from the southern hemisphere was found sufficiently complete for the harmonic analysis except a year's observations of self-registering tide-gauge at Port Louis, Mauritius, and personal observations made at regular hourly, and sometimes half-hourly, intervals for about six months (May to December) of 1842, at Port Louis, Berkeley Sound, East Falkland, under the direction of Sir James Clark Ross. These have been subjected to complete analysis.

So also have twelve months' observations by a self-registering tide-gauge during 1871-2 at Malta, contributed by Admiral Sir A. Cooper Key, K.C.B., F.R.S.

Tide-curves for two more years of Toulon (1847 and 1848) in addition to the one (1853) previously analysed, and for Marseilles for a twelvemonth of 1850-51, supplied by the French Hydrographic Office, have also been subjected to the harmonic analysis.

These results, both for the southern hemishere and the Mediterranean, will form the subject of a paper which Capt. Evans and Sir William Thomson hope to communicate to the Royal Society in the course of the coming session. In the meantime the numbers resulting from the harmonic analysis are submitted without further comments to the British Association for comparison with those for other places in previous Reports of the Tidal Committee. Those of them which represent the most important of the diurnal and semi-diurnal tides are shown in the following table, which includes also for immediate comparison the results for Toulon, 1853.

R in every case denotes, as in previous tables of the British Association Committee, the range of the particular tidal constituent on *either side of mean level;* so that $2R$ is the whole rise *from lowest to highest of the individual constituent.* (In comparing results with those shown in the Admiralty Tide Tables, it must be borne in mind that in the latter it is the rise above the level of ordinary low water spring tides that is given as "heights."

ϵ (technically called the epoch) is the angle, reckoned in degrees, through which an arm, revolving uniformly in the period of the particular tide, has to turn till high water of this constituent, from a certain instant or era of reckoning defined for each constituent as follows:—

Definition of ϵ.[1]

To explain the meaning of the values of ϵ given in the following table of results, it is convenient to use Laplace's "astres fictifs," or ideal stars. Let them be as follows:—

M the "mean moon."

[1] Abstract of paper by Capt. Evans, R.N., F.R.S., and Sir William Thomson, LL.D., F.R.S., read in Section E of the Dublin meeting of the British Association.

[1] This definition for the several cases of K diurnal, and O, P, Q, and L differs by 90°, or 180°, or 270° from the definition given in the British Association 1876 Report for a reason obvious on inspection of Tables I. and II., pp. 304 and 305 of that report, which (except in respect to the longitudes of perigee and perihelion) show ϵ as previously reckoned for the several constituents.

S the "mean sun."
K for diurnal tide, a star whose right ascension is 90°.
K for semi-diurnal tide, the "first point of Aries," or ♈.
O a point moving with angular velocity 2σ, and having 270° of right ascension when *M* is in ♈.
Q a point moving with angular velocity $2\sigma - \varpi$, and 270° before *M* in right ascension when the longitude of *M* is half the longitude of the perigee.

P a point moving with angular velocity, 2η having 270° of right ascension when *S* is in ♈.
N a point moving with angular velocity, $\frac{3}{2}\sigma - \frac{1}{2}\varpi$, and passing alternately through the perigee and apogee of the moon's orbit when *M* is in perigee.
L a point moving with angular velocity, $\frac{1}{2}\sigma + \frac{1}{2}\varpi$, and passing alternately through 90° on either side of the perigee of the moon's orbit when *M* is in perigee.

			SOUTHERN HEMISPHERE.			MEDITERRANEAN.				
			Fremantle, Western Australia. Lat. 32° 3′ S., Long. 115° 45′ E. Year 1873-74. I = 27°·8; ν = +6°·5.	Port Louis, Mauritius, Lat. 20° 9′ S., Long. 57° 11′ E. Year 1838-39. I = 28°·6; ν = −1°·5.	Port Louis, Berkeley Sound, East Falkland. Lat. 51° 29′ S., Long. 58° 0′ W. Year 1842, from May 10 to September 8, and from November 14 to December 15. I = 25°·6; ν = −11°·3.	Toulon, Lat. 43° 7′ N., Long. 5° 55′ E. Year 1847. I = 18°·6; ν = −4°·6.	Toulon, Year 1848. I = 18°·5; ν = 0°·7.	Toulon, Year 1853. I = 24°·8; ν = +12°·1.	Marseilles, Lat. 43° 18′ N.; Long. 5° 22′·5 E. Year 1850-51. I = 20°·1; ν = +10°·2.	Malta, Year 1871-72. Lat. 35° 55′ N.; Long. 14° 30′ E. I = 24°·6; ν = +12°·2.
S Speed of semi-diurnal $2(\gamma-\eta)$ = 30° per hour.	Mean solar ...	Diurnal	R_1=0·039 ft. ϵ_1=59°·7	0·013 ft. 31°·8	0·289 ft. 25°·1	0·42 cm. 39°·3	0·28 cm. 14°·9	0·29 cm. 5°·6	0·57 cm. 48°·1	0·10 in. 161°·6
		Semi-diurnal ...	R_2=0·145 ft. ϵ_2=291°·9	0·331 ft. 26°·0	0·492 ft. 195°·3	2·70 cm. 254°·3	2·74 cm. 246°·3	2·74 cm 249°·8	2·37 cm. 246°·6	1·44 in. 100°·2
		Quarter-diurnal.	R_4=0·004 ft. ϵ_4=71°·6	0·003 ft. 115°·6	0·007 ft. 64°·2	0·05 cm. 306°·1	0·05 cm. 259°·9	0·06 cm. 297°·7	0·08 cm. 276°·9	0·01 in. 36°·7
M Speed of semi-diurnal $2(\gamma-\sigma)$ = 28°·984 per hour.	Mean lunar ...	Diurnal	R_1=0·059 ft. ϵ_1=349°·2	0·008 ft. 174°·8	0·052 ft. 347°·7	0·11 cm. 223°·0	0·09 cm. 303°·7	0·47 cm. 12°·3	0·16 cm. 83°·1	0·07 in. 268°·4
		Semi-diurnal ...	R_2=0·154 ft. ϵ_2=287°·2	0·417 ft. 22°·8	1·530 ft. 155°·4	5·45 cm. 253°·9	7·04 cm. 244°·8	5·75 cm. 242°·4	6·87 cm. 229°·9	2·36 in. 94°·8
		Ter-diurnal ..	R_3=0·008 ft. ϵ_3=219°·1	0·016 ft. 166°·9	0·018 ft. 339°·7	0·15 cm. 166°·3	0·08 cm. 183°·4	0·13 cm. 174°·0	0·15 cm. 187°·7	0·02 in. 207°·7
		Quarter-diurnal.	R_4=0·009 ft. ϵ_4=262°·4	0·004 ft. 295°·2	0·066 ft. 352°·7	0·40 cm. 347°·5	0·61 cm. 24°·1	0·33 cm. 329°·0	0 62 cm. 3°·0	0·03 in. 354°·5
K Speeds γ and 2γ = 15°·041 and 30°·082 per hour.	Luni-solar declinational	Diurnal	R_1=0·611 ft. ϵ_1=304°·8	0·236 ft. 119°·7	0·350 ft. 29°·2	2·98 cm. 185°·3	3·23 cm. 185°·8	3·67 cm. 191°·9	3·29 cm. 187°·6	0·41 in. 51°·9
		Semi-diurnal ...	R_2=0·051 ft. ϵ_2=296°·6	0·124 ft. 21°·2	0·160 ft. 189°·4	0·68 cm. 251°·4	0·37 cm. 250°·6	0·80 cm. 272°·5	0·52 cm. 267°·0	0·38 in. 127°·3
O Speed $(\gamma-2\sigma)$ = 13°·943 per hour.	Lunar-declinational	Diurnal	R_1=0·430 ft. ϵ_1=286°·0	0·165 ft. 99°·4	0·481 ft. 13°·7	1·26 cm. 129°·8	1·68 cm. 124°·2	1·88 cm. 99°·0	1·85 cm. 97°·1	0·30 in. 72°·4
P Speed $(\gamma-2\eta)$ = 14°·959 per hour.	Solar-declinational	Diurnal	R_1=0·156 ft. ϵ_1=296°·7	0·056 ft. 131°·7	0·141 ft. 86°·7	1·25 cm. 178°·6	1·29 cm. 175°·8	1·25 cm. 179°·4	1·19 cm. 182°·4	0·13 in. 57°·9
L Speed $(2\gamma-\sigma-\varpi)$ = 29°·533 per hour.	Smaller lunar-elliptic	Semi-diurnal ..	R_2=0·011 ft. ϵ_2=250°·3	0·024 ft. 18°·7	0·060 ft. 132°·8	0·21 cm. 254°·1	0·33 cm. 232°·5	0·21 cm. 235°·3	0·22 cm. 274°·5	0·25 in. 116° 0
N Speed $(2\gamma-3\sigma+\varpi)$ = 28°435 per hour.	Greater lunar-elliptic	Semi-diurnal ..	R_2=0·040 ft. ϵ_2=341°·1	0·132 ft. 31°·9	0·332 ft. 128°·3	1·53 cm. 223°·4	1·61 cm. 234°·3	1·39 cm. 223°·1	1·36 cm. 222°·7	0·37 in. 116°·0
Q Speed $(\gamma-3\sigma+\varpi)$ = 13°·394 per hour.	Greater lunar-elliptic declinational	Diurnal	R_1=0·114 ft. ϵ_1=284°·9	0·028 ft. 79°·2	— —	0·38 cm. 42°·0	0·17 cm. 48°·8	0·19 cm. 32°·7	0·32 cm. 18°·4	0·08 in. 58°·5

The value of ϵ in each case above means the number of 360ths of its period which the corresponding tidal constituent has still to execute till its high-water from the instant when the ideal star crosses the meridian of the place. Thus if n denote the periodic speed of the particular tide in degrees per mean solar hour; its time of high-water is $\frac{\epsilon}{n}$, reckoned in mean solar hours after the transit of the ideal star.

In this definition, and in the table of results, the following notation is employed[1]:—

I to denote the mean inclination of the moon's orbit to the earth's equator during the time of the series of tidal observations included in each instance.
ν „ „ the mean right ascension of the ascending node of the moon's orbit on the earth's equator during the same time.
γ „ „ the angular velocity of the earth's rotation.
σ „ „ the mean angular velocity of the moon's revolution round the earth.
η „ „ the mean angular velocity of the earth round the sun.
ϖ „ „ the angular velocity of the progression of the moon's perigee.

"Speed" means the angular velocity of an arm revolving uniformly in the period of any particular tidal constituent; each angular velocity being reckoned in degrees per mean solar hour.

THE PHYSICAL FUNCTIONS OF LEAVES

An elaborate study on the above subject has lately been published by Prof. J. Boussingault, of Paris, in the *Annales de Chimie et de Physique* (vol. xiii. pp. 289-394); in which the phenomena of absorption and transpiration by leaves are treated at great length. Since the memorable experiments of Hales in 1727, recounted in his work on "Vegetable Statics," this branch of vegetable physiology has been rarely touched, and the carefully recorded observations of Boussingault. carried out with the best of modern scientific appliances, possess an unusual value.

The first point studied was the loss of water by transpiration from the leaves of plants under normal circumstances. For this purpose a healthy Jerusalem artichoke (*Helianthus tuberosus*) in a roomy flower-pot was chosen. The top of the pot was covered with a sheet of india-rubber, tightly inclosing the stem of the plant, and provided with an opening for the admission of water. The whole was then weighed, and the loss noted which ensued under various circumstances, by evaporation of water from the leaves, the plant receiving during the experiment weighed normal amounts of water. The total surface of the leaves of the plant (both upper and lower sides) was carefully estimated, and the result reckoned on the square metre. The averages of fourteen experiments showed that the artichoke lost hourly, for every square metre of foliage, the following amounts of water:—in the sunshine sixty-five grammes, in the shade eight grammes, during the night three grammes.

In the next place the question was investigated whether the absorption of water by plants, and the ascent of the sap is due to the force resulting from the transpiration on the surface of the leaves, or whether the roots exercise also a certain amount of force to this end. For this purpose experiments similar to the above were carried out with various plants, firstly under normal circumstances, secondly with the stem minus the roots immersed in water. As an instance we can take mint. The plant with roots showed an hourly evaporation per metre, of eighty-two grammes in the sunshine, and thirty-six in the shade. Under the same condition, without roots, the evaporation was sixteen and fifteen grammes respectively.

[1] The values of I and ν are given to facilitate comparison with the equilibrium values of the several tidal constituents, according to Tables I. and II. of the British Association Tidal Committee's Report of 1876.

The results show that the absorption of water by plants is determined in a great measure by the transpiration occurring in the leaves, that this is maintained for a certain length of time without the assistance of the roots, but cannot continue long, being dependent on the injective power possessed by the roots. The effects of pressure on the absorption was next examined, and it was found possible by this means for a time in certain cases to even more than replace the water lost by transpiration. For example: a chestnut branch dipped in water was found to transpire hourly per metre of foliage, 16 grammes. It was then inserted into a tube of water, and subjected to the pressure of a column of water $2\frac{1}{2}$ metres high. Under these conditions the evaporation mounted to 55 grammes per hour, and the branch at the end of five hours weighed more than at the commencement.

The general result of these experiments shows the mutual working of the various parts of the plant with reference to the phenomena of transpiration. The roots absorbing water from the soil by endosmose, direct it towards the stem. Whether the motive force here is injection by the roots or absorption resulting from the transpiration in the green parts of the plant, or a union of both, is a question still unsettled. The stem serves not only as a passage for the water to reach the leaves, but also as a reservoir to be drawn on during rapid evaporation. In the leaves the sap is concentrated by the transpiration, and the matters in solution enter into the cell formation, or, changed by the action of light, are distributed throughout the plant by the descending sap. The circulation would be quite similar to that in an animal, were it not for the irregularity. While the supply of water from the roots varies but slightly, the loss by evaporation from the leaves is subject to the greatest fluctuations, according to the temperature and hygroscopic condition of the surrounding air. During these periods the leaves draw on their stock of constitution water and the supply in the stem; and when both fail, the phenomenon of wilting ensues.

Numerous experiments were made on the difference in *evaporation during the day and during the night*. Those carried out with leaves of the grape vine gave the following hourly averages per square metre of foliage; in sunshine, 35 grammes; in shade, 11; during the night, 0·5. The trellis on which the vine was trained was 1 metre high and 38 metres long, and presented a surface of 138 square metres of foliage. In sunny weather this would lose by evaporation in the course of 24 hours, 48 kilogrammes of water, and nearly half of that amount during cloudy weather. To give an idea of the enormous amount of aqueous vapour dissipated by plants in the sunshine, calculation showed that an acre of beets could lose in the course of 24 hours between 8,000 and 9,000 kilogrammes. Another experiment made with a chestnut-tree 35 years old showed that it lost over 60 litres of water in the course of 24 hours. The structure of the leaf, however, containing 70-80 per cent. of water, and possessing a thickness frequently of but $\frac{1}{10}$th of a millimetre, would suggest the question why the evaporation is not much more rapid. The answer to this is found in the peculiar structure of the tissue forming the epidermis, designed especially to moderate the transpiration. In order to see the remarkable retentive power exercised by this epidermis, one can expose for a few hours to the sun two cactus leaves of the same superficies, one of which has been deprived of its epidermis. The evaporation in the latter case will be about fifteen times as rapid as in the other. It is the presence of a similar tissue forming the skin of fruits which prevents an otherwise rapid evaporation. For instance, an apple deprived of its skin loses 55 times as much water as a whole specimen in the same time. Losses by rapid evaporation lessen notably the physiological energy of leaves. Thus an oleander leaf containing 60 per cent. of water, when introduced into an atmo-

sphere containing carbonic acid, decomposed 16 c.c. of this gas; one containing 36 per cent. decomposed 11 c.c.; and one containing but 29 per cent. was without action.

A series of observations was made on the relative powers of *evaporation on the upper and lower sides of leaves.* They consisted in plucking two leaves of the same kind at the same moment, covering on the one the upper, on the other the lower side with melted tallow, and then noticing the loss of weight by evaporation in a given time. The average of the results showed that the proportion between the amounts of water evaporated on the upper and lower side of a dozen varieties of leaves was 1 : 4·3. In all cases the amount evaporated from the two exposed sides of two equal leaves was greater than from the entire surface of a similar leaf under the same circumstances.

A point of no small interest with regard to the physical function of leaves is that of their ability to replace the roots of a plant in serving as the agent of absorption. A variety of tests were undertaken to settle this question; among them the following:—A forked branch of lilac (Fig. 1) was so disposed that the one branch was immersed in water while the other was exposed to the ordinary atmospheric conditions. The superficies of foliage was the same on both branches. The transpiration from the surface of the leaves on the latter branch was the same as under normal circumstances, and after the lapse of two weeks the foliage was as fresh as at the commencement, showing that the submerged leaves were fully able to replace the roots in one of their functions. In an experiment with a beet in which one-half of the leaves were in water and one-half in the air, communication being maintained by means of the root, the free portion of the leaves wilted in the course of a day, the neck of the root apparently not offering a sufficient means of communication with the submerged leaves. A grapevine shoot half plunged in water (Fig. 2) maintained a normal evaporation in the free foliage, and remained fresh for over a month. An oleander shoot under similar conditions maintained its normal appearance for four months. With the artichoke it was found necessary that the surface of the leaves beneath the water should be four times that of the leaves above.

FIG. 1.

Closely bordering on this question is another which has excited much dispute, viz., the ability of leaves to draw water from the surrounding air or by immersion, after having suffered losses by transpiration. Prof. Boussingault's numerous experiments show that leaves, after having been exposed to influences causing a rapid evaporation, are able to absorb water rapidly on immersion, and even from an atmosphere saturated with aqueous vapour. There is, however, in both cases no absorption unless the leaves have lost a portion of their water of constitution, *i.e.*, that which is essential to their normal existence. Thus, a wilted branch of periwinkle weighing 4·0 grammes, after remaining in an atmosphere saturated with aqueous vapour for a day and a half, weighed 4·2 grammes, and after twelve hours' immersion in water 9·4 grammes.

The last function of leaves studied by Prof. Boussingault is their ability to absorb solutions of mineral matter, *i.e.*, perform another of the ordinary duties of the roots. For this purpose a solution of gypsum containing $\frac{2}{1000}$ of solid matter was used. Drops of this solution were placed on the leaves of a great variety of plants—under conditions favouring absorption, as in the experiments just described—and protected from evaporation by superincumbent watch-glasses with greased edges (Fig. 3).

FIG. 2

In most instances the drops were absorbed entirely, leaving no traces of the mineral matter; in some cases a slight residue was left, which the addition of a minute quantity of water caused to disappear. As in the case of pure water, the under side of the leaves absorbed much more rapidly than the upper side. Solutions of sulphate and nitrate of potassium gave quite similar results; the absorption of solutions of chloride of sodium and nitrate of ammonium was not so perfect. These results would tend to show that the foliage of a plant is able to supply it with perhaps no small portion of its saline constituents by means of the ammoniacal salts formed in the air, and the alkaline and earthy salts suspended there which are deposited on the surface of the leaves by rain and dew.

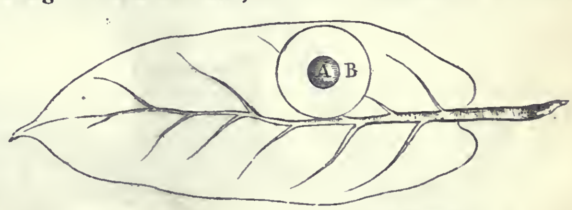


FIG. 3.—A, drop of solution; B, watch glass.

T. H. N.

EDISON'S INVENTIONS

THE fertility of Mr. Edison's inventive genius has frequently been referred to recently, though the attractive and popular nature of the phonograph has had the effect of throwing some almost equally important inventions into the shade. We propose, with the aid of the *Scientific American*, copies of which have been sent us by Mr. Edison, to draw attention to a few of these other inventions; the illustrations which we are able to give have also been kindly sent us by Mr. Edison himself.

Mr. Edison's laboratory, in size and external appearance, resembles a country church. The interior, however, is not so church-like. The first apartment is a reception room, on the right of which is the private office, containing a large library of scientific works. Beyond these there is a large room containing materials and a number of glass cases filled with physical and chemical apparatus.

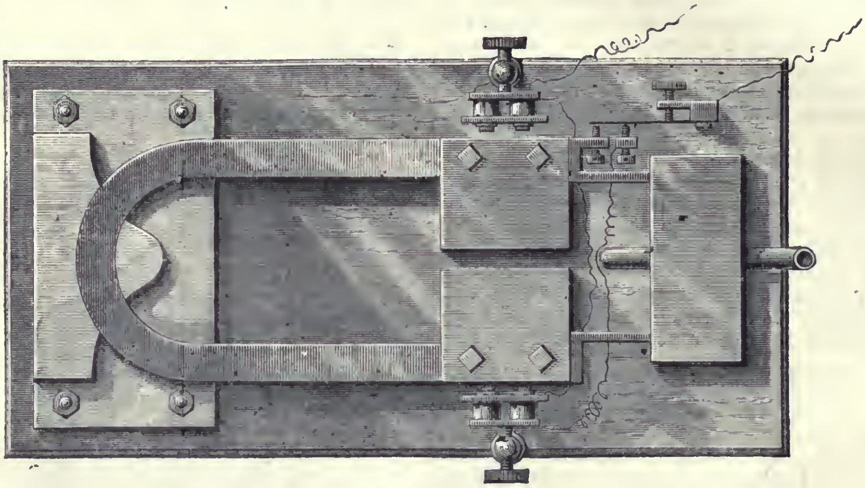


FIG. 1.—Edison's Harmonic Engine.

The machine shop at the rear is furnished with the best of machinery and tools, and is kept constantly in operation in carrying out the plans of Mr. Edison. On the second floor there is a single spacious room, which is the laboratory proper. Here, upon the walls, are shelves which are thickly studded with bottles, jars, and boxes, containing a multitude of substances, both common and rare. It is said to be a chronic habit of Mr. Edison to purchase every newly discovered substance, so that it will be at hand should it be required. Here also is the carbon relay, the progenitor of all existing carbon telephones, "microphones," and other instruments dependent on the changeable conductivity of carbon under a varying pressure.

One of the earlier of Mr. Edison's inventions is the electro-motograph; a telegraphic instrument in which

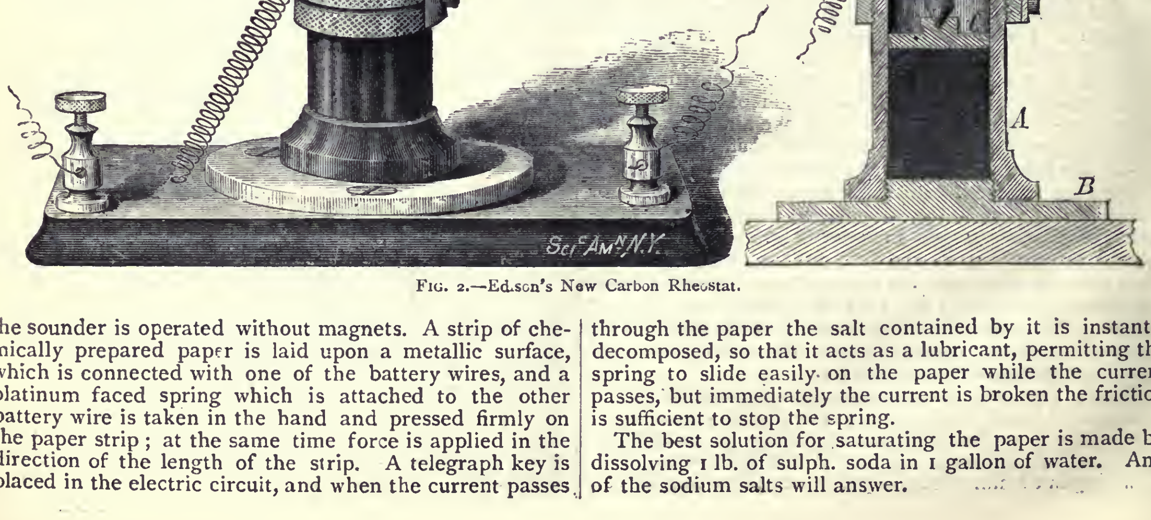


FIG. 2.—Edison's New Carbon Rheostat.

the sounder is operated without magnets. A strip of chemically prepared paper is laid upon a metallic surface, which is connected with one of the battery wires, and a platinum faced spring which is attached to the other battery wire is taken in the hand and pressed firmly on the paper strip; at the same time force is applied in the direction of the length of the strip. A telegraph key is placed in the electric circuit, and when the current passes through the paper the salt contained by it is instantly decomposed, so that it acts as a lubricant, permitting the spring to slide easily on the paper while the current passes, but immediately the current is broken the friction is sufficient to stop the spring.

The best solution for saturating the paper is made by dissolving 1 lb. of sulph. soda in 1 gallon of water. Any of the sodium salts will answer.

Electricity as a motive power, until now, has been a comparative failure, as 90 per cent. of the battery has been wasted. Mr. Edison has devised a novel electrical machine which he calls the harmonic engine, in which 90 per cent. of the power is realised. With two small electro-magnets and three or four small battery cells, sufficient power is generated to drive a sewing machine or pump water for household purposes. This engine, which is represented in Fig. 1, consists of a fork $2\frac{1}{2}$ feet long, made of 2-inch square steel. The curved part of the fork is firmly keyed in a solid casting which is bolted to a suitable foundation, and to each arm of the fork is secured a 35 lb. weight. Outside of and near the end of each arm is placed a very small electro-magnet. These magnets are connected with each other, and with a commutator that is operated by one of the arms. The arms make thirty-five vibrations per second, the amplitude of which is $\frac{1}{8}$ inch. Small arms extend from the fork arms into a box containing a miniature pump having two pistons, one piston being attached to each arm. Each stroke of the pump raises a very small quantity of water, but this is compensated for by the rapidity of the strokes. Mr. Edison proposes to compress air with the harmonic engine, and use it as a motive agent for propelling sewing machines and other light machinery. The power must be taken from the fork arms so as not to affect the synchronism of their vibrations, otherwise the engine will not operate.

In quadruplex telegraphy it is vital to the working of the system to perfectly balance the electrical current. The common method of doing this is to employ a rheostat containing a great length of resistance wire, more or less of which may be thrown into or cut out of the electrical circuit by inserting or withdrawing plugs or keys. This operation often requires thirty minutes or more of time that is or might be very valuable. To remedy this difficulty

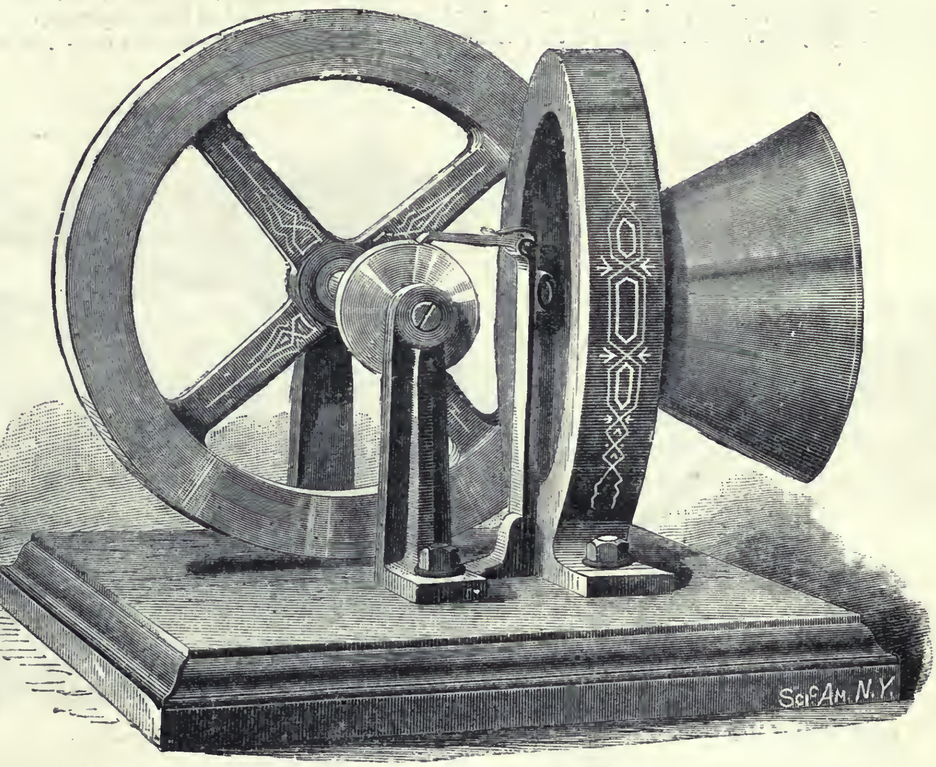


FIG. 3.—Edison's Phonometer.

Mr. Edison has devised the instrument represented in Fig. 2, 1 being a perspective view and 2 a vertical section. A hollow vulcanite cylinder A is screwed on a boss on the brass plate B. Fifty disks—cut from a piece of silk that has been saturated with sizing and well filled with fine plumbago and dried—are placed upon the boss of the plate B, and are surmounted by a plate C, having a central conical cavity in its upper surface. A pointed screw D passes through the cap E at the top of the cylinder A, and projects into the conical cavity in the plate C. The screw is provided with a disk F, having a knife edge periphery which extends to the scale G, and serves as an index to show the degree of compression to which the silk disks are subjected. The instrument is placed in the circuit by connecting the cap E with one end of the battery wire and the plate B with the other end.

The principle of the instrument is identical with that of Mr. Edison's carbon telephone. The compression of the series of disks increases conductivity; a diminution of pressure increases the resistance. Any degree of resistance within the scope of the instrument may be had by turning the screw one way or the other. In this instrument the resistance may be varied from 400 to 6,000 ohms, and any amount of resistance may be had by increasing the number of silk disks.

The thermo-telephone, although at present without special practical value, is certainly a novelty. It consists of a thermopile having placed in its collecting funnel a hard rubber disc. A sound made in front of this disc is heard in a receiving telephone connected with the thermopile.

The *rationale* of this is at once apparent when a strip of hard rubber is placed against the lips and bent, so that the strip will be alternately concave and convex. The difference in temperature is very perceptible, the convex surface being cold and the concave surface warm, and, however rapid the vibrations which render the surfaces alternately convex and concave, the result is the same.

Mr. Edison, in his telephone and phonograph experiments, discovered that the vibrations of the vocal chords were capable of producing considerable mechanical effect. Acting on this hint, he began experiments on a phonometer, or instrument for measuring the mechanical force of sound-waves produced by the human voice. In the course of these experiments he constructed the machine shown in Fig. 3, which exhibits the dynamic force of the voice. The machine has a diaphragm and mouth-piece similar to a phonograph. A spring which is secured to the bed piece rests on a piece of rubber tubing placed against the diaphragm. This spring carries a pawl that acts on a ratchet or roughened wheel on the fly-wheel shaft. A sound made in the mouth-piece creates vibrations in the diaphragm, which are sufficient to propel the fly-wheel with considerable velocity. It requires a surprising amount of pressure on the fly-wheel shaft to stop the machine while a continuous sound is made in the mouth-piece.

The speaking trumpet, which, for two centuries at least, has been employed to direct sound so that it may be heard over a long distance, is much used at sea, and is often employed on land to direct vocal sounds so that they may be heard above other sounds. It is tolerably certain that the speaking trumpet is of modern origin, and that it is the invention of Samuel Moreland, 1670.

Kircher, in his "Ars Magna et Umbra" and in his "Phonurgia," mentions a kind of gigantic speaking-trumpet, described as the horn of Alexander. According to Kircher, this horn enabled Alexander the Great to call his soldiers from a distance of ten miles. The diameter of the ring must have been eight feet, and Kircher conjectures that it was mounted on three poles.

Late in the last century Prof. Huth, a German, made a model of the horn, and found that it served as a powerful speaking-trumpet, but we are considerably in doubt as to the distance through which sounds can be projected through such an instrument.

The ear-trumpet, which is the counterpart of the speaking-trumpet, has been made in various forms during the last two centuries, but no form yet devised has any advantage over a plain conical tube with a bell-shaped or flaring mouth.

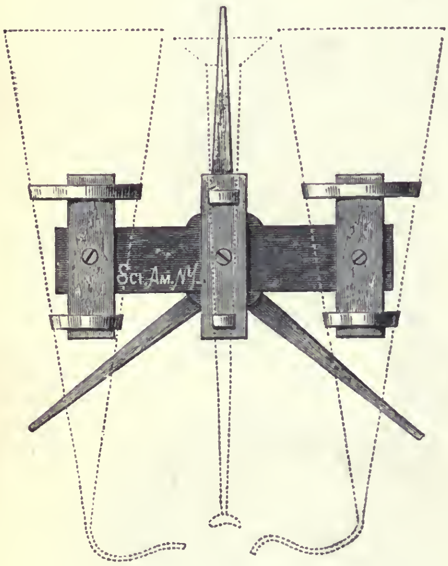


FIG. 4.—Plan of Megaphone.

FIG. 5.—Edison's Megaphone.

Mr. Edison, in his researches on sound, has made many curious experiments, one of the most interesting of which is that of conversing through a distance of $1\frac{1}{2}$ to 2 miles with no other apparatus than a few paper funnels. These funnels constitute the megaphone, an instrument wonderful both for its simplicity and effectiveness. In the plan, Fig. 4, the details of construction are clearly shown, and Fig. 5 represents the instrument as it stands on the balcony of Mr. Edison's laboratory. A mile and a half distant there is another instrument exactly like the one in Fig. 5.

The two larger funnels are 6 feet 8 inches long and $27\frac{1}{2}$ inches in diameter at the larger end. These funnels are each provided with a flexible ear-tube, the end of which is placed in the ear. The speaking-trumpet in the middle does not differ materially from the ordinary ones. It is a little longer and has a larger bell mouth. With this instrument conversation can be readily carried on through a distance of $1\frac{1}{2}$ to 2 miles. A low whisper, uttered without using the speaking-trumpet, is distinctly audible at a thousand feet, and walking through grass and weeds may be heard at a much greater distance.

These statements, it must be understood, are given on the authority of the *Scientific American*, but some experiments lately made with a paper megaphone by Prof. Barrett lend them strong support

COLOUR BLINDNESS IN RELATION TO THE HOMERIC EXPRESSIONS FOR COLOUR

IN an article on "The Colour Sense" in the number of the *Nineteenth Century* for October last, Mr. Gladstone points out certain peculiarities, very remarkable and very difficult to account for, in the expressions for colour used by Homer. "Although," he says, "this writer has used light in its various forms for his purposes with perhaps greater splendour and effect than any other poet, yet the colour adjectives and colour descriptions of the poems are not only imperfect but highly

ambiguous and confused." And again—"We find that his sense of colour was not only narrow, but also vague, and wanting in discrimination."

The article is an expansion of a chapter in the same author's "Studies on Homer and the Homeric Age," published in 1858 (vol. iii. page 457), from which the proposition is quoted; "That Homer's perception of the prismatic colours, or colours of the rainbow, and *à fortiori* of their compounds, were, as a general rule, vague and indeterminate." Mr. Gladstone gives many examples illustrating these opinions, and by powerful and ingenious reasoning, he endeavours to establish from them the general conclusion that "the organ of colour was but partially developed among the Greeks of the heroic age."

I have no intention of disputing this conclusion, in favour of which no doubt much may be said, but I think it may be worth while, on scientific grounds, to point out how remarkably the anomalies and imperfections in question correspond with those that might be expected to arise if the writer were assumed to be *colour blind*.

Mr. Gladstone makes some allusion to the possibility of a defect in the poet's organisation; but it appears to me that, probably from the facts connected with colour blindness not being fully known to him, he hardly gives this point the attention it deserves. In his earlier essay he expresses the opinion that such a supposition "cannot be resorted to, when we bear in mind Homer's intense feeling for form, and when we observe his effective and powerful handling of the ideas of light and dark." From this remark it is clear that Mr. Gladstone was unaware of the fact that colour blindness can, and usually does, coexist with a perfect feeling for form, and with as vivid ideas of light and dark as are possessed by the normal-eyed. In the later article he mentions the defect more specifically, and refers to Prof. Wilson's book on the subject (published in 1855), but he does not add anything to the argument, and appears to leave the point still open to discussion, when further data can be supplied.

In 1856 I presented to the Royal Society a paper on Colour Blindness, which was afterwards published in the *Philosophical Transactions*, vol. 149, p. 323. Its object was to state my own case (which happens to be one of the most decided on record) and to show that the general phenomena attending this defect of vision were more simple, uniform, and consistent than was generally supposed.

Before I wrote that paper the general impression was that, although there were certain broad particulars in which the sensations of different colour-blind patients agreed, yet there were many varieties of the defect, differing much in character as well as in severity, each being denoted by its own peculiar symptoms, and each, therefore, requiring special classification. Prof. Elie Wartmann, of Lausanne, whose paper on the subject was published in England in 1846, held the opinion that there were as many varieties of the defect as of individuals affected with it, so that no classification was possible. Dr. Wilson did not go so far as this, but he considered the cases as varying much in degree, and he inclined strongly to the opinion that the most severe form of the defect was very rare.

To illustrate the great variety of mistakes made by the colour blind, and to show the confusion resulting therefrom, I gave the following list of "symptoms" which had been observed in various patients, and which were all combined in my own case:—

Blue and yellow are always perfectly distinguished, even in their lighter or darker tones, and are never confounded with each other.

Only these two colours are seen in the solar spectrum, the blue corresponding to the more and the yellow to the less refrangible rays. The red space is seen as yellow.

Red, though frequently identified in certain cases, is often confounded with black, white, or grey, with orange, with yellow, with green, with brown, with blue, and with violet. Crimson and pink have no relation to the red of vermilion.

Green is a colour most perplexing to the patient, who cannot be said generally to manifest any definite sensation about it at all. It is confounded not only with red, but also with black, white, or grey, with orange, with yellow, with blue, with violet, and with brown.

Orange is confounded with yellow; violet is confounded with black or grey, and with blue.

No wonder that philosophers should have despaired of finding any reasonably simple diagnosis for such a heterogeneous mass of symptoms; it was, however, my object to show this could be done. By a long and careful study of my own sensations, aided by a masterly suggestion of Sir John Herschel's that had been published shortly before, I found, in the first place, that notwithstanding the apparent variety of the symptoms in different persons, the defect was uniform, or nearly so, in all; and in the second place, that in spite of the apparent complexity of the phenomena, this defect was of a very simple character. I believe the explanations I gave have been generally accepted, and subsequent experience has amply confirmed them.

As few people have a clear understanding what Colour Blindness really means, and as without such an understanding it would be impossible to make my remarks intelligible, I must ask leave to describe the defect as briefly as I can, referring to the *Philosophical Transactions* for fuller details and demonstrations.

In the first place we see white and black, and their intermediate or compound grey (provided they are free from alloy with other colours), precisely as others do.

Secondly, there are two colours properly so called, namely, yellow and blue, which also, if unalloyed, we see, so far as can be ascertained, in the normal manner.

But these two are the *only* colours of which we have any sensation; and hence the defect has been given by Sir John Herschel the scientific name of *dichromic vision*.

But now comes the difficulty of the explanation. It may naturally be asked: Do we not *see* objects of other colours, such as roses, grass, violets, oranges, and so on? And if we do see them, what do they look like? The answer is that we do see all such things, but that they do not give us the colour sensations correctly belonging to them; their colours appear to us varieties of the other colour sensations which we are able to receive.

This will be best explained by examples. Take first the colour red. A soldier's coat or a stick of red sealing-wax conveys to me a very positive sensation of colour, by which I am perfectly able to identify, in a great number of instances, bodies of this hue. If, therefore, the investigation of my experience ended here, there would be no reason to consider me blind to red, or as having any grave defect in my vision regarding it. But when I examine more closely what I really do see, I am obliged to come to the conclusion that the sensation I perceive is not one that I can identify separately, but is simply a modification of one of my other sensations, namely, *yellow*. It is, in fact, a yellow shaded with black or grey—a darkened yellow, or what I may call yellow brown. I find that all the most common hues of red correspond with this description, and in proportion as they are more scarlet or more tending towards orange, the yellow I see is more vivid. The explanation, I suppose, is, that none of such reds are pure, they are combinations of red with yellow; so that I see the yellow element of the combination, while the true red element is invisible to me as a colour, and acts only as a darkening shade.

I obtain a further proof of this by the change of sensation when the hue of red is altered. I find that as the colour approaches crimson the yellow element becomes fainter and the darkening shade more powerful, until very

soon the yellow disappears, and nothing but a grey or colourless hue is presented to my eye, although the colour is still a positive and powerful red to the normal-eyed. So that there is a hue of red which as a colour is absolutely invisible to the colour-blind.

If I go on beyond this point and take reds that pass from crimson towards the hue called lake, I see my other colour come in, a faint blue, which increases till violet is reached, when it becomes more decided.

Violet is understood, I believe, to be a compound of blue with red, and accordingly, the red element being invisible to the colour-blind, violet hues generally appear to them only as darkened blue. There are, however, examples where, from the red being very strong, the blue appears to lose its effect, and the impression given is colourless, black, or grey. They correspond, in fact, with the neutral red before described, although still called violet or purple by the normal-eyed. This latter effect is much enhanced under the artificial light of gas or candles.

A similar explanation will apply to orange, a combination of red and yellow, in which the yellow only is perceived.

The appearance of green to the colour-blind corresponds exactly to that of red; green in its true aspect is invisible to them, and consequently when neutral, *i.e.*, unmixed with any other colour, it presents to their eyes he appearance of grey. When, however, it is mixed with yellow (and most of the greens in nature are yellow greens) they see the yellow only, but diluted or darkened by the invisible green element. And in the less frequent cases where the green is mixed with blue, they see the blue only, in like manner.

It may now easily be understood how it is that so simple a defect of vision gives rise to so complex a series of symptoms as those already described.

Take first the colour red. If it is a scarlet variety, as the majority of reds are, presenting the appearance of yellow to the colour blind, they may naturally confound it with the latter colour, as well as with orange, with yellow green, and with brown, all which cause to them the same sensation. If, on the other hand, the red contains a predominance of blue, it may be confounded, on the same principle, with blue or violet. If it is a neutral red, lying between the two, it will be confounded with black or grey. A pale pink, though very distinctly coloured to the normal-eyed, often offers so little colour to the colour-blind as to be mistaken for white, or very light grey.

The same explanation will apply to green. Its yellow varieties may be confounded with red, orange, yellow, and brown, its blue varieties with blue and violet, and its neutral hue with black or grey, or if very pale, with white.

The confusion of orange with yellow, and of violet with blue, black, and grey, have been already sufficiently explained.

I must now go on to show how the hypothesis of Colour Blindness may serve to explain or account for the anomalies in Homer's descriptions of colour. It is out of my province to meddle with any questions of classical scholarship, I adopt all Mr. Gladstone's critical nterpretations, and I suppose I need not desire a higher authority. It will be convenient to refer to his two essays indiscriminately, using the letter N. for the article in the *Nineteenth Century*, and H. for the chapter in the Studies on Homer.

Before going into any detail I may notice the general classification which Mr. Gladstone (H. 458, &c.) has given of the Homeric peculiarities, and it is impossible not to see, at a glance, how exactly this corresponds with what might be expected from the colour-blind.

I. The paucity of Homer's colours. Excluding black and white, Mr. Gladstone reduces them to four, intimating, however, in the following sentence, that even this number is too many.

The colour-blind list is limited to two.

II. The use of the same word to denote not only different hues or tints of the same colour, but colours which, according to the normal-eyed, are essentially different.

This is the *shibboleth* of the colour-blind defect.

III. The description of the same object under epithets of colour fundamentally disagreeing one from the other.

Mr. Gladstone only names three instances, referring to iron, to the dragon, and to a thunderbolt, none of which appear to me to be very conclusive; but since a colour-blind person may, with perfect correctness according to his own sensations, describe grass as either green, red, orange, or yellow, the defect will amply account for this peculiarity.

IV. The vast predominance of black and white; and

V. The slight use of colour, as compared with other elements of beauty for the purpose of poetic effect, and its absence in certain cases where we might confidently expect to find it.

Nothing can be more natural than this, in the case of a writer to whom the great mass of colours in nature are invisible.

When we go into detail, and review the instances Mr. Gladstone has given of Homer's particular applications of colour adjectives, the correspondence with the colour-blind hypothesis becomes much closer and more conclusive.

As a general preliminary, let us ask what such a writer, if colour-blind, might be expected to do? How would he be likely to use his epithets of colour?

In the first place he would be certain to use them incorrectly, that is, in a way not consistent with the ideas ordinarily attached to them. He must adopt words in ordinary use; but he can form no proper idea of their meaning, and, as the objects they apply to appear under totally different aspects to him, his use of the words must necessarily be confused and often inappropriate. This may be particularly looked for in regard to red and green; terms which, although conveying ideas so positive and unmistakable to the normal-eyed, have to the colour-blind no definite signification at all.

But secondly, there ought to be a certain consistency and intelligibility in his use of the terms when viewed in regard to his own sensations; and if my view of the simple and uniform character of the disease be correct, we ought to be able, knowing what these sensations are, to form a tolerable idea how colour epithets would be applied so as to accord with them.

Omitting all considerations of white, black, or grey, and confining ourselves to colour proper, we know that every coloured object in nature presents to the colour-blind person one of two sensations, either that of yellow or that of blue, modified in tone or shade, as I have described. These are positively distinct from each other, and are never confounded. We therefore ought to expect that the colour-epithets used by such a person should be found capable of being arranged in two groups or classes, one corresponding to the yellow sensation, the other to the blue sensation. The various words in either group may have very different meanings to the normal-eyed, but if they all convey the same sensation to the colour-blind person, he may be expected to use them indiscriminately. And, moreover, he ought never to apply a word belonging to one class or group, to an object belonging to the other group; if he did he would fail in the consistency to his own sensations which I am now insisting on. For example, if a person applied the word "orange," a word belonging to the yellow group, to lapis lazuli, or the violet flower, which, on the colour-blind

hypothesis ought to convey to him the idea of blue, the error, although not appearing worse to the normal-eyed than calling grass red, would be altogether inconsistent with the proper colour-blind sensations, and would prove that such a person had not dichromic vision in the sense here intended.[1]

It may further be remarked that, as the colour-blind person finds the yellow sensation much predominating in what he sees, and as he will have observed that it corresponds to a larger number of ordinary colour-words than the blue sensation, his vocabulary for this group will be naturally more copious than for the opposite one.

William Pole

(*To be continued.*)

[1] Some errors might, however, legitimately arise in the use of the words for red and green, from the fact that some hues of these colours give yellow sensations, while others give blue sensations.

CHEMICAL NOTES

Estimation of Mercury.—Prof. Clark, of Cincinnati, describes (*Ber. d. deutsch. chem. Gesell.* xi. 1,409) an interesting application of electrolytic decomposition for the purposes of analytical chemistry, viz., in the separation and estimation of mercury. The solution of the mercury salt, acidulated with sulphuric acid, is placed in a platinum vessel, connected with the zinc pole of a Bunsen bichromate battery of six cells. A piece of platinum foil in connection with the carbon pole is dipped into the liquid, and the decomposition commences at once. At first a mercurous salt is precipitated. This is reduced gradually, until, in the course of an hour, it is completely changed into the metal, which requires simply to be separated from the solution, washed, dried, and weighed. Similar methods for the estimation of zinc, nickel, and copper have for some years been in use.

Separation of Antimony and Arsenic.—One of the problems in analytical chemistry awaiting solution is a satisfactory separation of arsenic from antimony. In the last number of Liebig's *Annalen* (vol. 192) Prof. Bunsen presents a new method intended to supersede that hitherto employed, which was discovered by him a number of years since, depending on the treatment of the combined sulphides with sulphurous acid. In the new process the sulphides are dissolved in potash and subjected to the action of chlorine. A quantity of a saturated aqueous solution of sulphuretted hydrogen, sufficient to precipitate the antimony, as Sb_2S_5, is then added, and in the filtrate the arsenic acid is precipitated on heating as As_2S_5 by a long-continued stream of H_2S.

Specific Heats of Mercury and Iron.—O. Pettersson and E. Hedelius have recently made careful determinations of the specific heat of mercury and iron in the following manner (*Ofvers. f. Vetensk. Förhandl.*, 37, p. 35):—A piece of wrought iron was heated in an air bath to 26°, and then plunged in baths containing weighed quantities of mercury and water at 0°. The resultant temperatures gave the specific heat of iron as referred, firstly to water, and secondly to mercury, and the division of the first value by the second yielded the specific heat of mercury referred to water. The averages derived from twenty experiments give for the average specific heat of wrought iron between 4° and 27°, 0·10808; and for the specific heat of mercury between 0° and 5°, 0·033266. The authors find that the specific heat of mercury suffers but slight alterations between 0° and 100°.

Latent Heat of Water at Temperatures below 0° C.—O. Pettersson (*Ofvers. f. Vetensk. Förhandl.*, 37, p. 53) has lately determined the latent heat of water at temperatures below 0°. For this purpose thin tubes containing water were placed in a mercury calorimeter, cooled to certain temperatures below 0°, and congelation was induced by the insertion of a snow crystal. The latent heat of water at 0° according to Regnault is 79·25. The results obtained by Pettersson at lower temperatures are as follows: − 2·80°, 77·71; − 4·995°, 76·60; − 6·28°, 75·94; − 6·50°, 76·03; − 6·62°, 75·99; all of them coinciding closely with the estimations of the theoretical formula $\frac{\delta r}{\delta T} = c - h$, where r represents the latent heat of fusion, T the absolute temperature, and $c - h$ the difference between the specific heats of the solid and liquid body. Experiments were likewise made with sea water containing 3·536 per cent. of solid matter, and freezing at − 9°. At this temperature pure water would possess a latent heat of 75; the sea water possessed on the contrary but 54, showing that the above proportion of saline matter was sufficient to cause a diminution of 28 per cent. in the latent heat.

Preparation of Salts of Nitrous Oxide.—In the *Journal* of the Chemical Society (clxxxix.) Mr. A. E. Menké describes some of the above salts. In analysing a sample of cast iron an experiment was made attempting the conversion of the phosphorus contained in it by fusion with nitre and sodium carbonate, into an alkaline phosphate. During the operation a bulky yellow precipitate was obtained which proved to be, not a phosphate, but identical with the body obtained by Dr. Divers in the action of sodium amalgam on sodium nitrate. The analysis of the silver salt gave a mean percentage of 78·09 Ag, agreeing therefore with the formula AgNO, which requires 78·26 per cent. Ag. The salt may also be obtained by the simple fusion of iron filings with nitre, the best heat to employ being that of a charcoal furnace. The analysis of the sodium salt obtained by the fusion of iron filings with sodium nitrate gave numbers closely agreeing with the formula $NaNO + 3H_2O$. The substitution of zinc for iron filings failed to produce the body. On acting on the silver salt with ethyl iodide the silver is converted into iodide, and on fractionating the distillate evidence of the formation of an ethereal salt of low boiling point is obtained.

On Manganese Tetrachloride ($MnCl_4$).—Some doubt still existing with regard to the decomposition of manganese oxides higher than the dioxide MnO_2, Mr. W. Fisher has recently made experiments bearing upon this point. The oxides employed are the sesquioxide, Mn_2O_3, and the red oxide of manganese, Mn_3O_4. The analyses of the liquids obtained by the action of the acid on the different oxides were made by decomposing the freshly-prepared solutions with potassium iodide, and then titrating the amount of iodine liberated in each case with sodium hyposulphite. From his experiments Mr. Fisher finds that the higher oxides when treated with excess of acid give a brown liquid containing a highly chlorinated manganese compound easily resolved into manganous chloride and free chlorine, and on dilution with water yielding manganese binoxide in both instances. The solutions appear to be identical, probably containing $MnCl_4$ in each. Under the conditions of the experiment the corresponding chlorides, Mn_2Cl_6 and Mn_2Cl_8, do not appear to be formed from their corresponding oxides, nor do they appear as products of the partial dechlorination of the tetrachloride. The action of the acid on the two oxides the author considers may be represented by the formulæ—

$$Mn_2O_3 + 6HCl = MnCl_2 + MnCl_4 + 3H_2O,$$
$$Mn_3O_4 + 8HCl = 2MnCl_2 + MnCl_4 + 4H_2O,$$

and as a large excess of acid or alkaline chloride renders $MnCl_4$ more stable, he thinks it probable that this body may exist in a form analogous to chloroplatinic acid.

Spontaneous Ignition of Hydrogen by finely-divided Zinc.—In dissolving zinc in hydrochloric acid P. W. Hofmann has observed explosions on the surface

of the liquid in the vessels employed. These phenomena he describes in a paper in the *Chem. Centr.*, 1878, 351, and explains them by the supposition that the gas in its evolution throws up small portions of zinc, rendered porous by the action of the acid, and that these finely-divided particles coming in contact with the air act like spongy platinum, causing the gaseous mixture to explode. The spontaneous ignition of hydrogen has been observed by others, but no satisfactory explanation has been given of the action.

METEOROLOGICAL NOTES

CAPT. HOFFMEYER has made an original and highly important contribution to our knowledge of the distribution of atmospheric pressure in winter over the North Atlantic, and its influence on the climate of Europe, in the last published number of the *Journal of the Meteorological Society of Austria* (October 15). The contribution takes the form of a rectification of Buchan's isobaric charts for this part of the globe, and, by a most ingenious and able method of investigation, entirely his own, Hoffmeyer conclusively shows that Greenland and Iceland exert a powerful influence on the distribution of atmospheric pressure not hitherto properly recognised, resulting in the mean minimum of pressure being localised distinctly to the south-west of Iceland—a minimum accompanied with two subordinate minima, one in Davis Straits and the other in the Arctic Ocean, mid-way between Jan Mayen and the Lofoden Isles. Four typical charts are also given, showing the actual mean pressure of as many individual winter months, from which it is plain that one or other of these three minima plays the chief roll, the other two being, for the time, subordinate; and that, according as the one or the other of these minima of pressure predominates, so is the character of the weather, as regards its mildness or severity, of the winter of the regions surrounding the North Atlantic, determined.

THE *Report of the Royal Meteorological Institute of Prussia for* 1877 has been received. It is the thirtieth Report, and like all the foregoing Reports, is published by the Royal Statistical Bureau, Berlin, with which the Institute, since its establishment in 1848, has stood in close and uninterrupted connection. Important changes are in contemplation, the most vital of which are the severance of the connection between the Institute and the Statistical Bureau, and the establishment of an independent central direction for meteorology. The Bureau has done a graceful act in presenting with the Report a highly characteristic portrait of the veteran meteorologist and physicist Dove, who has directed the affairs of the Institute since December 9, 1848, and who, by the number and thoroughness of his writings and their breadth of view, deserves of all men to be styled the father of meteorology.

AMONG the separate papers incorporated in this Report is a discussion by Dr. Hellmann of the observations of cloud at Crefeld, being in continuation of the author's researches into the cloud-covering of the sky as influenced by the hour of the day, the season of the year, and geographical situation. The daily maximum occurs at Crefeld, about, or a little before sunrise, from September to April, whereas, from May to August, the maximum is from about 11 A.M. to 3 P.M. The monthly maximum, which holds also for all hours of the day, is December, whilst the month with clearest skies is September. The barometric observations at Berlin for the past thirty years are carefully discussed by Prof. Arndt, from the general results of which it appears that the great summer depression of the barometer which is so characteristic a feature of the climatology of the Europeo-Asiatic Continent, is not shown at Berlin, it being a little to eastwards of Berlin, to which the limits of the western outskirts of this wide-spread barometric depression extend.

THE Dutch Meteorological Institute has issued Wind Charts of the North Atlantic, Series I., including the six months from December to May. The region covered by the charts lies between 51—30° N. lat. and 4—52° W. long., and between 30—8° N. lat., and 13—39° W. long. The frequency of the different winds is graphically shown by radii, the length of each being proportional to the frequency of the particular wind it represents. Instead of grouping the observations into 5° squares and into seasons as has been generally done, Dr. Buys Ballot has presented the facts on the charts for each 1° square and for each month, the object being to lay down the geographical position of the winds of this region, so important to navigators as well as men of science, with the closest approach to truth and least possible admixture of hypothesis.

IN a circular letter addressed to the Permanent Committee on Meteorology, Prof. Hildberandsson invites the co-operation of all meteorologists to the carrying out of a more systematic observation of the upper currents of the atmosphere than has yet been attempted. Hitherto the observation of the upper clouds and the directions in which they march, has been confined to isolated observers whose services were enlisted through the enthusiasm of individual meteorologists. But fragmentary and scattered though the observations have necessarily been, the results fairly deduced from them are of so important a nature from their bearings on the great problem of atmospheric circulation, that we have no hesitation in giving our hearty support to Prof. Hildebrandsson's proposal that the meteorological societies and observatories make observations of the movements of clouds, chiefly of the upper clouds, part and parcel of their regular observations, and that the results regularly appear in their publications.

THE great storm of September 15-16, so widely and so severely felt, deserves to be specially noticed on account of the low barometers accompanying it, which were not only exceptionally low for the season but even exceptionally low for any season of the year. From the observations made at the stations of the Scottish Meteorological Society in the north and north-west it is seen that at Thorshavn, Farö, the barometer at 32° and sea-level fell at 9 P.M. of the 15th to 28·058 inches, being the lowest point to which it fell, and about that time the wind shifted from south-east to north-west. At Stornway the barometer fell to its lowest, 28·400 inches at 7 P.M., or two hours earlier than in Farö; at the same hour it fell to the lowest point, 28·457 inches, at Monach lighthouse, the wind at this time attaining its maximum violence during the storm; at Sandwick to 28·404 at midnight; and at North Unst lighthouse it fell during the night to 28·305 inches. Heavy showers with thunder and lightning and heavy continued rain occurred in the North-west Highlands, nearly an inch of rain falling in less than an hour at Portree on the morning of the 15th, and 10·57 inches at Glenquoich during the six days beginning with the 14th.

GEOGRAPHICAL NOTES

THE Gothenburg *Handels Tidning*, of the 16th inst., contains a telegram from Irkutsk, addressed to Mr. Oscar Dickson by Prof. Nordenskjöld, announcing that he had reached the mouth of the Lena on August 27, after having passed Cape Chelyuskin, without meeting with any noteworthy obstacle from ice, and that the voyage would be continued towards Behring's Straits with the highest hopes of success. It is probable that Prof. Nordenskjöld's anticipations have been by this time realised, that the *Vega* has reached Behring's Straits, and thus successfully accomplished the North-East Passage. News has also arrived that the *Lena*, a small steamer which accompanied the *Vega*, has ascended the

river of the same name, having arrived at the town of Yakutsk on September 22.

DR. GERHARD ROHLFS had finally fixed his departure from Malta for the 20th instant. He will be accompanied by two Austrian travellers, Dr. Hecker, and Herr von Csillagh. The party will first go to Tripolis, and direct their principal efforts to the investigation of the Shari River, and of the sources of the Binue and Congo Rivers.

CAPT. TYSON, having arrived at St. John's, Newfoundland, has sent a telegram to Washington stating that his voyage has been quite successful, and every part of the task imposed upon him has been accomplished. Capt. Tyson learns with deep regret that all has been rendered useless by the postponement of the definitive expedition.

ON reference to a map of China lately published by Mr. Stanford for the China Inland Mission, it will be seen that the missionaries of that body have for some years past been emulating their Roman Catholic brethren in the energy with which they have pushed their way into various parts of the empire. One of the most remarkable journeys, an account of which has but lately become available, was performed principally on foot, by Mr. John McCarthy, who left Shanghai in December, 1876, and reached Bhamô on August 26, 1877, having travelled a distance, including detours, of about 3,000 miles. Mr. McCarthy, though taking a somewhat different course, made the same journey as the Grosvenor Mission, and he claims to be the first non-official traveller who has thus traversed the entire width of the empire, and crossed the Kah-chen hills to Bhamô. Wearing the Chinese dress, and having nothing strange or novel with him, he was able to move along without any difficulty through the various towns and cities, and it is certainly worthy of note that throughout the whole of his long and hazardous journey he was not once obliged to appeal to an officer for help of any kind, and in no case did any officer put an obstacle in his way. The country, as far as the great commercial mart of Chungking, in Szechuen, is now comparatively so well known, that that part of Mr. McCarthy's journey was tolerably easy, but after leaving Chungking the case is different. Circumstances induced him to make for Kweiyung-fu, the capital of Kweichow; he found that the country due south of Chungking was not at all to be compared with the eastern portion of Szechuen, large tracts of land being uncultivated, towns fewer, the people more scattered, and worse housed and clothed. It would be difficult, he says, to picture the desolation of a great part of the Kweichow province; in consequence of the many years' internal strife, whole districts having been entirely depopulated. After a fortnight's stay in Kweiyang-fu Mr. McCarthy decided to walk westward, as far, at least, as Yünnan-fu, being anxious to test the feelings of the people towards foreigners. The people everywhere continued civil, and no more difficulties were met with in Yünnan than in previous portions of the journey. On the road between Yünnan-fu and Tali-fu Mr. McCarthy remarks that "the people generally are in a deplorable condition: the women compelled to do manual labour, which in other places is confined entirely to men. Men and women—but especially the latter—suffer from the formation of goitre, some of immense size." From Tali-fu to Têng-yüeh, or Momien, Mr. McCarthy says, is really the most fertile part of the country. Yungchang-fu, between these places, has been a fine city, and even now the southern part is well built over, and a good deal of business is done there. Mr. McCarthy found that the fame of the medical work at Bhamô had spread even to Momien, and during the four days' walk to Manwyne he met many Shans and Chinese who spoke approvingly of the wonderful cures effected by the foreign teachers. Having secured the services of a chieftain he crossed the hills in two days, and arrived at Bhamô in safety. This Kah-chen chief and his followers professed the greatest friendship for the English, and treated Mr. McCarthy so well that he agreed to return with him in a fortnight, in order to continue his journeys in Yünnan. This plan, however, he was obliged to give up, as he was informed by our political agent at Bhamô that he could not be allowed to re-enter China from Burmah, that being forbidden by the Indian government!

THE Wesleyan Missionary Society have recently received some very satisfactory intelligence in regard to the attitude of the natives towards foreigners in a part of Central China where they had probably never been seen before. Writing from Kwang Chi, Hankow, under date of June 7, the Rev. Thos. Bramfit says, in describing a missionary tour, that, "as Lo Tien, Ma Tsien, and the other towns have not been visited before, the people came out in crowds to see the foreigner, and to hear his doctrine; and it is to be recorded to their credit that without exception they treated us kindly, and seem inclined to give the truth a fair hearing. It may be that the Chefoo Convention has something to do with the change in the temper of the people, as compared with that in other places when first visited; but it struck us that the change is to be accounted for chiefly by the increased intercourse between the people and foreigners."

THE French Government have sent M. Léon Cahun, well known from his researches in Eastern Europe and in Central Asia, to Cyprus, on a special mission, in order to investigate the island in an anthropological and archæological direction.

NOTES

THE death is announced, on the 13th inst., of M. Delafosse, Professor of Mineralogy in the Paris Museum of Natural History and the Faculty of Science.

WE have received a "first proof" of the list of awards made to British exhibitors, from which we cull the following items:—A Grand Prix and Diplôme d'honneur have been awarded, in Class 8, Organisation, Methods and Appliances for Superior Instruction, to South Kensington; in Class 15, Mathematics and Philosophical Instruments, gold medals have been awarded to Mr. J. H. G. Dallmeyer, Mr. Howard Grubb, Messrs. A. Léqé and Co., Messrs. Negretti and Zambra, Messrs. Ross and Co., and Sir William Thomson; in Class 16, Maps and Geographical and Cosmographical Apparatus, a gold medal has been awarded to Mr. Edward Stanford; in Class 65, Telegraphic Apparatus and Processes, a Grand Prix is awarded to Prof. A. Graham Bell, for his magneto-electric telephone, without electric current; in Class 67, Navigation and Life Saving, a gold medal to Sir William Thomson, for his compass.

THE Council of the Meteorological Society have arranged for a course of six lectures on meteorology to be given at the Institution of Civil Engineers, 25, Great George Street, Westminster, on successive Thursday evenings, commencing on the 31st instant at eight o'clock. The first lecture will be by Dr. R. J. Mann, on the "Physical Properties of the Atmosphere." The other lectures will be by J. K. Laughton, F.R.A.S., R. Strachan, F.M.S., Rev. W. Clement Ley, M.A., F.M.S., G. J. Symons, F.R.S., and R. H. Scott, F.R.S. Admission to the lectures will be by ticket only, which may be obtained free on application to the Assistant Secretary at the office of the Society, 30, Great George Street.

IN a lately-issued number of the *Proceedings* of the American Philosophical Society, Prof. Cope has given an account of the collection of fishes made by Prof. Orton at various points on the head-waters of the Amazon. Species of the genera *Belone* and

Tetrodon—characteristically marine forms—are here recorded as having been obtained in these districts 2,500 miles from the mouth of the river—thus showing how far marine animals will penetrate into, and become ultimately acclimatised in, fresh waters.

THE existence of the true heath plant in North America was for a long time considered very doubtful, and its detection in New England some years ago is a matter of much interest. The published localities hitherto are Newfoundland, Nova Scotia, and Massachusetts, but, according to the *Bulletin* of the Torrey Botanical Club, Dr. Hexamer, of Newcastle, has lately found a few plants of it near Egg Harbour, New Jersey.

PROF. MORSE, since his return to Japan, has been diligently engaged in prosecuting investigations into the natural history and archæology of the coast region. During his summer's stay at Yezzo he brought together a large material of the most interesting character, the transmission of a portion of which to American museums, it is hoped, he will be able to bring about.

THE operations of the United States Fish Commission a Gloucester, Massachusetts, were brought to a close, so far as the deep-sea research was concerned, on September 26, on which day the last trip of the United States steamer *Speedwell* was made. This vessel, under the command of Capt. L. A. Beardslee, of the navy, has been diligently occupied since the middle of July in her work, and has made trips of greater or less extent, varying from five to fifty miles, nearly every suitable day during the summer. With an efficient corps of naturalists, consisting of Prof. A. E. Verrill, Mr. Richard Rathbun, and Prof. Sanderson Smith for the marine invertebrates, Dr. W. G. Farlow for the algæ, Prof. G. Brown Goode, Dr. T. H. Bean, and Mr. Earll for the fishes, and Mr. Asaph Hall, Jun., in charge of the temperature observations, very important results have been secured, the location and extent of important fishing banks have been ascertained, and existence established, of many new and important fishes and invertebrates. A full account of these will be published in the annual report of the commission. The existence of species of shark, chimæras, and other strange fishes in the deep water off the coast, has been established by numerous specimens, while the corals, gorgonians, star-fishes, and other invertebrates brought in are of the most interesting character. A portion of the commission has also been engaged during the season in collecting numerous facts in regard to the fisheries of Gloucester, especially those relating to the cod, halibut, and mackerel, the report upon which, when published, will doubtless be considered the first reliable statement of the history of this great industry.

IT is desired to enlist the co-operation of botanists in general, and more especially of bryologists, in a scheme set on foot by the Botanical Locality Record Club for investigating the geographical distribution of mosses in the British Isles. The Botanical Locality Record Club was founded in 1873 for the purpose of working out the distribution of British plants, records, accompanied by specimens as vouchers, being sent in by the members and embodied by the recorder in an annual report on the plan of "topographical botany." The club, which commenced with fifty-four members, now contains nearly 100, including some of our most eminent botanists. A large amount of work has been done by the club during the five years that it has been in existence, the floras of several counties previously almost unexplored have been worked out, and a very large number of additions have been made to the flora of many others. Up to the present time the reports have dealt only with the flowering plants and vascular acrogens. In 1875, however, a suggestion was made that the club should include in its field the other orders of cryptogamia, and it is considered desirable that, if possible, this should be carried into effect. It is proposed to start with the mosses, for which order Mr. C. P. Hobkirk, F.L.S., and Mr. H. Boswell have consented to act as recorders. A list of the mosses hitherto recorded in Great Britain, entitled "The London Catalogue of British Mosses," has been drawn up by Messrs. H. Boswell and C. P. Hobkirk, as an aid to collectors, and to secure uniformity of nomenclature. A number of specimens have already been sent in by several members. At present, however, the funds in hand are not sufficient to allow of the publication of a report on mosses without seriously curtailing the report on the flowering plants, which it is not considered desirable to do, especially seeing that bryologists constitute but a comparatively small proportion of the members of the club. If more botanists interested in mosses (say thirty) could be induced to join the club, the additional subscriptions (5*s*. each per annum) would suffice for the publication of a report on mosses. Botanists wishing to join the club are requested to send their names either to Dr. H. F. Parsons, Goole, or C. P. Hobkirk, Huddersfield.

WE are glad to see that the Metropolitan Board of Works have decided to give the electric light a fair trial by making a large and continued experiment along the Victoria Embankment and perhaps the recently freed Waterloo Bridge. The Commission of Sewers have resolved to try similar experiments on the Holborn Viaduct and in the open space in front of the Royal Exchange and Mansion House. We trust proper discrimination will be exercised in making arrangements, and if the interests of the public are alone considered, we can hardly doubt what will be the result.

Engineering has been publishing an interesting series of articles on physical science at the Paris Exhibition. In an article in the last number on the Jablochkoff system of electric lighting, some figures are given as to the total cost of the light from sixteen Jablochkoff candles, which shows it to amount to 8*s*. 6½*d*. per hour. The light of the sixteen candles is estimated by the Jablochkoff Company as equal to that of 1,600 gas-burners, each consuming 3¾ cubic feet per hour, or about 6,000 cubic feet altogether. The price of gas in Paris being 6*s*. 10*d*. per thousand cubic feet to the public and 3*s*. 5*d*. to the Administration of the Ville de Paris, it follows that the cost of illuminating by gas, equal in power to that produced by the Jablochkoff system for 8*s*. 6½*d*., would be 2*l*. 1*s*. to the public and 20*s*. 6*d*. to the Ville de Paris, while in London, taking gas at 3*s*. per thousand, its hourly cost would be 18*s*.

CARRIER pigeons, it would seem, are being turned to useful account in a new direction in Germany, for Consul Ward writes to the Foreign Office that the successful results attained by the establishment of communication between the two Eider light-ships and the port of Tönning, in Schleswig, by these means has led to the organisation of a similar arrangement between the light-vessel stationed off the Island of Borkum, at the mouth of the Ems and the island itself, whence any news brought by the pigeons can at once be forwarded by telegraph to the main-land.

SERGEANT JENNINGS, of the Signal Corps of the United States, has established a temporary station in the American section of the International Exhibition, and is practising weather-warning according to the rules of the service, as far as it is possible with his limited knowledge of the peculiarities of the Parisian climate and atmospheric conditions. This has attracted the attention of the public and scientific authorities.

MR. A. CRAIG-CHRISTIE, of Edinburgh, writes to us to say that it occurred to him some little time ago that our common grass, *Molinia cærulea*, might form a good material for paper-making, on account of its tenacity of fibre and the comparatively small quantity of silica in its composition—two characters which dis-

tinguish it from all our native grasses. He sent a small quantity of the grass to Mr. Thomas Routledge, of Sunderland, who, after experiment, concludes that, taken as dried and put up carefully in bundles, free from weeds and dirt, its value would be equal to esparto at 5*l.* per ton dry. "I, however," Mr. Routledge states, "must refrain from reporting *positively* as to its value for paper-making from the result of so small an experiment. I should require at least one ton (more would be better) to test it practically and make paper from it. . . It may be worth more than the value I mention, but only a practical working trial into paper can properly test this point." Mr. Christie writes in the hope that some of our landed proprietors may be sufficiently interested in the matter to send Mr. Routledge a quantity of the grass, so as to repeat his experiment on the large scale. The grass grows in the open parts of woods and on moorlands all over Scotland, and could be cultivated where nothing else of any value will grow. As the plant lasts for several years, the only expense after the first outlay would be that of gathering in the crop.

A DESPATCH from Poughkeepsie, dated October 4, gives the particulars of a severe earthquake shock along both sides of the Hudson from Marlborough to Peekskill, a distance of twenty-five miles. The shock was first felt at 2.30 o'clock A.M. at West Point. The shock seemed to come from the north and to pass south. For several seconds the earth seemed to rock, and houses were shaken and windows rattled. The rumbling and shock together lasted half a minute.

IN reference to the article in vol. xviii. p. 620, on the balloon experiments at Woolwich, Mr. Percy Smith writes to remind us of the fact that the first aerial voyage made in England took place on September 14, 1784, when Lunardi ascended from London in a balloon thirty-three feet in diameter, and filled with hydrogen.

M. AMÉDÉE GUILLEMIN, the well-known author of "Le Ciel" and other works on popular astronomy, has been selected by the Liberal-Republican Committee to stand as a candidate in the next elections for the French Senate, for the Department of Saone et Loire. It is stated that a number of scientific men will contend for other vacant seats.

A TELEGRAM from Sydney states that it has now been definitively decided to hold the International Exhibition in August, 1879, as gazetted last February. The Exhibition is being organised under the auspices of the Agricultural Society of New South Wales.

IN connection with the Bristol Museum and Library we have received a syllabus of a course of ten lectures, on literary and scientific subjects, to be delivered in the lecture theatre of the Museum during the winter. The lectures are mostly scientific, and the lecturers men of established eminence in their own departments.

AUSTRIA, Spain, Egypt, China, Morocco, Portugal, Russia, and England, with all her colonies, have presented the French government with all the objects which have been exhibited by them in the ethnographical and pedagogical departments of the exhibition. These invaluable collections will be exhibited in the Ethnographical and Pedagogical Museum, which the French government intends to establish, according to the announcement made by M. Bardoux in one of his last speeches.

THE *Journal Officiel* of the French republic published, on October 19, a decree of the President, appointing a general commission to investigate the means of making the best use of running waters all over France. A systematic inspection will take place. The commission has been divided into the following sections:—1. Irrigation; president, M. Andral. 2. Supply of cities; president, M. Magnin. 3. Creation of reservoirs; president, M. Cocheris. Any person wishing to suggest improvements or useful works is requested to write to the president of the section which his communication concerns. The letters are to be directed to the Ministry of Public Works.

THE number of aeronautical ascents in France is increasing in a most remarkable manner, owing to the splendid working of the Giffard captive balloon. Every Thursday and Sunday two free balloons, inflated with pure hydrogen, have been sent up from the Cour des Tuileries during several weeks. The number is to be enlarged progressively, so that three, four, and at last five mounted balloons will be sent up.

TWO volumes of the *Memoirs* of the Geneva Society of Physics and Natural History have just appeared. The first, forming the second part of vol. xxv. of these *Memoirs*, is occupied entirely by the sixth fascicule of the "Mélanges Orthoptérologiques" of Henri de Saussure, and contains the monograph on the Gryllidæ. The second volume, forming the first part of vol. xxvi. of the *Memoirs*, contains (1) a paper by M. J. E. Duby on new or imperfectly known exotic mosses; (2) a stratigraphical study of the south-west part of the Crimea, by M. Ernest Favre; (3) researches on fecundation among various animals, by M. Hermann Fol.

A FINE elephant tusk was found in the month of August last in a locality near Geneva on a hill to the south-west of the confluence of the Arve and the Rhone. It was taken from a gallery in a very compact sand along with beds of hard and tenacious gravels formed of pebbles bound together by calcareous cement. The part of the tusk which has been extracted is about eighty centimetres long; it is the anterior portion, but the fragments left behind lead to the presumption that its total length exceeded 1·50 m. The bed in which it was found belongs to the old alluvium, several metres below the glacial earth. Prof. Alph. Favre assigns it to the *Elephas antiquus*, of which *débris* were met with in 1786 by H. B. de Saussure in two places in the southern part of the valley of Geneva Lake. A portion of the parenchymatous matter contained in the interior of the tusk has been analysed by Prof. Brun, of the University of Geneva, who, having dissolved it in hydrochloric acid, obtained a residuum of organic particles—charcoal, grains of fecula, spores of algæ and of mushrooms, four species of diatoms still living in the waters of the country, and lastly a polycistina, a silicious form not well determined, and discovered previously in the mud of the Lake of Geneva. From the presence of these Prof. Brun concludes that the elephant's tusk must have been a long time in fluviatile waters.

A THIRD mathematical tract (Invariants) for American students is now being written by Dr. W. J. Wright, recently Professor of Mathematics at Wilson College, Pa. The previous tracts, which we have noticed in these columns, bore the titles of Determinants, Trilinear Co-ordinates.

ON August 29, 1879, a century will have passed since Johann Jakob von Berzelius, the celebrated chemist, was born at Westerlösa, in Sweden. The Swedish papers draw attention to this fact, and request that preparations for a dignified celebration of that day should be made in good time.

WE have received three parts (containing the concluding chapters) of the second edition of Herr F. von Hauer's work, "Die Geologie und ihre Anwendung auf die Kenntniss der Bodenbeschaffenheit der oesterr. ungar. Monarchie" (Vienna, Hölder); also a German work, by Prof. F. Lorber, "On the Exactness of Measurements of Length."

THE investigations which have been carried on for more than ten years at the Hague, in order to find out the house which Spinoza inhabited from the year 1652 until the day of his death, on February 21, 1678, have at last been crowned with success.

It is now proved that the great philosopher occupied an attic in the house, No. 28, Paveljoens-gracht in Doublet Straat, opposite to the Holy Ghost House (hoffje). The house belonged to a Heer van der Spyk. Shortly a marble tablet with a fitting inscription will be placed over the entrance.

THE German Geological Society met at Göttingen in the last week of September. The first lecture was delivered by Prof. Credner, of Leipzig, who spoke on the granite of Geyer. Then Prof. Klein, of Göttingen, spoke on a collection of thin sections of minerals forming rocks.—Herr Levin presented a petrified starfish (*Asteria cilicia*) found by him in the upper Muschelkalk of the Hainberg. This specimen is particularly interesting, being the first starfish which has ever been found in the North German limestone.—A communication was read from Prof. Martin, of Leyden, on the tertiary fauna of Java.—Dr. Hornstein (Cassel) reported on a new treatise on Eozoon by Prof. Möbius (Kiel), which will be published in Parts 5 and 6 of the "Palæontographica." Prof. Möbius arrives at the conclusion, from long-continued investigations of numerous specimens of Eozoon, that the latter is of inorganic nature.—Then followed some minor communications from Herr von Groddeck, Prof. Streng, and Prof. Weiss (Berlin), the latter pointing to a recent discovery of coal in the so-called "Eherne Kammer," some few miles to the south of Eisenach. The other speakers at the meeting were Prof. von Seebach (Göttingen), Herren Grotrian (Brunswick), Römer (Breslau), Schmid (Jena), and Prof. vom Rath (Bonn). The Society will meet at Baden Baden in 1879.

ACCORDING to a communication made at the Berlin meeting of ornithologists by Dr. Brehm, the Crown Prince Rudolf of Austria is about to publish a work on Eagles, in conjunction with Drs. Brehm and von Homeyer.

WE have on our table the following works:—"Commercial Products of the Sea," P. L. Simmonds (Griffith and Farran); "Talks about Plants," Mrs. Lankester (Griffith and Farran); "Notes on a Tour in America," H. Hussey Vivian (E. Stanford); "Philosophical Fragments," Dr. Morell (Longmans); "Geological Survey of Canada: Report of Progress for 1876 and 1877," "The Germ Theories of Infectious Diseases," J. Drysdale, M.D. (Baillière); "Sedimentary Formation of New South Wales," Rev. W. B. Clarke (Trübner); "Annual Report of the Department of Mines of New South Wales for 1877" (Trübner); "Australian Orchids," Parts 3 and 4, R. D. Fitzgerald (Trübner); "Geology of Ireland," G. H. Kinahan (Kegan Paul, and Co.); "Animal Chemistry," C. H. Kingzett (Longmans); "Manuel du Voyageur," D. Kaltbrunner (Würster and Co., Zurich); "Report on the Geological Survey of the United States," Dr. Hayden.

THE additions to the Zoological Society's Gardens during the past week include a Squirrel Monkey (*Saimaris sciurea*), from Guiana, presented by Mr. Edward Calthrop; an Emu (*Dromæus novæ-hollandiæ*), from Australia, presented by Mr. C. Hampden Wigram; two Radiated Tortoises (*Testudo radiata*), from Madagascar, presented by the Rev. G. H. R. Fisk, C.M.Z.S.; a Water Rail (*Rallus aquaticus*), British Isles, presented by Capt. F. H. Salvin; a Michie's Tufted Deer (*Elaphodus cephalophus*), from China; a Naked-throated Bell-bird (*Chasmorhynchus nudicollis*); three Blue-bearded Jays (*Cyanocorax cyanopogon*); a Dark-winged Buzzard (*Leucopternis scotopterus*), from Bahia; a Saturnine Mocking Bird (*Mimus saturninus*); two Lined Finches (*Spermophila lineola*); two Gutteral Finches (*Spermophila gutteralis*), from Pernambuco; a Pileated Song Sparrow (*Zonotrichia pileata*), from Rio de Janeiro; a Palm Tanager (*Tanagra palmarum*), from Monte Video, deposited; a Patas Monkey (*Cercopithecus ruber*), from West Africa; two Ruddy Sheldrakes (*Tadorna rutila*), European, purchased.

ON A NEW METHOD OF STUDYING THE OPTICAL CHARACTERS OF MINERALS[1]

AS is well known, the optical characters of minerals furnish us with a most valuable means for identifying the various species. The practical application of these phenomena has, however, been much restricted by the difficulty of obtaining crystals sufficiently large and transparent to be cut into appropriate sections, so that the properties of some of the commonest minerals were very imperfectly known. By the methods hitherto employed it was almost impossible to study the black or imperfectly transparent minerals constituting the chief part of rock masses, and in fact little could be learned unless the specimens were so large and perfect that the individual species could be identified by other means. What we want is a method which will enable us to ascertain the approximate value of the principal optical constants, when we have at our disposal only detached, small, and imperfect crystals in their natural state, or those scattered about in thin sections of rocks, cut into plates which are inclined at every varying angle to the optic axes.

The method now to be described satisfies these requirements sufficiently well. Time would not permit me to give a full description of the apparatus, the manner of using it, and the conclusions to be drawn from the observed data. I must therefore avoid all unnecessary detail, and confine myself to such an outline as will serve to indicate the general character of the method. I may here say that I have received most valuable assistance from Prof. Stokes in the mathematical part of the subject.

When, many years ago, I first commenced to apply the microscope to the study of minerals, I was told by a well-known professor of mineralogy that it would never be possible to learn much by the use of that instrument. If we merely magnify a portion of a pure transparent crystal, we do, indeed learn little or nothing; but if, instead of viewing the crystal itself, we look through it with a suitable magnifying power at some appropriate object, we can learn more facts of interest and importance than by any other single method whatever. The property possessed by the object-glass of collecting divergent rays to form an image gives rise to an entirely new class of phenomena, and converts the microscope into a most valuable apparatus for optical research. The object examined through the crystals is the image of a small circular hole, or of rectangular lines ruled on a piece of glass, formed at the focal point of a well-corrected achromatic condenser fixed below the stage, and so arranged that the image is placed either just below or just above the lower surface of the crystal. The divergent rays passing through it to the object-glass are bent so that the focal length is, as it were, increased by an amount depending on the thickness of the crystal and its refractive power. In order to see the lines in focus, it is therefore necessary to move back the body of the microscope. If we know the thickness (T) and the amount of the displacement of the focal length (d), we can calculate the value of the index of refraction (μ) from the equation

$$\mu = \frac{T}{T - d}.$$

These values are measured by means of a scale and vernier attached to the body of the microscope; and, with care, there ought to be no error greater than $\frac{1}{2000}$th of an inch. The thickness of the specimen (T) is determined by measuring the difference in the focal points for particles of dust on the surface of the supporting glass and on the upper surface of the mineral. In a similar manner the value of d is determined by the difference in the focal length for the lines of the grating seen through the supporting glass with or without the specimen under examination. From the value of the index thus determined a small amount must be deducted, depending upon the aperture and correction of the object-glass, and, when great accuracy is desired, several precautions must be taken to avoid a number of possible small errors, which it would be tedious to explain in detail.

In the equation, $\mu = \frac{T}{T - d}$, it is assumed that the substance possesses no double refraction, as in the case of glass or crystals belonging to the regular system. When viewed through such a substance, only one simple and undistorted image of the circular hole can be seen, and both sets of lines are in focus at the same

[1] Address by the Chairman, H. C. Sorby, F.R.S., Pres. G.S., &c., at the meeting of the Yorkshire Geological and Polytechnic Society, held in Selby on March 13.

adjustment, no matter what may be their azimuth to the axes of the crystal. We obtain by measurements and calculation one single index of refraction, but this may vary so much in different minerals as to clearly point out what they are. Thus, for example, it varies from 1·43 in fluor to 2·34 in blende. If, however, the crystal possesses strong double refraction, the phenomena are far more complex, and vary according to the direction in which the section is cut, its azimuth to the lines of the grating, and also according as it has one or two optic axes.

If we look through a parallel plate of a uniaxial crystal with powerful double refraction, like calcite, cut perpendicular to the principal axis, we see two undistorted images of the circular hole, directly superimposed, one over the other, but separated vertically by a wide interval. This doubling of the image is due to the collection by the object-glass of divergent light, since, for strictly parallel rays passing in the same general direction, there is no double refraction whatever. Both systems of perpendicular lines are seen in focus at the same time in each of the images, one of which is due to the ordinary, and the other to the extraordinary, ray. By observing these focal points we obtain two indices of refraction, one being the true index of the ordinary ray (μ), and the other not that of the extraordinary ray (μ'), but a very low apparent index, equal to $\frac{\mu'}{\mu}$.

When the section is cut in other directions the images differ very much from one another. That due to the ordinary ray has invariably the same properties. The circular hole is not distorted, and both systems of line are in focus at the same time, so that we may call that image *unifocal*. The other image, due to the extraordinary ray, instead of thus maintaining a constant character, changes very greatly, the maximum of change being when the section is cut parallel to the principal axis of the crystal. There is no focal point whatever at which the circular hole is seen of its true size and shape, and the entire circumference is never all in focus at once. There are two special foci, widely separated, at which the circle is, as it were, drawn out into a long band, at one focus parallel, and at the other perpendicular to the axis. If the section be strictly parallel to the axis the focal point of the ordinary image is nearly half-way between these two foci of the extraordinary ray, and coincides in horizontal position with the point at which the two elongated bands intersect. There is thus no lateral displacement of the images. If, however, the section be not parallel to the axis they are displaced laterally, this character being a very delicate test of the accuracy with which the section has been made. In fact, in all cases, if the two opposite surfaces are parallel, the character and position of the images at once indicate the exact relation between the optic axes and the planes of the plate, whether they be natural or artificial.

On viewing the rectangular grating through a section cut more or less nearly parallel to the principal axis, no lines whatever can be seen by means of the extraordinary ray, unless one system is nearly parallel to the axis. At one focal point one system of lines is seen, and at the other focal point the other system, so that the image due to the extraordinary ray may be said to be *bifocal*. On rotating the grating, the lines are seen to become broader, then obscure, and finally invisible. Unlike the image due to the ordinary ray, the bifocal image has thus a special focal axis, and the lines can never be seen in sharp focus if they are not either parallel or perpendicular to this axis.

On the whole, then, we have three focal points, one for the ordinary, and two for the extraordinary ray; and by observing these we obtain three different indices of refraction, one being that of the ordinary ray μ; and, provided that the section is closely parallel to the axis, the index derived from the lines parallel to the axis in the extraordinary image is the true index (μ) of the extraordinary ray, whilst the third index is of the very abnormally high apparent value $\frac{\mu^2}{\mu'}$.

The characteristic peculiarity of crystals like aragonite, which have two optic axes, is that, when the section is so cut that the images are directly superimposed without lateral displacement, they give two bifocal images, and four apparent indices. When cut in particular directions one of these images may become unifocal, but then there is a more or less considerable lateral displacement of the two images. When the section is cut perpendicular to the line bisecting the acute angle between the optic axes, so as to give two very bifocal images, the images of the circular hole are crosses at two different foci, and not, as in the case of calcite, two circles. Biaxial crystals have three true indices of refraction (μ, μ', μ''), and, if the section be accurately cut in the plane of any two of the axes of elasticity, so that there is no lateral displacement of the images, the four apparent indices observed from the lines of the gratings are as follows:—

	Polarised in one plane.	Polarised in the opposite plane.
From lines perpendicular to the plane of polarisation	μ	μ'
From lines parallel to the plane of polarisation	$\frac{\mu''^2}{\mu}$	$\frac{\mu''^2}{\mu'}$

Calling these observed indices a, b, c, and d respectively, we thus have $\mu'' = \sqrt{a\,c}$ or $\sqrt{b\,d}$. It follows from this that we can determine the value of all three indices by very simple observations, made by employing a single section cut in the plane of any two of the three axes of elasticity. Absence of lateral displacement in the images at once shows us that the specimen in its natural state, or as artificially cut, is sufficiently parallel to one of these planes to be suitable for the determination of the indices; but even if it is not such as to give all three indices absolutely true, one at least may be correct, and the others may be determined approximately. In any case the character and position of the images at once shows in what direction the section is cut, or the relation which any parallel planes of a natural crystal bear to the optic axes, though the phenomena are more complex than in the case of uniaxial minerals.

It would occupy far more time than can be allowed on the present occasion to describe in detail the curious and anomalous appearance due to dichroism or to the laminar structure of particular minerals which gives rise to complex internal reflections. My chief aim has been to call attention to the very valuable facts which may be learned by viewing a circular hole or rectangular grating with a microscope through a parallel plate of any crystalline mineral. The data thus obtained are so remarkably characteristic that they alone would amply suffice to identify a large proportion of natural minerals. In many cases all the necessary observations can easily be made with small crystals in their natural state, which alone is of course a very great gain for practical mineralogy. The chief value of the method is, however, that it enables us to identify portions of minerals of microscopic size in sections of rocks as thin, or even thinner than $\frac{1}{100}$th of an inch with an amount of certainty which leaves little to be desired.

When examining specimens of such a size that their thickness must be measured by means of the scale attached to the body of the microscope, I find that an object-glass of about $\frac{2}{3}$ inch focal length, combined with a somewhat highly magnifying eye-piece, gives the best results. When, however, we come to study the minerals in moderately thin sections of rocks, it is impossible to measure the thickness and the displacement of the focus sufficiently accurately by means of the scale and vernier. The fine adjustment screw of the microscope may then be employed along with a $\frac{2}{5}$ or $\frac{1}{5}$ object-glass, and, if properly constructed and used, the requisite measurements may be made to within $\frac{1}{20000}$th of an inch. We may thus approximately determine the indices in sections only $\frac{1}{1000}$ of an inch in thickness. It is, however, necessary to adopt a system which reduces the number of separate measurements and to a great extent eliminates several sources of error. Instead of attempting to measure the *absolute* thickness of any particular crystal, and the actual displacement of the focal length due to it, the *apparent* thickness of the mineral, *as seen through itself* (t), is measured by means of the rotation of the graduated circular head of the fine adjustment by focussing, first to the top and then to the bottom, of some appropriate specimen. In each particular substance this apparent thickness is equal to the true thickness divided by the index of refraction. The thin glass cover is made somewhat larger than the section, so as to project beyond it, and inclose a layer of the hard and brittle balsam used to fasten down the piece of rock. Selecting for observation a specimen as near as possible to this balsam, so as to avoid any error due to unequal thickness, the difference (d') in the displacement of the focal length due to the mineral and the balsam is ascertained by focussing through each the lines of the grating. This value is positive or negative, according as the index

of the mineral is greater or less than that of the balsam. It then follows that $t \pm d'$ is the thickness, as seen through itself, of the amount of balsam of the same real thickness as that of the mineral: the effects of the balsam below and above it, and of the covering-glass, being thus entirely eliminated. If, then, the index of the balsam be m, we can easily calculate that of the mineral (μ) from the following equation:—

$$\mu = m \frac{t \pm d'}{t}.$$

In the case of the hard and brittle balsam used to fasten down the specimen, the value of m is about 1·54; but if there be any doubt about the true index, it can be ascertained by special measurements.

In a similar manner we may determine the index of some unknown mineral by comparing it directly with some other mineral lying near to it, the true index of which is either well known or has been previously ascertained from special measurements. For this purpose quartz is often very suitable, since its index varies very little. One great advantage of this method is that specimens may be observed far away from the edge of the section, provided of course that the minerals compared are so close together as to prevent any error due to unequal thickness in different parts.

It must be borne in mind that, when any mineral has a very powerful double refraction, its apparent thickness, as seen through itself, varies according to the particular ray used for illumination and the direction of the objects chosen to determine the focal distances of the lower surface. There is, however, generally no difficulty in measuring with sufficient accuracy the mean apparent thickness, or that corresponding to some one image, and in calculating out the results accordingly.

In connection with this subject it may be well to call attention to a somewhat interesting fact. If we have, side by side, two substances of different refractive power, but of the same *absolute* thickness, their apparent thicknesses, *as seen through themselves*, vary directly as the velocity with which light moves in them. Indeed, strictly speaking, the determination of minerals in the manner now described depends entirely on an indirect measurement of the velocity with which light is propagated through them in different directions.

In order to illustrate the practical applications of this method, I will describe the results obtained in the case of a section of dolerite from near Glasgow, which, on an average, is about $\frac{1}{400}$th of an inch thick.

I found that the index of a colourless transparent mineral, filling up cavities between the original minerals, was about 1·48 or 1·49. This exactly corresponds with that of analcime, with which its other optical characters agree.

Another colourless mineral, also filling cavities, was found to have the indices and other characters of calcite.

A third colourless mineral, evidently an original constituent, was seen to have a comparatively feeble double refraction, and its index was found to be 1·61. Its general appearance was like that of some felspar, but this index clearly proves that it cannot be any species which contains a considerable amount of alkali, which would greatly reduce the refractive power. The index of labradorite was not previously known, but I find that it is 1·61, and therefore there can be little doubt that the mineral in the section is that species.

The section also contains a number of transparent reddish-brown crystals, their index of refraction being about 1·79. This and their other optical characters closely agree with those of the dark augite in the lava of Vesuvius.

In now concluding this short address I cannot but feel that I have been obliged to omit all allusion to many points of considerable practical importance. I have not attempted to describe the subject in such a manner as would enable any one to at once practically apply the method in all sorts of cases. I gave a somewhat full account of one branch of the subject in my address at the meeting of the Mineralogical Society at Plymouth, and entered into the more purely microscopical aspect of the question in my late address at the anniversary meeting of the Royal Microscopical Society. I propose to communicate a detailed paper to the Royal Society as soon as a correct explanation can be discovered of certain small but remarkable discrepancies between mathematical theory and observation. My chief object now is simply to point out what valuable facts may be learned respecting the nature of any mineral by looking through it with a microscope at a circular hole or rectangular grating. This is a totally different thing to magnifying the mineral itself, or to looking through the mineral at any distant object without a microscope. The success of the method depends entirely upon the optical conditions characteristic of a compound microscope. I have lately greatly improved the apparatus hitherto employed, but the examples already given will, I trust, serve to prove that, even with the less perfect appliances, it was possible to identify in a very satisfactory manner, many of the minerals met with in their microscopical sections of rocks, and thus to determine their constitution with far more certainty than heretofore.

RECENT OBSERVATIONS UPON THE PLACENTATION OF THE SLOTHS

M. JOLY has recently brought before the Academy of Sciences of Paris the results of a careful examination of the structure of the placenta of the Aï, or Three-toed Sloth (*Bradypus tridactylus*, Linn.), and proposes important changes in classification, after comparison of this structure with that of certain allied mammals.[1]

It is now six years since we contributed to NATURE a notice of the observations of M. Alphonse Milne-Edwards upon the fœtal envelopes of another member of the Edentata, the "Tamandua" Ant-eater, published by him in the *Annales des Sciences Naturelles*. This Edentate is there stated to have a "placenta discoïdal envahissant."

The sloths are literally, as Buffon described them, ruminant animals, in that they have four stomachs, but they are, at the same time, wanting in all the other characters which pertain to Ruminants proper. Linnæus, on the contrary, classed them, at first among the Primates, but afterwards among the Bruta—the "Edéntés" of Cuvier—and his example was followed by De Blainville. Cuvier placed the "Tardigrades" (*Bradypus*) at the head of the Edentata, although they possess well-developed canine and molar teeth.

It will be seen, then, that as regards the position of these animals the embarrassment of the taxonomist has been extreme, as the genus *Bradypus* has been bandied about from the Ruminants to the Primates, and from these latter to the Edentata. Latterly, however, great importance—and with good reason—has been attributed to the structure of the placenta, as affording characters distinctive of the various groups of mammals, and as giving valuable indications of their zoological affinities. The classification of the placenta by Carl Vogt, though scarce a quarter of a century old, into zonary, diffuse, and discoidal, is nowadays acknowledged to be incomplete, nay, even faulty in some of its applications; for we know now, thanks to the work of Alphonse Milne-Edwards, that if the majority of Ruminants have a multicotyledonary placenta, the camel, the chevrotain, and the *Tragulus* have, on the other hand, one of the diffuse variety. It is the same with the digitigrade Pachyderms (wild boar, &c.), while the plantigrade ones (*Proboscidea, Hyracoidea*) differ from the first in having a zonary placenta like that of the Carnivora and Amphibia (seals, &c.). In fine, although stated to be so, this organ is neither diffuse nor subdivided among any of the Edentata studied from the point of view of their placentation. Nay, more; among these animals the placenta offers, according to genera, and even according to species, differences so well marked that it is necessary, following the apposite remark of M. Alphonse Milne-Edwards, to give up seeing between the different types of Edentata affinities as narrow as those which are supposed, even now generally, to exist among them. Carus has represented the placenta of the Aï, or three-toed sloth, as being multi-lobed, but he does not give any precise information as to the number of these lobes, their structure, the extent which they occupy relatively to the membranes of the ovum, and their connection with the uterine mucous membrane [decidua ?], &c.

The placenta of the Aï examined by M. Joly presented itself under the form of a veritable membranous pouch constituted by the amnion and the chorion, and garnished, on almost all its external surface, with a large number (more than a hundred) of lobes or cotyledons of more or less irregular shape and of very variable size, from one millimetre to one or two centimetres. Viewed from the external face of the placenta, these cotyledons appear, some rounded and flat, like Nummulites, others of the form and size of seeds of millet. Others, lastly, much larger, grouped in numbers together, recall by their aspect the multi-

[1] *Comptes Rendus*, August 12, 1878.

lobed kidneys of birds and of certain Ophidian reptiles. Cavities, more or less roomy, in which are doubtless inserted the vessels of the hypertrophied mucous membrane, are also visible on the outer surface of the fœtal placenta. But it is specially upon the internal aspect that the lobules form numerous folds exactly limited, of a thickness frequently considerable (more than one centimetre), strongly adherent to the chorion by a pretty long base, free for the most part for the rest of their extent. We can understand, then, up to a certain point, that Carus should have been able to compare this placenta with that of the ruminants, from which it nevertheless differs much, since its cotyledons are made up of full lobes, generally antiguous, and not of isolated capsules, and distant one from the other like those of the fœtal placenta in the cow, or the maternal one in the sheep.

But we are as yet more disposed to assimilate the placenta of Aï to that of the Lemuroids, notably that of the *Propithecus* of Madagascar, which has been described by M. Alphonse Milne-Edwards under the name of *placenta en cloche* or *placenta envahissant*. In Aï, as in *Propithecus*, the chorion is covered almost entirely with thick and crowded villosities, constituting a kind of vascular cushion resulting from the confluence of a multitude of irregular cotyledons. But the Aï approaches *Propithecus* not only in the structure of the placenta but also in its habits, for both are arboreal, and have a diet exclusively vegetable. Besides this the uterus of the Aï is pyriform, like that of the human female and the female of most apes, a peculiarity which, with the possession of pectoral mammœ, approximates *Bradypus* to *Propithecus*. Linnæus and De Blainville seem then to have been guided by a "kind of divinatory intuition," as it were, when they ranked the sloths of Brazil in the order of Primates, only that they ought not to be classed among the apes proper, but by the side of the *Propithecus* of Madagascar and the slow Loris of the East Indies, of which they are the analogues, or American representatives.

M. Joly finally concludes thus: By its bursiform placenta, as well as by many other peculiarities of organisation, the Aï is a Lemuroid, and not an Edentate.

Not the slightest allusion is made by M. Joly to the well-known publications of Prof. Turner upon the comparative anatomy of the placenta, and especially to a paper read before the Royal Society in May, 1873, upon the fœtal structures of that variety of two-toed sloth called by Prof. Peters *Cholœpus Hoffmanni*.

J. C. GALTON

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

THE Master and Fellows of Gonville and Caius College, Cambridge, have considerably enlarged the chemical laboratory of the College, and have added a small but very serviceable lecture-room, with apparatus-room adjoining. They have likewise provided a private laboratory for the Prælector. The main laboratory now accommodates fifteen students working at one time.

UNIVERSITY COLLEGE, BRISTOL.—The third session of this young institution opened on October 8. The competition for the entrance scholarships was closer than any preceding year, and the general standard of attainments higher. The engineering department of the College is now fairly started, and is almost, if not quite, unique in character, the principal engineering firms in the district having agreed to an arrangement, whereby they receive into their works the engineering students of the College for the six summer months, the six winter months being devoted to the theoretical training of the College. The number of male day students of the College has largely increased; the entries in classical and modern literature, in chemistry, mathematics, and physics, exhibiting a satisfactory increase on those of the preceding year. The attendance at the evening classes is also very large. The scientific side of the College course has been strengthened by the appointment of Mr. W. J. Sollas, M.A., F.G.S., as lecturer on geology. A course of lectures on analytical chemistry is being given by Prof. E. A. Letts, who also resumes his industrial lectures on Dyeing and Scouring at Stroud.—Mr. J. Clapham also continues his course of instruction on Textile Fabrics. A course of lectures on the Technical Applications of Electricity, by Prof. S. P. Thompson, is also announced. The morning lectures on Political Economy are this year delivered by Mrs. Paley Marshall.—Mr. L. A. Goodeve, B.A., has been appointed lecturer on Law.

SCIENTIFIC SERIALS

The American Journal of Science and Arts, September.—In an opening paper on the origin of comets Prof. Newton compares the hypotheses of Kant and Laplace, the former of which represents that these bodies are formed from the matter of the condensing solar nebula; the latter, that they have no relation with this, but were made from matter scattered through stellar space. He shows that the curve of actual distribution of the inclinations of cometic orbits to the ecliptic, agrees well with that required by the hypothesis of Laplace, if we first make reasonable allowance for known perturbations, and for the comets of short periods, but that it is not thus made to agree with Kant's hypothesis.—Prof. Gray explains the distribution of tree species in North America, and traces similar species dispersed over widely-separated continents to a polar centre, where they once flourished in a temperate climate. Among other facts he mentions that while the Atlantic American Forest has almost three times as many genera and four times as many species of non-coniferous trees as the Pacific Forest, it has slightly fewer genera, and almost one-half fewer species of coniferous trees.—Prof. Marsh describes a new pterodactyl from the Jurassic of the Rocky Mountains.—Professors Draper and Watson give their observations on the solar eclipse, and an intra-Mercurial planet respectively.—The animal of *Millepora alcicornis* is figured by Mr. Rice, who confirms the conclusions of Agassiz.—Prof. Verrill notes some additions to the marine fauna of the east coast of North America; and among chemical notes is one on antimony tannate, by the Misses Swallow and Palmer.

Journal de Physique, September.—Some experiments showing the power of a vibratory motion to produce decomposition of explosive liquids and ebullition of superheated liquids are here described by M. Gernez. They consist in rubbing with a damp cloth a clean glass tube containing, *e.g.*, supersaturated seltzer water that has been kept in it for months, or a little nitrous acid below water, or methylchlorhydric ether. In the two former cases there is a projection of liquid; in the latter, a vigorous boiling occurs but soon ceases, owing to the consumption of heat by the vapour formed, reducing the temperature to near the normal boiling-point.—M. Cornu gives an account of his valuable researches on the ultra-violet solar spectrum, which have from time to time been communicated to the Paris Academy.—M. Planté describes effects got with his rheostatic machine; it gives, in general, all the effects of electric machines and induction coils, and these are not apparently much interfered with by the hygrometric state of the air.—We note, among the abstracts, one of recent proceedings of the St. Petersburg Physical Society.

Atti della R. Accademia dei Lincei (Rome) 1876-77, vol. i.—This part commences with a second instalment of Prof. Respighi's memoir on the latitude of the Roman Observatory.—On fluoride of magnesium, by A. Cossa.—On the theoretical velocity of sound and the molecular velocity of gases, by A. Rieti.—Petrographical studies, by G. Struever (two plates).—On the constitution of chloral ammonia and aldehyd-ammonia, by R. Schiff.—Electrostatic researches, by P. Volpicelli.—On the microscopic aspect of certain nervous fibres, by Franz Boll (two plates).—On some palæozoic fossils of the Maritime Alps and of the Ligurian Apennines, by B. Gastaldi (four plates).—On an objection to Melloni's theory of electrostatic influence, by P. Volpicelli.—Memoir on modular equations, by H. T. Stephen Smith.—On the dilatation, the capillarity, and the viscosity of fused sulphur, by G. Pisati.—On the titanite and the apatite of the Lama dello Spedalaccio, by G. Uzielli.—On the direction of gravity at the Barberini Station on the Monte Mario, by F. Keller.—Experimental researches on the tenacity of metals at different temperatures, by G. Pisati, C. Saporito, and S. Scichilone. The author experimented with copper, steel, brass, and aluminium.—Geological investigation of the mountain group of the Gran Paradiso, by M. Baretti (with seven carefully executed maps).—Experimental researches on electric discharges, by A. Richi (five plates). This and the previous one are amongst the most elaborate papers in the volume.—On the small oscillations of a rigid and perfectly free body, by V. Cerrutti.—On the anatomy and the physiology of the retina, by Franz Boll (one plate).—Ephemerides and statistics of the River Tiber before and after the confluence with the Aniene River, during the year 1876, by A. Bettocchi.—On some cave miriapoda of France and Spain, by F. Fanzago.—On the duration of vitality in the germinative spot, by Dr. G. Colasanti.

—Researches in theoretical crystallography, by G. Uzielli.—On the experimental determination of the electric density on the surface of conducting bodies, by E. Beltrami.

Reale Istituto Lombardo di Scienze e Lettere, Rendiconti, vol. xi. fasc. xiv.–xv.—We note the following papers in this number:—Colouring matters contained in the grape and a new means of judging of the degree of ripeness of this fruit, by S. Pollacci.—Transformation of hydroxylamine into nitrous acid, by Dr. Bertoni.—Action of solar rays on haloid compounds of silver, by Dr. Tommasi.—Reduction of chloral, by the same.—Results of vivisection of the cerebellum, the transverse peduncles, the semi-circular canals, and the nerves of taste, by Dr. Lussana.

SOCIETIES AND ACADEMIES

London

Entomological Society, October 2.—H. W. Bates, F.L.S., F.Z.S., vice-president, in the chair.—Mr. J. Lawrence Hamilton, M.R.C.S., was elected a Subscriber, and Mr. Thos. Nottidge a Member of the Society.—In reference to the statement of Mr. F. Smith at the last meeting of the Society, to the effect that the Linnean collection of insects contained in the apartments of the Linnean Society had fallen into a state of complete neglect, Mr. McLachlan read a report on the result of an examination he had since made of that collection. Mr. McLachlan considered that the collection was now in the same condition as it had been for probably a quarter of a century, and that the charge of neglect could not be sustained. Mr. Stainton fully corroborated this view, and stated that from a recent examination of the lepidopterous portion of the collection he had been unable to detect any appreciable deterioration in it since the year 1848, when he had first occasion to consult it.—Mr. Jenner Weir exhibited specimens of *Hipparchia semele* from various localities, showing a tendency to vary in colour on the under side in accordance with the nature of the soil of the district in which the specimens had been taken.—Mr. McLachlan exhibited the eggs and young larvæ of *Ascalaphus longicornis*, found by M. E. L. Ragonot, in the Forest of Lardy, apparently the northern limit of distribution of the species. Mr. McLachlan also exhibited, on behalf of Mr. Edwin Birchall, an example of *Heliothis scutosa*, captured by Mr. Campbell in the north of Co. Donegal, Ireland.—Mr. Rutherford exhibited and communicated a description of a new species of cetoniidæ, from Mount Camaroons. Mr. Rutherford also exhibited a specimen of *Ranaleosoma ruspina*, which was curiously and symmetrically destitute of scales.—Mr. Champion exhibited specimens of *Amara infima*, taken at Cobham, Surrey.—Mr. Forbes exhibited a collection of insects from Switzerland.—Mr. Wood Mason read a note on a saltatorial *Mantis*, and exhibited a specimen of the insect which had been captured on the banks of the Tagus. He also read notes on the hatching period of Mantidæ in Eastern Bengal, and on the presence of stridulating apparatus in certain *Mantidæ*. Mr. Wood Mason also stated that he had discovered a remarkable case of viviparity in an orthopterous insect, *Panesthia javanica*, a cockroach inhabiting the tropical forests of Southern Asia and Australia.

Paris

Academy of Sciences, October 14.—M. Fizeau in the chair.—The President announced the death of M. Delafosse, member in the Section of Mineralogy.—The following papers were read:—Presentation of vol. ix. of the "Observations of Pulkowa," by M. Otto Struve. This contains micrometric observations made by the author during forty years (with the same instrument and by the same method) on double and multiple stars. They continue the series of twelve years' like observations by his father at Dorpat. By these measurements M. Struve has been able to observe, *e.g.*, the epicycloidal motions of the third star in ζ Cancri, to determine approximately the orbit of 42 Comæ Ber., and clear up the controverted system of 61 Cygni. The measurements in this volume relate chiefly to double stars of the Dorpat catalogue, in the northern hemisphere, and to all systems discovered at Pulkowa. Another volume will contain extended observations.—Formulæ relating to perforation of iron armour plates, by M. Martin de Brettes.—M. Decharme presented a supplement to his memoir on vibratory forms of solid or liquid bodies; it relates to experiments with a large glass plate, with which the former results (with small plates) were confirmed.—M. Champin communicated an observation regarding transformation of apterous into winged phylloxera in the galls.—Third letter of Prof. Watson on the discovery of intra Mercurial planets. M. Mouchez considered the information here given answered his objections in great part, and left no doubt of the reality of the discovery of at least one of the two planets.—Reply to a communication by Herr Weber on thermodynamics, by M. Levy.—On a new micrometer, meant especially for meteorological researches, by M. Govi. In this the threads or fine wires are replaced by the two edges of a slit made in a very thin layer of silver, gold, platinum, or other inalterable metal, placed on the surface of a plate of glass having perfectly plane and parallel faces. The slit is produced by means of a light metallic tracer; and for larger slits the tracer is made to remove the metal in advancing parallel lines. Advantages attach to the extreme thinness of the metallic layer, its opacity, rigidity, and inalterability under considerable thermometric and hygrometric changes, the possibility of easily making slits as narrow or as wide as may be desired, and the facility of substituting different plates for each other in the same frame.—On a new metal, *philippium*, by M. Delafontaine. It is so called in honour of M. Philippe Plantamour, of Geneva, the friend and pupil of Berzelius. The author's former conclusions are confirmed; the new earth (of samarskite), which has a colour and a molecular weight intermediate between those of yttria and terbine, is not a mixture of these two bodies, but an oxide of a new metal. Supposing provisionally that philippine is a protoxide, its approximate equivalent is between 90 and 95. M. Delafontaine gives the properties of some compounds, the philippic formiate, sulphate, nitrate, &c., and describes the spectroscopic appearances gievn by concentrated solutions of philippium. These present a very broad characteristic absoption band which is absent from terbic, yttric, and erbic solutions.—Action of the juice of beet-leaves on perchloride of iron, under the influence of light, by M. Pellet. It has, in absence of chlorophyll, the power of reducing salts of iron easily, in light. This reduction may take place in the dry state, and with solutions having no longer vitality. It is due to oxidation of several organic substances contained in the leaves, such as sugars, tannin, azotised matter, &c.—M. Ronder presented a note on an arrangement for observing the stars in broad daylight, without the aid of a telescope; it consists in the use of a long tube, the lower end of which terminates in a dark chamber.

CONTENTS

THURSDAY, OCTOBER 31, 1878

SCIENTIFIC WORTHIES

XIII.—SIR GEORGE BIDDELL AIRY

SIR GEORGE BIDDELL AIRY was born at Alnwick, Northumberland, on July 27, 1801. He was first educated at private schools in Hereford and Colchester, and passed at the age of eighteen to Cambridge, where he entered at Trinity College as sizar. Here he developed his love for mathematics and graduated as senior wrangler in the year 1823.

In the following year, being elected a Fellow of Trinity College, he was closely engaged with the introduction of a new class of studies into the University, and published his "Mathematical Tracts on the Lunar and Planetary Theories," the "Figure of the Earth," &c., and the "Undulatory Theory of Optics," a work of considerable merit, which showed at once both the ingenious mathematician and the accomplished philosopher. In the year 1825 he wrote for the Cambridge *Transactions* papers "On the Forms of the Teeth of Wheels," and "On Escapements."

In the following year he was elected Lucasian Professor of Mathematics, and applied himself with the utmost ardour to the promotion of the knowledge of experimental philosophy in the University, and a great many of his papers, published at that time and afterwards in the *Transactions* of the Cambridge Philosophical Society, bear on those subjects, and principally on the most remarkable of them, Undulatory Optics, a field of research quite new at that time. The requirements of universities were never afterwards lost sight of. He gave, for instance, in the year 1868, a course of lectures in the University of Cambridge on the subject of Magnetism, with the view of introducing that important department of physical science into the studies of the University. His books—"Theory of Errors of Observations," "On Magnetism," and various memoirs in the *Transactions* of learned societies were written principally for this purpose.

In the year 1828 he was elected Plumian Professor of Astronomy, and was charged with the directorship of the Astronomical Observatory, where he had to superintend the erection of several instruments, principally the mounting of the great Northumberland equatorial, which was constructed almost entirely under his own direction. In the same year he was elected a Fellow of the Royal Astronomical Society; and now commences an activity which is almost unsurpassed in the annals of astronomy.

Prof. Airy, after the example first given by Maskelyne, and followed by Bessel and Struve, introduced into the observatory a most efficient system for reducing the observations, and printed them annually. The greatest regularity in the routine of consecutive years was aimed at and attained, in great measure, from adherence to the rule of forming the plan of observations for each year in the greatest detail practicable before the close of the preceding year.

These practical occupations did not divert his mind from theoretical studies. In the year 1831 a most important paper was published in the Cambridge *Transactions*, "On the Inequality of Long Period in the Motions of the Earth and Venus." In the following year he wrote for the British Association a very interesting "Report on the Recent Progress of Astronomy," a little work which may be read still with great profit by every student of astronomy. Various lacunæ of our science discovered on that occasion were filled up by Prof. Airy in the next year by his papers "On the Mass of Jupiter."

When Mr. Pond, the fifth Astronomer-Royal, resigned in the year 1835, Prof. Airy was appointed his successor, by Lord Auckland, first Lord of the Admiralty, and at the same time he was elected President of the Royal Astronomical Society. During the forty-three years that have elapsed since his appointment as Astronomer-Royal, Sir George has always been most keenly intent in promoting astronomy and science in general in every way.

He has equipped the Royal Observatory at Greenwich with a series of new instruments of very exact construction, all made after his own designs, many of them invented by himself. The first of the new instruments was erected in the year 1847. It was constructed in as few separate parts as possible, and no important parts were connected by small screws, in order that the instrument might possess the greatest amount of firmness. The end to be attained by its use was to make observations out of the meridian as accurate as observations in the meridian, and its main object of observation was the moon. It must be recollected that the moon can very seldom be observed with the meridian instruments before her first quarter and after the last. The altazimuth was designed to obtain observations of her as often as she was visible in the sky and, I am sure, every astronomer will agree that the erection of this instrument was a most important innovation. Its great services were fully acknowledged many years later, when the greatest errors in Burkhardt's Tables of the Moon were shown to exist in parts of her orbit never accessible to meridian observations. The number of days on which the moon is observed by this instrument is nearly double that of the observations in the meridian.

At the end of the year 1850 the new meridian circle was erected, the object-glass of which, made by Mr. Simms, has 8 inches clear aperture and 11 feet 6 inches focal length. With this instrument there was also introduced a great change in the observing routine at Greenwich, the transits and the zenith distances of the stars being now taken by one astronomer at the same time. Nearly simultaneously the American method of observing transits was adopted.

The Troughton zenith-sector, found by Mr. Airy at the Observatory, had given much trouble, and various alterations had not improved the results obtained by it. It was therefore dismantled in the year 1848, and the "reflex-zenith-tube" erected, an instrument admirably calculated for observing the small changes in the zenith distances of γ Draconis, the Greenwich zenith-star.

When all these new instruments were well in working order, Mr. Airy directed the attention of the Board of Visitors, in the year 1855, to the fact that the extra-meridional apparatus was by no means fit for the present wants of astronomy. A large object-glass (12 French inches in diameter) was, in consequence, procured by the Astronomer-Royal from Mr. Merz, and was mounted

in the manner which was formerly adopted by Mr. Airy in the mounting of the equatorials at Cambridge and Liverpool. With the inauguration of the new equatorial in the year 1859 the change from the old state of the Observatory was complete. There was not then a single person or instrument in the Observatory that had been there in Mr. Pond's time.

A fifth new instrument was planned by Mr. Airy in the year 1869, and erected in the following year in the Royal Observatory, in order to decide the most delicate question of the dependence of the measurable amount of sidereal aberration upon the thickness of the glass or other transparent material in the telescope. The tube of a telescope (the lenses of which were ground to proper curves) was filled with water, and the telescope mounted as zenith-sector. Several years' observations of γ Draconis did not reveal a perceptible difference in the constant of aberration found in this manner from the value generally adopted.

Of the instruments invented by Sir George Airy I may mention also the double-image micrometer, a very useful apparatus, if thoroughly investigated; the eye-piece for correcting the atmospheric dispersion; the orbit-sweeper, a most ingenious contrivance to detect comets approaching perihelion passage, the time of which cannot be exactly fixed.

The observations were made, under his own responsibility, during nearly half a century, without interruption, reduced with great care, regularly printed, and—a very essential thing—very liberally distributed. They form a vast collection of the most important fundamenta of astronomy. Every year Sir George publishes a report on the work done in the Royal Observatory; these reports form a series which will be of the greatest use for the writer of the history of astronomy and science in general in the nineteenth century.

Since the year 1833 the incessant activity of Sir George Airy had been directed to an undertaking, proposed to astronomers by Bessel, in the preface of his "Tabulæ Regiomontanæ," viz., the reduction of the Greenwich lunar and planetary observations since 1750. This most arduous task was completed in the year 1848; and we may say that our present tables of the motions of the moon and the planets rest, for the greatest part, on those bulky volumes, containing these reductions.

But not only was this immense magazine of dormant facts opened by Sir George to science: he reduced also the observations of Groombridge, of Catton, and of Fallows, the first Astronomer-Royal of the Cape of Good Hope. The importance of these reductions is not limited by the usefulness of the observations themselves to the astronomer by the appearance of the "Star-Catalogue," containing Mr. Groombridge's most valuable observations. Mr. Johnson, for instance, was induced to undertake those beautiful observations of circumpolar stars, forming afterwards the Radcliffe catalogue of stars. Prof. Hansen, of Gotha, was very materially supported by the Astronomer-Royal in finishing his great work on the moon and calculating tables of her motion.

The pressure of daily work and the responsibility of keeping up the Greenwich series of solar and lunar observations absolutely uninterrupted, did not prevent the Astronomer-Royal from taking up other scientific questions of the day. In the year 1842 he made a voyage to Turin, in order to observe the total solar eclipse; the same object induced him to visit Gothenburg, in Sweden, in 1851, and to organise, in the year 1860, the famous *Himalaya* expedition to Spain.

His great interest in every branch of his favourite science is evinced by the recent introduction at Greenwich of heliographic and spectroscopic services.

In the year 1847 Sir George went to Russia for the purpose of inspecting the new Russian Central observatory. It is highly gratifying to read with what absence of prejudice the great astronomer expresses himself in regard to this observatory, the personal establishment of which and the construction of instruments is so very different from those at Greenwich.

Magnetical and meteorological observations were not made at Greenwich before the time of the present Astronomer-Royal. Sir George Airy proposed to the Government to make them at Greenwich, and since 1838 the new Magnetical and Meteorological Observatory has been in activity. He introduced for this department also the self-registering instruments constructed by Mr. Brooke.

In later years the Astronomer-Royal has been oppressed with the difficulty of making the meteorological observations practically available. With a store of records, extensive, accurate, and rich, beyond any other which exists, he does not see a probability of physical connections or physical laws sufficiently strong to induce him to enter confidently on an expensive comparison, and he expresses strongly his opinion, that the want of meteorology at the present time is principally in suggestive theory.

Only very briefly can I mention his very useful experiments on iron-built ships, for the purpose of discovering a correction for the deviation of the compass, which resulted in a system of mechanical corrections, universally adopted; his researches on the density of the earth by observations in the Harton Colliery; his extensive aid to Government in recovering the lost standard for measures; in fixing the breadth of railways; in introducing a new system for the sale of gas, &c. All these transactions have proved Sir George Airy "the thorough man of business." Indeed, the promptness of his correspondence and his kindness in answering every scientific inquiry in the most minute manner, is most remarkable and seldom to be met with in so profound a philosopher.

In recent years the Government intrusted to his care the equipment and instruction of the British Expedition for the Transit of Venus, a subject which had engaged the attention of the Astronomer-Royal during thirty years, and had induced him to write a number of most important papers on the matter. In the year 1848 he gave a series of lectures on this difficult subject at Ipswich. The whole responsibility for reducing the observations made during the transit rests likewise with him. Much of his time has, during the last few years, also been spent in promoting the lunar theory by a method of his own.

Sir George Airy has, of course, deservedly received the recognition of his country and the scientific world in general. He is medallist of the French Institute,

of the Royal Society (twice), of the Royal Astronomical Society (twice), and of the Institute of Civil Engineers. Most scientific societies are proud to have his name on their list of members; he is one of the eight Associés Étrangers de l'Académie des Sciences à Paris.

A. WINNECKE

FOREIGN ORDERS

IN several articles and letters in vol. viii. of NATURE the question of the conferment of foreign orders on British subjects, so far as it concerns men of science, was pretty thoroughly discussed, as well as the proposal made in Parliament, in 1873, to establish an order of intellectual merit. The subject has again come up in connection with the distribution of awards at the close of the Paris Exhibition, and there has been much disappointment and even bitterness of feeling expressed at the refusal of our Government to allow British subjects to accept the coveted Cross of the Legion of Honour. It is well known that many of our men of science, as well as others, possess foreign orders in abundance, and that our Government takes no notice unless consulted, when, on the ground of some antiquated regulations, it thinks it its duty to refuse permission to accept such orders. If not illegal, it is at any rate weak and childish on the part of Government to take such a course, worthy of the days of "good" Queen Bess, who wished her dogs to wear no collars but her own. In the case of the Exhibition awards it has been shown that this decision on the part of our Government falls with peculiar hardship on British exhibitors. It will very naturally be inferred by the general public that as a body they occupy an inferior position to foreign exhibitors, who are allowed to accept the great French honour, which is conspicuous by its absence from the awards in the British department. It is especially hard, we think, upon those who have served on the British jury. From some parsimonious caprice on the part of Government no allowance was made to those who served as jurors at the Paris Exhibition, and the eminent men of science who gave up their time and knowledge for the benefit of the country and the world not only go entirely unrewarded, but must have been seriously out of pocket. One case we know of—and we believe it is not the only one—where a well-known chemist, besides incurring serious expense, worked so hard as to materially affect his health, and all not even for bare thanks.

So far as we ourselves are concerned, we are not anxious to see men of science eager to obtain, or easy to be satisfied, with such honours as those which, if they are simple enough to ask, they are told they must not accept. Our own Government is niggardly enough in its recognition of the services done by the scientific worker to his country; and how can it be otherwise with a Cabinet that has scarcely a member, we believe, who knows the difference between a telescope and a telephone. Fortunately for his self-respect, the purely scientific worker, however eminent he may be as such, is rarely, if ever, embarrassed by the offer of honours from our own Government. These are reserved for the militant and civil services, where, as a rule, they are least requisite, seeing that those who obtain them are generally pretty well paid for their zeal. As for what is called the "honour" of knighthood, it has now become so common, so easily obtainable, that the mere offer of it must make one suspect that after all he must be regarded by its dispensers as a very tenth-rate man. We know of a humble grocer in a small country town in the north in which a statue erected to the late Prince Consort was unveiled by the Queen, when the decent man happened to be provost; of course he was dubbed "Sir," and his life was ever after rendered miserable by the waggish little urchins of the town, who would gravely pass their cans across the counter for "A bawbee worth o' treacle, Sir Dawvid." And C.B. is rapidly becoming little better, so that virtually in this country there is no imperial honour attainable by the purely scientific worker, however eminent, which his self-respect would permit him to accept entirely without question.

As to the creation of an order of merit for men of high eminence in science, literature, or art, we have already expressed our opinion. In the present state of things it is better to let the existing chaos alone. Who is there among those who would now have the dispensation of such an honour who is capable of selecting those really most worthy of it? Had we a Minister of Science, with a council of scientific specialists to guide him, then there would be some chance that such an honour would reach those who really deserved it; but at present it is hopeless. Indeed the devotee of scientific research would much prefer that Government, if it desires to do science honour, would do so by giving her substantial aid to pursue her work, than that it should load her servants with all the honours at her Majesty's disposal.

Still with the parsimony both of money and "honours" at home, it is peculiarly hard that scientific men can accept the distinctions which foreign governments are ready enough to award only as if they were contraband goods. Literature and art are abundantly rewarded in both ways, but, like virtue, science, on which the substantial welfare of the world depends, is its own reward; but this, unfortunately, is not marketable. We trust that the present outcry will lead to a modification of the unreasonable regulation as to foreign orders.

THE "ENCYCLOPÆDIA BRITANNICA"

The Encyclopædia Britannica. Ninth Edition. Vols. vii. and viii. (*Deacon* to *Fakir*). (Edinburgh: A. and C. Black, 1877-78.)

IN the article ENCYCLOPÆDIA, which finds a place in the second of the volumes now before us Mr. Lyons defines an encyclopædia as a book treating of all the various kinds of knowledge. The definition applies well enough to the older encyclopædias, composed when it was still thought practicable to set forth in a single work all that was worth knowing in science and art. To define the province of a modern encyclopædia is a more difficult task, which will probably be avoided by every one who is not compelled either to plan and edit a work of the kind, or to review an editor's plan. Smaller cyclopædias, on the type of the "Conversations Lexicon," naturally limit

themselves to such an abstract of miscellaneous information as may be of service to the ordinary reader. No article is admitted which requires for its comprehension either special preparation or special application. The "Encyclopædia Britannica" aims at something more than this; it addresses itself to the general readers, but it also has a real value for students. On this large plan it becomes very difficult to adjust the respective claims of the two classes to whom the work appeals, and the practical solution must probably be to give what is likely to attract purchasers of a special class without repelling the general public in larger numbers. This seems to be what Prof. Baynes has in view when, along with such articles of general interest as Mr. Freeman and Mr. Gardiner's ENGLAND, he gives us on the one hand an abstruse essay on ELASTICITY, bristling with mathematical formulæ, and on the other a selection of hints for success in playing EUCHRE.

The chief difficulty in successfully carrying out so large a plan lies in the scientific monographs, and more especially in the treatises on subjects which cannot be thoroughly handled without mathematics. In the ninth edition these articles are of a very high class. The editor must be congratulated who, within the two volumes now before us, has articles by Prof. Clerk Maxwell on DIAGRAMS, DIFFUSION, and ETHER, by Sir W. Thomson, on ELASTICITY, by Prof. Cayley on EQUATION, and by Prof. Chrystal on ELECTRICITY. All these are admirable pieces of scientific writing of different kinds. The article Electricity is a singularly clear and well-arranged exposition, which, if printed as a separate volume, would form the best possible text-book for students who are well advanced in pure mathematics. On the other hand Prof. Cayley's account of Determinants and the Theory of Equations is not of the nature of a text-book, but can be appreciated only by those who have some knowledge of the subject. Prof. Maxwell's papers, full of his usual quaint illustration and felicitous turns of expression, sometimes amounting to scientific epigram, would be perfect encyclopædia articles if the ordinary reader possessed accurate habits of thought on physical subjects without actual physical knowledge. But in the present condition of things they are most likely to serve an opposite purpose in clarifying the thoughts of those who have already some reading on the topics dealt with. All these papers suggest the question whether an Encyclopædia of general information ought not to limit itself to articles which can be followed by a painstaking reader who has no other preparation or assistance than he can find in the Encyclopædia itself. Yet, on the other hand, we are grateful to the editor who has opened his pages to so much valuable writing which otherwise might never have appeared at all.

In passing from this topic we may notice, in the article ENERGY, a curious over-statement of the inference to be drawn from the ingenious speculation associated with what are called Prof. Maxwell's "demons." We are told by Mr. Garnett that this speculation "shows that the principle of dissipation of energy has control over the actions of those agents only whose faculties are too gross to enable them to grapple with those portions of matter in virtue of the relative motions or relative positions of which the energy exists with which they are concerned." Mr. Garnett has forgotten the trap-doors which the hypothetical demons command. In fact the simplest form of the hypothesis would be to drop the demons, and make the trap-doors themselves intelligent beings, possessing resistance, and capable of moving without expenditure of energy. Such beings would not be controlled by the law of dissipation, but they would differ from all agents known to observation in a more essential point than the possession of subtler faculties.

Passing over less abstruse branches of science, which are well represented in these volumes by Prof. Huxley, Mr. Wallace, Prof. McKendrick, and other names of mark, we must devote a few sentences to the articles which deal with the history of human life and the movements of human thought. A valuable feature in the editor's plan is the prominence given to subjects connected with the ideas, habits, and traditions of primitive man. In this line we have an excellent article on DELUGE, by Mr. Cheyne, and a very interesting paper on DEMONOLOGY, by Mr. E. B. Tylor. Some of the facts adduced in the latter article may probably need further sifting. We do not think so highly as Mr. Tylor does of Maury's book on magic. Lenormant's work on Chaldean magic must be used with reserve, and, to mention but one other point, the theory of a schism between Indians and Iranians, connected with a change in the meaning of the word *deva*, is open to grave objections. Among properly philosophic papers Mr. Sully's EVOLUTION is valuable from its comprehensive survey of the history of the subject, while Mr. Sidgwick's sketch of the progress of ethical speculation, which may be said to replace Sir J. Mackintosh's dissertation, written for the seventh edition of the "Encyclopædia," gives striking proof of the real advances that are being made in what is often regarded as the most stationary department of human thought.

The articles in Geography, History, and Biography, which to the general reader form the most valuable part of a book of reference, are as a rule very good. Subjects of special importance or attractiveness are treated in these as in the previous volumes by writers of distinguished position and special information, while the minor articles speak well for the diligence and scholarship of the permanent staff. In a few cases material taken over from the last edition might with advantage have been more strictly revised. In conclusion, we would offer one or two hints for this part of the work. It is very desirable that bibliographical references should be as complete and uniform as possible. At present there is considerable inequality in this respect, both as regards the statement of sources of information, and the enumeration in the biographical articles of authors' works and their editions. In such an article as that on DICTIONARY it would be unfair to expect completeness or absolute freedom from error, and one does not complain of such a slip as the reference to Rabbi Iona ibu Ganach's Lexicon as still unpublished, when in reality it has been edited by Neubauer. But an enumeration of modern books upon Ethiopic ought not to have omitted Dillmann's Grammar and Lexicon, and an article on EPHESIANS is not complete without reference to Holtzmann.

There is one other point which a reviewer ought not to pass over. In every encyclopædia there must be a certain

amount of mere compilation by men who have not made an independent study of the subject dealt with. But the public has a right to expect that work of this kind shall not appear with the signature of men of known literary standing. In making this remark we have an eye on Principal Tulloch's article, EUSEBIUS. The whole article is the slimmest literary hack-work, and the notice of Eusebius' works contains mistakes which can be best explained by supposing that the writer was hastily abridging from Smith's "Dictionary," from which several sentences are copied almost word for word. What is said about the "Chronica" is one tissue of error and confusion. The whole work is described in terms that apply only to the second book. The Latin translation of the Armenian version appears to be confounded with the original work, and there is not one syllable to indicate that only fragments of the latter exist. So, again, the "Præparatio Evangelica" is described as a collection of facts and quotations from the works of the ancient philosophers, without any allusion to the important fragments of ancient historians which it embodies. The celebrated Nitrian MS. of the Theophania is said to have been found in an Italian Monastery. And finally, there is no account of the editions of Eusebius, not even of Schöne's great edition of the Chronica.

OUR BOOK SHELF

Cyprus: its History, its Present Resources, and Future Prospects. By R. Hamilton Lang. Illustrations and Maps. (London: Macmillan and Co. 1878.)

THIS, we venture to think, is really the most important and useful contribution to a knowledge of our new dependency that has been published since the surprise was announced. Mr. Lang's long residence in the island and his position there have given him exceptional advantages to acquire a thorough knowledge of it in all its aspects. He has, moreover, made diligent research into the history and antiquities of the classical island, and has succeeded in presenting in this volume a clear and instructive account of these. Mr. Lang maintains that Cyprus must have had a somewhat civilised population before the arrival of either Greek or Phœnician colonists, and that the remains of early writings which have been discovered prove these earlier inhabitants to have been Aryan, and not Semitic, and probably of the same parent stock as the Greeks. About one-half of the work is devoted to the history of the island. In the chapter devoted to agriculture and produce Mr. Lang shows that, in this respect, the capabilities of the island are very large, and that, with improved systems, it might really be made one of the most fruitful of our dependencies. He himself made long experiments in farming, and with the most satisfactory results; and for intending settlers in the island this part of his work will prove of much value. Mr. Lang gives a condensed account of M. Gaudry's researches on the minerals of the island; but our knowledge of its geology is by no means satisfactory, and we trust with Mr. Lang that no time will be lost in getting a thorough geological survey of the island. The chapters on archæology and rock tombs and their contents are of special interest; and of great practical value is the chapter devoted to "my farm in Cyprus." Mr. Lang is very hopeful of the results of this annexation. One of the most interesting results, so far, in our opinion, is his own work on the island. The five beautiful maps by Stanford add much to the scientific value of the work.

Studies in Physical Science. By W. J. Millar, C.E. (London: Marlborough and Co.)

IT is difficult to imagine what want this little book of 102 pages is intended to fill. In form and matter it appears not unlike a schoolboy's notes of some popular lectures. If any one really desires simple and accurate information on the elementary propositions of physical science he can have no difficulty in finding it in the many brief works which have recently appeared from the pens of the foremost authorities on each special branch. What are we to make of the following paragraphs when it is stated in the preface that "the whole has been carefully revised and the most recent scientific views considered"? "The vapours which ascend from the surface of our globe are the channels by which the electricity of the atmosphere is supplied. Evaporation is an active source of electricity, and thus the clouds, which are made up of hollow vesicles of aqueous vapour filled with air, contain a considerable quantity of electricity stored up and ready to be discharged." Concerning the telephone, the author remarks that its "action appears to depend upon the principles of magnetism, electricity, and acoustics." Further quotation is needless.

Hydrostatics and Pneumatics. By Philip Magnus, B.Sc., B.A. (London: Longmans and Co. 1878.)

THIS work forms the seventh volume of the "London Science Class-Book Series," jointly edited by Prof. G. C. Foster and the author of the work before us. The books are intended for "school purposes," but we cannot imagine that the author of the "Hydrostatics and Pneumatics" can be acquainted with science teaching in schools, or its requirements, to judge of his treatment of the question of the relative densities of air at different heights (p. 125), or of the method of finding the difference of height of two stations by means of a barometer, supposing the temperature and force of gravity constant (p. 127). Or again, if we glance at pp. 18, 41, and 58, we perceive at once that the treatment is of far too complex a nature to be taught to young boys in Form who probably have one hour, or at most two, to devote to the subject in a week. For advanced boys in Upper Fifth and Sixth Forms the book will undoubtedly prove useful, provided they can devote several hours a-week in any one Term to the subject. The work is clearly written for the most part, and there are but few omissions. We do not notice, however, any mention of the experiments of Mr. Tomlinson and Prof. Van der Mensbrugge on surface tension, nor of the experiments of Venturi, Bernouilli, and Magnus, on the lateral action of a fluid in motion.

The Bulb Garden; or, How to Cultivate Bulbous and Tuberous-rooted Flowering Plants to Perfection. By Samuel Wood. (London: Crosby Lockwood and Co. 1878.)

IT is not too much to say that the best part of the present book is its cover. The binder has certainly performed his part well, and produced a pretty-looking book, but we must look no further than the cover for a word of eulogy. We have only to turn over the fly-sheet to read the title-page, and we are startled by an extraordinary gaudily-coloured plate, which suggests a design for a patchwork counterpane, but which, upon closer examination, turns out to be one for a bulb garden. With this we are not prepossessed; and the next coloured plate still further lessens our appreciation of the author's artistic taste. We leave the plates and turn to the text, in the hope of finding the literary character of the book such as to make amends for its artistic shortcomings; but still we are disappointed, for, when we find such plants as *Dielytra*, *Tritoma*, *Lychnis fulgens*, the Hellebores, &c., classed as bulbs, we are inclined to ask, Does the author know what a bulb is?

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

American Exploration

North-western Wyoming and Yellowstone Park

THE letter from Capt. W. A. Jones in NATURE, vol. xviii. p. 667, seems to show a feeling of irritation on his part at the notice of his Report upon a reconnaissance in North-western Wyoming, which appeared in your columns some months ago. There was not in that notice any expression which could be interpreted into a want of recognition of the ability with which he had conducted the operations committed to his charge. Of course the desirability of these operations, and whether they were important enough to justify the expedition, are matters of opinion regarding which we may differ from Capt. Jones, without for a moment casting any reflection upon him. One object of the remarks which have displeased him was to point out the need of some central authority to control the various exploratory surveys in the United States, and prevent a needless expenditure of labour and money in the reduplication of work by parties operating without any concert with each other. This subject was brought before the notice of Congress, and a special committee was appointed to consider it and take evidence. The report of the sittings of this committee shows a most laudable desire of patiently getting at the truth. It recommends that the Engineer Department should not be authorised to undertake any surveys except such as might be required for purely military purposes. Of course, Capt. Jones, as an officer of engineers, thinks this a very "one-sided report." But the decision of the committee met with the approval of the great body of scientific men in America, whose only desire could be the best means of facilitating the thorough exploration of their great country. And the decision was equally welcomed on this side of the Atlantic by men who knew nothing and cared less for the personal bickerings of the different Government departments and surveyors in the United States. Capt. Jones speaks of a quotation made from the committee's report as being "out of the pale of decent characterisation." It was nevertheless the deliberate statement of men who gave their testimony upon oath. Strong expressions of this kind are apt to raise more than a doubt as to the strength of the cause in support of which they are adduced.

With every wish to do full justice to Capt. Jones and his associates, I feel that there is a far larger question behind his complaint than the mere recognition of their contributions to our knowledge of the North American Continent. To students of science in Europe it is a matter of small moment under what Government department or by what organisation of surveying parties the work of exploration is carried on. Most cheerfully do we recognise the labour, the patience, the courage, the physical endurance, the sagacity of observation, and the admirable powers of generalisation which during the last fifteen or twenty years have been bestowed by the various departments upon the task of unravelling the geography and the geological history of vast tracts of the United States. Thus the Department of Engineers has earned our lasting gratitude for the thoroughness and accuracy of its contributions; and had it no other record of its work than the magnificent series of quartos relative to the survey of the 40th parallel, that department would have raised an enduring monument of its scientific prowess. But besides the exhaustive reports of Clarence King and his associates, the Bureau of Engineers has issued many other most admirable volumes, not least among which is that of Capt. Jones himself. Then the Department of the Interior has for a number of years enjoyed the lustre shed upon it by the researches conducted by Dr. F. V. Hayden. Capt. Jones speaks of this distinguished explorer in the generous spirit of a true lover of science. To have succeeded as Dr. Hayden has done means not merely that he has conquered the physical difficulties of unexplored regions, that he has possessed mental powers capable of grappling with the many difficult problems presented by these western territories, that he has that judgment and *bonhomie* which have enabled him to select and keep round him year after year such a band of skilled observers as has included the names of Marvine, Peale, Endlich, and Gardner; but—what, perhaps, demands greater skill and patience than all the rest—that he has had the self-denial and courage to canvass the Congress, and literally persuade or coax its members into granting the necessary appropriations. It is all very well to talk of the dignity of science; but science cannot get on without money; and to get money she must, in America at least, "hiding her dignity in her necessity, be fain to shuffle, to hedge and to lurch." That a man of Dr. Hayden's powers should require to go through this annual penance is sad to think, but, as matters stand, he must either go through it or give up his explorations. He has chosen the former alternative. That in so doing he has done wisely must be granted not only as regards the prosecution of his own operations, but indirectly in reference to the other explorations sanctioned and paid for by Congress. The altogether admirable surveys conducted by Major Powell, for example, also under the Department of the Interior, have a powerful backing in the prestige of Dr. Hayden's work. The Coast Survey has long been a model of accurate and exhaustive methods of research.

Capt. Jones remarks that "in the cause of science a little duplication and reduplication are things not to be sneered at." A few lines further on he says that "exploring is but imperfect work, so far as the survey, which is its foundation feature, is concerned;" and that "observations for longitude with any known portable instruments are painfully erratic, unless there be abundant time." No one ever "sneered at" the repetition of surveys by different exploring parties; but every one, unless perhaps a candidate for future employment in these expeditions, must admit that it is a pity to reduplicate work which is so confessedly "imperfect" and "painfully erratic." Let the first preliminary surveys be made, but let them be done systematically, so that different surveying parties shall work in concert, and not blindly re-survey each other's ground. If any subsequent reduplication can be undertaken let it be again done methodically, with the view of correcting and filling up the first rough outlines. This requires some central controlling authority, and it is the absence of this authority which has led to the misunderstandings and dispeace. At present any man who can gain the ear of Congress, and get an appropriation of so many thousand dollars may go and explore as he pleases, and very much where he pleases, provided only he renders account to the Department of the Interior for the disbursement of the money. Of course this want of supervision leaves the explorer untrammelled by the official bonds which would hamper him if he were surveying in a longer-settled country. He is entirely his own master, can arrange his work and dispose his staff precisely as he judges best for the sake of progress and efficiency. No doubt these are enormous advantages. But then, on the other hand, he must stoop to button-hole the Congress-men, and spend many valuable weeks in getting them to see that they ought to continue, or even to increase their grants to him. He has no departmental organisation behind him on whose support he can rely, and the mere passive existence of which would often of itself be enough for his purpose. He must every year fight his own battle over again against competing organisations, rival explorers, and utterly indifferent members of Congress. It was in this respect that the Department of Engineers proved so formidable an antagonist in the conflict which led to the appointment of the Congressional Committee referred to by Capt. Jones. It is an organised department of long standing with traditions and military *esprit de corps*. The several explorers under its wing had not to stand each with his back to the wall fighting for his own. Their cause was taken up as a general one by the engineers and the army, and it was only after much evidence had been led that the Committee agreed upon that "one-sided" Report which the engineers naturally resent.

If all the surveying work undertaken, or at least paid for, by Congress, were placed under some central control, enough would probably be done were precautions taken to secure that the various operations were carried on in concert and not in utter ignorance of each other, and that the maps and memoirs were issued in some one general form which would facilitate reference. The various explorers need not lose their practical independence. They might remain as unhampered as ever, and be left free to make their own dispositions within certain general limits. Such a central board ought to charge itself with securing the necessary money grants from Congress, and thus save its scientific men from the degradation and loss of valuable time in per-

sonally canvassing the members. The gain to the explorers in this way would surely far more than compensate for any fancied loss of independence.

One word more about American exploration, and it shall be one of unqualified admiration. When a member of any of the branches of the public service in this country which are concerned with scientific publications contemplates the style in which such publications are prepared and issued in the United States, he finds a spirit of envy rising uppermost within him. Quarto after quarto, atlas after atlas, all published in the most sumptuous style as regards paper, printing, engraving, and chromo-lithography, are poured out from the American national press, yet at such prices as not to place them beyond the reach of all but the rich. The number of copies of these costly works actually distributed gratuitously is almost incredible. They are scattered lavishly over Europe, not merely to public libraries, but even to private students of science whose names are known to few of their own countrymen save those who read their writings in the scientific journals. Such open-handed generosity makes many a recipient of the gifts accept them almost with reluctance when he knows how little we in this country can offer in exchange. It is not that we are idle, or that the results of our labours would not furnish materials for important memoirs. But they manage these things better in the States. Perhaps we may profit by their example some day.

ARCH. GEIKIE

Discovery of a Scottish Crannog

WILL you kindly allow me, through the columns of NATURE, to draw the attention of archæologists to a recent discovery of an ancient crannog on the farm of Lochlee, near Tarbolton, Ayrshire. It appears that formerly a considerable portion of what is now arable land, and divided into several fields, was occupied by a loch with mossy banks and bottom, and that about forty years ago its outlet was deepened and its whole area completely drained. When this was done a small mound was observed near the outlet of the lake and about 100 yards from its nearest bank, which, from its artificial appearance and the discovery of two canoes in the bed of the lake, then attracted the curiosity of a few observant people in the neighbourhood, but led to no further result, and soon the whole affair was entirely forgotten. Just now the same locality is being re-drained under the direction of Mr. Turner, factor for the Duke of Portland, and his men, while engaged in cutting the main drain which happened to pass through a small bit of this mound, came upon the peculiar structure of the crannog. Fortunately this came under the cognizance of Mr. James Brown, Tarbolton, who wrote a note to Mr. J. Anderson, Keeper of the Museum of the Society of Antiquaries of Scotland, drawing his attention to this discovery. This gentleman immediately wrote to R. W. Cochran Patrick, Esq., of Woodside, Secretary of the Archæological Society for the counties of Ayr and Wigton, who lost no time in visiting the district, and at once recognised the nature and importance of the discovery. Meantime Mr. Turner and myself made several visits to the locality, in the course of which we observed that three rows of closely-set wooden piles, six feet apart, extended from the mound to the mainland—presumably forming the foundation for a wooden gangway. The tops of these piles, except in a very few instances, are below the surface of the soil. At the same time the men dug up a canoe, in a good state of preservation, hollowed out of one log, and tapering rapidly and uniformly at both ends. It was lying about 150 yards from the mound, and the highest portion of it was three feet below the surface. It measures ten feet long, two feet six inches broad, and one foot nine inches deep. It was then arranged that a careful exploration of the mound should be made, and accordingly systematic excavations were begun on Tuesday last, in presence of Messrs. Turner, Patrick, Anderson, Dr. McDonald, Ayr, and myself, and are now being prosecuted with great vigour and success. As a detailed account of whatever discoveries may be made, together with plans, sections, and drawings of the crannog, will be published in the Collections of the Archæological Society of this county, under the superintendence of its accomplished secretary, Mr. R. W. Cochran Patrick, it is unnecessary for me to give here more than a few remarks, just sufficient to convey to your readers some idea of what has already been done and may yet be expected. Guided by the tops of a few upright piles which just appeared on the surface, a broad trench was dug right round the mound. Some of these piles, all of which were formed of young oak trees, were found to terminate in holes in large horizontal beams, while others appeared to be driven into the muddy bottom and surrounded by thick planks of oak, young trees, and brushwood, amongst which beech, birch, and hazel were readily recognised. On the north-east side, and only about one foot below the surface, were two series of horizontal beams of oak from five to six feet long, and about five feet apart, each of which had two square-cut holes near its extremities, through which upright piles penetrated and were firmly fixed by wedges of wood. These mortised beams rested on round trees which lay horizontally but pointed in various directions. Conterminons with these beams and running towards the centre, there was a rude and very much decayed platform formed of rough planks and saplings lying on large beams of split oak trees. The oozing of water prevented the complete exposure of the mossy bottom on which this curious structure was reared, but it was ascertained to be from seven to eight feet below the present surface. In all the parts that were examined large stones were found interspersed with the woodwork, and the diameter of the foundations of the mound was estimated at about twenty-five yards. A trench was then dug across this circular area, and near the centre we exposed two stony pavements, one lying immediately above the other, the space between being 2½ feet thick. These pavements rested on a thick stratum of clay which extended for several feet all round, gradually thinning towards the rim, and, from the abundant remains of ashes, charcoal, and burnt bones, evidently formed fire-places. About two feet below the lower pavement another layer of clay, together with ashes, charcoal, &c., was observed, and though not yet excavated, we concluded that it must have been a third fire-place. Nearly on a level with this was a layer of chips of wood as if cut by an axe, and underneath this was a layer of turf with the heather part downwards. On pressing the spade still further down it struck a log of wood. The perpendicular height from this log to the top of the upper pavement was seven feet nine inches. All these fire-places were below the level of the water before the first drainage was made. As it is ascertained that previously there was no island to be seen, the whole island must have sunk very much since its original structure. Round these fireplaces were the remains of a series of seven or eight large piles with their bases cut flat and resting on the floor of the middle pavement or a few inches below it. These upright piles inclosed a somewhat circular area, with a diameter varying from ten to fifteen feet. The articles hitherto discovered in the interior of the mound consist of querns, hammer-stones, bone-chisels, and lance-like objects, a spindle-wheel, wooden implements like clubs or paddles, &c., deer-horns, some cut across and marked with holes and other markings, numerous boars' tusks, and a great assortment of bones and teeth belonging to various kinds of animals. With the exception of a singular three-pronged iron instrument found in the large drain outside the mound and a rusty piece of iron shaped like a door-handle, picked up very near the surface of the mound, not the slightest trace of either iron or bronze has been discovered. A piece of red pottery, said by a competent authority to be so-called Samian, found in the same drain and near the same spot as the iron implement above alluded to, and the half of a grooved bead of the size of a hazel-nut and covered with a greenish pigment, are the only fragments of pottery as yet brought to light.

Kilmarnock, October 21 ROBERT MUNRO

Power of Stupefying Spiders Possessed by Wasps

MR. ARMIT's letter, from Queensland, on this subject (NATURE, vol. xviii. p. 642) is, to my mind, of great interest as showing that the habits of insects are the same at the antipodes as on our side of the globe. I was well aware that the spiders were stupefied (or paralysed) and not killed, and that the use made of them by the wasp was as a nidus for her ovum, and to serve as fresh provisions for her larvæ when hatched. Of course if killed they would be useless for this purpose. We have a wasp of similar habits, but he makes use, in the cases in which I have watched his operations, of the larvæ of the garden white butterfly, which are rendered passive and helpless, but not killed, in a similar manner.

I make alternative suggestions for further, and if possible microscopic, examination into the matter. First, are the wounds producing this insensibility inflicted with the sting, or by an ovipositor in the act of inserting the ovum? Is the egg in the case of the wasp, as with the ichneumon, inserted in the insect to serve by and by as food, or outside it, in the cell? If

the latter, then the punctures are, no doubt, true stings; and I make the alternative suggestion that the wasp is guided by its instinct—as the larva of the ichneumon is when feeding—to select for attack parts of its victim not vital, where the injected acid produces insensibility or partial muscular paralysis, but not death. Because in the rare cases in which a wasp or bee struggling in a spider's web succeeds in stinging his captor, in anger and at random, the spider dies. May the observation made in your columns by a recent correspondent, on the self-administration, through the mouth, of the poison of the sting by wasps and bees under chloroform not point to a stupefying property in the acid when taken, as the natives of India take the poison of venomous snakes, into the stomach, and not directly into the circulation of the blood? There is good work here for an observer with patience and a good field microscope.

Bregner, Bournemouth, October 19 HENRY CECIL

A Fossil Plant—Misquotation

IN an article on a fossil plant from the Isle of Man, in NATURE, vol. xviii. p. 555, the following sentence is attributed, apparently on the authority of Mr. Leo Lesquereux, to my report on the Devonian and upper silurian plants of Canada: "that these fragments are probably originating in the upper silurian of Gaspé; that as they are found in the lower part of the limestone which underlies the Devonian Gaspé sandstone and become more abundant in the upper beds, this suffices to indicate the existence of the neighbouring land, probably composed of silurian rocks and supporting vegetation."

On referring to the report in question, I find that the original of this strange statement stands as follows:—

"These remains of *Psilophyton* occur in the lower part of the limestone, but are more abundant in the upper beds, and they suffice to indicate the existence of neighbouring land, probably composed of lower silurian rocks, and supporting vegetation."

I have no doubt that Mr. Lesquereux quoted from memory, and probably supposed that he was expressing my meaning, but an English writer should have referred to the original.

I may add that the specimen referred to in Mr. Binney's article does not exhibit the characters of my genus *Psilophyton*, which does not contain "fucoids," but land plants of the rank of acrogens, and of which not merely the external forms, but also the internal structures are described and figured in the report referred to. The plant in question much more closely resembles *Buthotlephis harknessii*, Nicholson, from the Skiddaw slates. J. W. DAWSON

McGill College, Montreal, October 5

Sense of Fear in Chamæleons

DURING the past summer I have kept five chamæleons in captivity, and have repeatedly observed their terror and rage when confronted with snakes. When a large Algerian chamæleon (*C. vulgaris*), now in my possession, perceives a common snake (*Tropidonotus natrix*) wriggling in his vicinity, he at once inflates his body and pouch, sways himself backwards and forwards with considerable energy, or walks rapidly away with his body leaning over in the direction furthest from the snake, opening his huge cavernous mouth, and hissing and even snapping at what he evidently regards as his natural enemy. At the same time his body assumes an almost instantaneous change of colour, and is quickly covered with a large number of small dark brown spots. It is curious that similar symptoms of fear and anger are displayed when a lizard (*Lacerta viridis*), or even a tree-frog (*Hyla arborea*) is exhibited to him. The climax of grotesque nervousness was, however, reached one day, when the sight of a child's doll produced the like effect; in this case, it is probable that the glass eyes of the doll, giving to it the appearance of life, were what caused this terror in the reptile.

R. MORTON MIDDLETON, Jun.

West Hartlepool, October 23

An Unusual Rainbow

OCTOBER 28 was a fine day with a brisk westerly wind blowing. At 2 P.M. a splendid well-defined nimbus cloud passed from north-west to north-east, about a mile to the north of this observatory, and rapidly driving away before the wind, left a large tract of cloudless sky behind it, the sun shining at the time. Suddenly at 2.12 P.M. a magnificent rainbow shone out most brilliantly across the blue space, the effect being exceedingly novel and charming. The veil of rain-drops forming the bow was so thin as to be invisible except near the zenith, where there appeared to be a thin cirrus. No rain fell on the observatory, and unfortunately there were no means of determining subsequently the area covered by the shower.

Eventually the rainbow faded away over the cloudless sky, and the 30° or so of the extreme eastern end which overlapped the receding nimbus shone out with a vivid brightness until it disappeared.

A secondary bow was not visible in front of the clear sky, but the violet band of the primary stood out with great distinctness, apparently separated from the remainder of the bow.

Kew Observatory, October 29 G. M. WHIPPLE

OUR ASTRONOMICAL COLUMN

A MISSING STAR.—There was a curious, and at the time suspicious, history attaching to an object, shining as a star of 9·10 magnitude, which was compared on several nights with the minor planet *Hygeia*, while under observation at Washington in the autumn of 1850. This star, which was designated *k* in a list published in Gould's journal, was missed by Mr. Hind, who reported the circumstance in a letter to Mr. W. C. Bond, of Harvard College, by whom the attention of Lieut. Maury, at that time superintendent of the Naval Observatory, Washington, was called to it. Mr. Ferguson having verified the disappearance of this object on August 29, 1851, a search was made for it on the assumption of it being a great planet exterior to Neptune; the reason for this assumption will be apparent from an inspection of the following positions, which result from the observations on three evenings:—

1850.	Washington Mean Time. h. m. s.	Right Ascension. h. m. s.	Declination. ° ′ ″
Oct. 16	6 52 36	19 17 42·81	−20 44 57·1
,, 21	7 6 40	19 17 42·19	−20 44 55·5
,, 22	6 35 35	19 17 43·90	−20 44 54·6

It was also observed on the 19th, but the accurate positions of the stars of comparison are not available. These observations appear to indicate that the object had motion in R.A., but that it was stationary at some time between October 16 and 22, and if we suppose it to have been a planet moving in a circular orbit, we find to allow of its being stationary at this elongation from the sun, its distance would be 49·94, and its period of revolution 351 years, or about twice the period of Neptune, and the period of Neptune is about twice that of Uranus. Notwithstanding the search was continued from August 29 to December 11, 1851, and extended to all stars of the eleventh magnitude between 19h. 20m. and 19h. 36m., and from −19° to −21° 20′, no planetary body was found. That the Washington observers considered suspicion to attach to the object is obvious, but the only likely explanation appears to be that there was a variable star in this position, and that the observations in right ascension were affected with greater error than might be expected, considering that on two of the days of observation several comparisons were made. To our knowledge search was also made in Europe for the Washington star. Further particulars will be found in two letters from Maury, published in Gould's *Astronomical Journal*, No. 36.

THE SATURNIAN SATELLITE MIMAS.—From some Washington observations of this difficult object between the years 1874 and 1877, it appears that the following elements may be taken as approximately representing the motion of the satellite in the interval on the assumption of a circular orbit in the plane of the rings: epoch 1878, January 1·0207 G.M.T., $u = 0° 0'$, $N = 126° 14'·5$, $I = 7° 3'·2$, radius of orbit at the mean distance of Saturn 27″·40, period of revolution 22h. 37m. 5·614s., or the logarithm of the period in days = 9·9742473. The

general run of the differences between calculation and observation, incident probably in part to a sensible excentricity, may be judged from the following results of comparison with a few of the observations made with the great refractor at Washington :—

	Pos. $(c-o)$	Dist. $(c-o)$		Pos. $(c-o)$	Dist. $(c-o)$
1874 Sept. 2	$+0°.6$	$+0''.28$	1877 Aug. 11	$+0°.9$	$+1''.13$
,, ,, 26	-2.6	-0.54	,, Oct. 15	$+0.5$	0.00
,, Oct. 15	$+0.2$	0.00	,, ,, 16	-1.2	...
1876 Oct. 13	$+1.2$	$+0.45$	,, ,, 17	$+4.1$	$+1.86$
,, ,, 31	-1.3	$+1.33$			

From the above elements we shall find for the times of greatest elongation of Mimas eastward, 1878, October 31, at 10·4h., November 1 at 9·0h., November 2 at 7·6h., and November 3 at 6·2h., and at these times, the distance of the satellite from the centre of Saturn about 30″.

GEOGRAPHICAL NOTES

ADVICES from Mr. John Carnegie, H.B.M. Consul at Loanda, of September 9 ult., give most encouraging news with respect to Mr. Heath's expedition to Angola. The young explorer had enjoyed excellent health and had just started on a six months' expedition up the River Bengo, proceeding to Galungo Alto, and, if his health permitted, returning by the Quanza River. The first small collection of birds has been received from Matamba, on the Rio Bengo, an account of which will shortly be laid before the Zoological Society by Mr. Bowdler Sharpe. As the result of a first month's collecting it is creditable, but the season of the year having been adverse, nothing of any striking interest is contained in it. More may be expected from the large case of specimens now on its way to England.

MANY attempts have been made to penetrate into the interior of Greenland from the west coast, but, until this summer, with little success. Three Danish gentlemen, Messrs. Jensen, Kornerup, and Groth, under the direction of the Commission for scientific exploration in the Danish colony, started to explore and survey the coast between Godhaab and Frederikshaab. Lieut. Jensen took advantage of the opportunity to make an excursion into the interior over the ice. The aim was to reach several mountain peaks rising out of the ice. The baggage was placed in three small sledges of the travellers' own, and the toilsome journey commenced on July 14. After two days the loose snow accumulated on the surface of the ice to such an extent that the journey became very dangerous, while they continually sank in concealed crevasses and holes, saving themselves only by adopting the Alpine expedient of attaching themselves to each other with a rope. The surface of the ice was generally undulating, but there were also many rugged parts and chasms, which rendered the journey a very difficult one. It was foggy nearly the whole time, and on July 23 a snowstorm came on. On the 24th the expedition reached the foot of the mountain referred to above. Then came on another storm which lasted for six days with continuous snow and fog; the travellers were snow blind. The weather cleared on the 31st, when the ascent of the mountain might be undertaken with some prospect of success. The height was estimated at about 5,000 feet above sea level, and on the other side of the mountain, as far as the eye could reach, ice sheets and glaciers were seen, and not the smallest speck of land free of ice. After finishing their observations the expedition returned, and reached their starting-point on August 5, having been away for twenty-three days. The mountain referred to was forty-five miles from the coast.

THE discovery of a new island in the Polar Seas is announced. E. Johannessen, who has just returned to Tromsö, reports that he penetrated a considerable distance to the east, beyond Novaya Zemlya. On September 3, in long. 66° E. and 77° 35′ N. lat., he discovered an island which he has named "Ensomheden" (loneliness). It is about ten miles long, and level, the highest point not exceeding 100 feet. It was free from snow, with poor vegetation, but an immense quantity of birds. The sea was free from ice towards the west, north, and south, but drift ice was seen towards the south-east. There was evidence that the Gulf Stream touched the west coast of the island; the Stream runs in a strong current round the north coast towards the south-east. Everything about the ice was favourable for navigation so long as the vessel did not go too near the mainland of Siberia. The newly-discovered island lies, therefore, somewhat to the south-east of the region visited by the Austrian expedition of 1873-4. It has been thought probable that a line of islands in the latitude of this island extends along the north coast of Asia.

NEWS has been received from Prof. Bastian, of Berlin, that he safely arrived at Bushire, on the Persian Gulf, *via* Teheran and Ispahan, and that he has thence continued his journey by sea.

LIEUT. SANDEBERG, whose explorations in the Kola Peninsula and the White Sea we have already referred to, has returned to Sweden with numerous zoological collections obtained during the past summer. Lieut. Sandeberg finds the coast-waters between Varanger Fjord and the White Sea extraordinarily rich in cod and whales.

THE *Deutsche Geographische Blätter* of the Bremen Society, No. 4, contains several items of interest, some of which we note separately. There is a long and valuable paper by Prof. Struder on a visit he made to Timor in 1875, and another on the results of the numerous voyages to Siberia made this summer, all of which have been so eminently successful that a regular summer trade-route to the great Siberian rivers may now be held as established.

THE *Bulletin* of the Geographical Society of Marseilles for July–August contains an interesting account of the little-known Island of Lamoo, on the African coast, a few degrees north of Zanzibar. The island itself is described, and a pretty full account given of its inhabitants and their habits.

A TELEGRAM from Hong-Kong states that the Chinese authorities are contemplating the construction of a railway from Taku to Tientsin, in order to facilitate communication with the capital and to avoid the difficulties to navigation caused by the tortuous course of the Pei-ho. A rumour from the north in regard to this scheme was published in the *North China Herald* of Shanghai, on August 10, "with all due reservation," as it appeared almost too good to be true. Our contemporary says that the plan is believed to have been agreed upon last year, but delayed in execution because it had been hoped that the plant of the condemned Shanghai and Woosung railway could have been made partly available for the purpose. This having been otherwise disposed of, it is said to have been now determined to purchase new plant throughout, and to press forward with the new line as quickly as possible. Mr. Tong Kingsing, a well-known Cantonese merchant, frequently employed by Li Hung-chang, who is said to be the prime mover in this matter, has been at the coal-mines in the north-east of the province of Chihli for some time, but he is expected to return to Tientsin shortly, when it is believed that immediate steps will be taken concerning the new line.

THE Society of Geography of Paris held its first meeting for the year 1878-1879 on Wednesday week, in its new hotel, Boulevard St. Germain, No. 194. The number of members present exceeded 200. M. Quatrefages, president of the Section Centrale, was in the chair, and gave an address, in which he congratulated his fellow members

on the success obtained in the building of the lofty mansion in which they were assembled, which is at a little distance from the place where the Society was founded in 1821.

In his just-published report to the Foreign Office on the trade and agriculture of French Guiana, Consul Wooldridge forwards some information of considerable interest in regard to the production of gold in that region. The quarter of Mana, hitherto unknown as a gold-producing territory, has, through the energetic endeavours of adventurers, been prospected, and is speedily being opened up; indeed, it promises to be one of the richest gold industrial quarters. The production of gold at a few hastily-established placers, in the month of March last year, gave 21,747 grammes of pure gold, and in April 39,662 grammes. It is to be feared, however, that the gold-workings here and in other parts where the precious metal is found to a large extent is having a disastrous effect on the general prosperity of the colony, for Her Majesty's Consul, quoting from the report of the Commission which periodically proceeds to the various quarters to inspect estates, draws a melancholy picture of the abandonment and poverty of agricultural property in French Guiana.

The latest work of the leader of the Austrian North Polar Expedition, Captain Karl Weyprecht, entitled "Die Metamorphosen des Polareises," is now in course of publication at Vienna (Perles).

A curious statement appears in the foreign correspondence of the *Times*, that Russian papers state that the Amu-Darya has returned to its original bed. This may very well be the case without any or much interference on the part of man, as may be seen from Major Herbert Wood's articles on the Aral region in vols. xi. and xii. of Nature. About twenty years ago the Loodon Canal at Bend, above the splitting up of the lower Amu, was dammed up. This canal seems to have been connected with the old course of the Amu into the Caspian, and a strong flood breaking down the dam might easily cause the river to resume its old course, especially as its present mouths seem to be gradually filling up with the abundant matter brought down by its waters. It is apparently at Bend that the deviation has taken place.

An able review of Geography at the Paris Exhibition appears in the last number of the *Revue Scientifique*.

THE TELEPHONE, ITS HISTORY AND ITS RECENT IMPROVEMENTS[1]

I.

The appearance of the two works mentioned below is indicative not only of the remarkable era of scientific invention through which we are passing, but also of the wide-spread interest in science which these inventions have aroused.

It will be noticed that neither of these works is published in England: one reaches us from America, the other from France. As a nation we are slow to appreciate the value of new inventions—a conservatism which arises less from caution than from popular ignorance of science; nor will an enlightened public opinion be possible until the first principles of science form an integral part of the education of every English boy and girl. But now that science is walking in the market-place, and holds its own on the exchange, ignorance of its elements becomes commercially perilous. A sound judgment on the value of a new scientific discovery may at any moment be indispensable to capitalists and very profitable to shareholders. We venture to say that such a conviction has been a prevalent idea on almost every stock exchange during the recent panic in gas shares. Scientific knowledge has presented itself in a new light: it is now a commercial article; and forthwith the British public promptly recognises its value. In fine, the business aspect of recent inventions may do more for the future extension of science teaching than years of earnest expostulation.

The two works before us cover nearly the same ground. They give the history of the invention of the telephone, the methods that have been devised for electrically transmitting and receiving speech, with the most recent improvements down to a month or two ago; they also describe the phonograph; and Prescott's book, while omitting the microphone, which is fully discussed by Du Moncel, devotes its concluding chapters to quadruplex telegraphy, electric call-bells, and electric lighting.

Of the two works Count du Moncel's is the more scientific, comprehensive, and impartial, and will add to the high reputation which its indefatigable author already possesses as the historian *par excellence* of the applications of electricity. We can therefore most heartily commend this treatise to our readers; it is, moreover, well printed, capitally illustrated, and withal published at a very low price.

Mr. Prescott's work is larger, the typography and illustrations are excellent, and in technical details and recent information it leaves nothing to be desired. The arrangement, however, is confusing. The body of the work consists almost wholly of reprints from the various papers, lectures and specifications of the workers at electric-telephony, and the absence of marks of quotation with the want of proper indication where one extract ends and another begins not only puzzles the reader but is apt to give rise to serious misapprehension. The work has obviously been hastily prepared for the press, repetitions are frequent, and the matter is arranged with little regard to the reader's convenience or to chronological sequence. Moreover, its author has an evident bias towards American inventions in general and the "Western Union Telegraph Company" in particular. It is true the work professes to deal with speaking telephones only, but as some American "tone telephones" are described in detail, we are surprised at the entire omission of the early and important telephonic experiments by Cromwell Varley in London, and afterwards by La Cour in Copenhagen.

Nevertheless, with all its defects, Mr. Prescott's book is a useful and needed contribution to scientific literature, and as each inventor is allowed to speak for himself, the careful reader is enabled to form his own judgment on certain disputed questions of priority of invention.

It is time that the history of the articulating telephone was written. Hitherto the English public have had little more to guide them on this subject than the lectures given in London by Prof. Graham Bell, lectures delivered with altogether admirable grace and diction. It is very natural that an inventor should give more prominence to his own ideas than to those of others, and hence the impression generally derived from Prof. Bell's lectures is that the sole credit of the first conception and successful construction of the articulating electric telephone is due to himself. There were, however, other workers in the field of electric-telephony besides Mr. Graham Bell, and it is to be regretted that Prof. Bell did not give sufficient prominence to this fact in his discourses. Mr. Prescott, indeed, brings some serious charges against Prof. Bell, asserting that to another American, Elisha Gray, of Chicago, is due the entire priority and merit Bell claimed for himself. Here is what Mr. Prescott says:—

"It was not till after Prof. Bell had substituted the apparatus shown in Mr. Gray's *caveat* that he was enabled

[1] "The Speaking Telephone, Talking Phonograph, and other Novelties," by G. B. Prescott. Illustrated. (New York: Appletons, 1878.)—"Le Téléphone, le Microphone, et le Phonographe," par Le Comte Th. du Moncel. (Hachette, 1878.)

to successfully accomplish the grand object of reproducing articulate speech at a distance" (p. 73).

A little further on Mr. Prescott remarks :—

"From the reading of the text [Prof. Bell's lecture in London] it might be erroneously inferred that the apparatus shown [a water variable-resistance telephone] was invented by Prof. Bell, and exhibited by him at the Centennial Exhibition. Prof. Bell neither invented nor exhibited it. The figure [given by Bell] represents the transmitting portion of Elisha Gray's original speaking telephone, the first articulating telephone ever invented. Mr. Gray experimented with the telephone at the Centennial Exhibition in America in 1876, and showed it, among others, to Prof. Barker, but did not exhibit it to the judges."

Even with reference to the present shape of Bell's telephone, Mr. Prescott denies that Bell was its inventor and he adduces evidence to show that the present portable form of the handle telephone was due to Dr. Channing and Mr. Jones, of Providence, R.I. This, however, is a minor matter, but the question of priority of invention of the principle of the articulating telephone is one of general interest and importance.

To the consideration of this matter the Count du Moncel brings not only an independent and unbiassed mind, but also a profound technical and historical knowledge of the various applications of electricity. And in the following opinion, which he expresses, we entirely agree :—

"Si M. Bell a été le premier à construire et à rendre pratique le téléphone parlant, M. Elisha Gray avait le premier conçu le principe de cet instrument et l'avait combiné en electricien consommé."

A similar opinion is expressed in a very able and lucid discourse by a well-known electrician, Mr. F. L. Pope, delivered last December before the American Electrical Society at Chicago, reprinted in Prescott's book. At the same time we must bear in mind Prof. Graham Bell had for some time back also been at work at a similar problem to that which had led Gray to the conception of an articulating telephone, namely, the problem of multiple telegraphic transmission by means of harmonic vibrations, and from this subject was led to the discovery of his magneto-electric telephone. As Mr. Pope remarks, "when we consider that each was working in ignorance of the labours of the other, the singular coincidence in the results they finally obtained is not a little remarkable." To Gray and Bell a third name has also to be added, namely, that of Edison, to whose work we shall refer more fully in another article. These three names stand conspicuously forth in connection with the discovery of the speaking telephone, and we therefore propose to trace the relationsbip each bears to this subject.

The dominant idea that stimulated each of these inventors was the possibility of transmitting several messages simultaneously along one wire. By his patent of 1870 Varley had led the way to the method by which this could be accomplished; he succeeded, in fact, in transmitting secondary currents, generated by the vibration of a tuning fork, in the primary circuit of an induction-coil, concurrently with the ordinary Morse signals, the former not sensibly affecting the usual electro-magnetic receiving apparatus, but producing audible signals on a peculiar receiver of his own. After this, in September, 1874, La Cour, of Copenhagen, patented an apparatus for multiple transmission, founded on a modification of Varley's plan. In this case the receiver was a tuning-fork, controlled by an electro-magnet, and tuned in unison with the transmitting fork, hence it was capable of being thrown into sympathetic vibration by the electric waves started by the latter. A series of such duplicate forks was employed corresponding to the notes of the musical scale, and it was found that the intermittent currents of several of these forks could be simultaneously transmitted without confusion, each receiver selecting and vibrating under its appropriate system of electro-magnetic impulses. Early in 1875 Gray, of Chicago, patented a somewhat similar, but more perfect arrangement. Gray's caveat, or application for his patent, dates from August, 1874, so that in point of fact he anticipated La Cour's method. Instead of using tuning-forks Gray employed strips of steel as being lighter and more sensitive; each transmitting reed instrument had, of course, its fellow at the receiving end, which promptly responded to its own system of waves, acting upon it through an adjacent electro-magnet.

The idea of synchronising the movements of two instruments at wide intervals apart by employing the principles of isochronous vibration is not novel, it was carried out by Helmholtz in his experiments on vowel sounds; and still earlier distant isochronous pendulums were used in telegraphy to control machinery, by Vail, in 1837, Ronalds in 1861, and Hughes in his printing-telegraph. But Gray accomplished more than this. Reiss, in 1862, had shown by his telephone how the *rate* of vibration might be electrically transmitted and reproduced, but the amplitude and mode of vibration were lost; Gray, towards the close of 1874, discovered a method whereby the proper *amplitude* of each vibration or combination of vibrations could be reproduced, "by causing the effective strength of the electric current, by which the transmission is

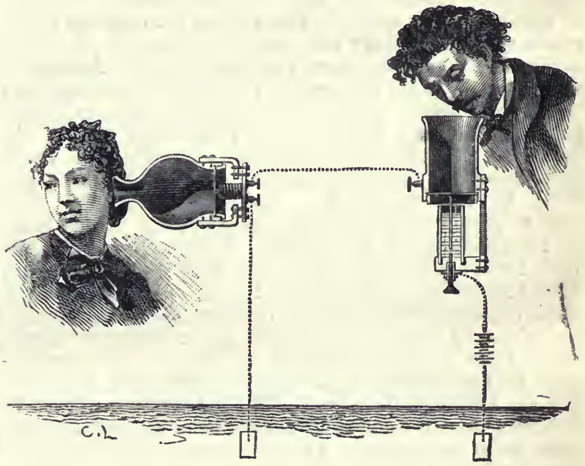


FIG. 1.—Original arrangement for the electric transmission of speech designed by Gray. A current from the battery on the right passes into a vessel containing water, into which dips a wire attached to the vibrating membrane and in circuit with the line. (From Du Moncel's work.)

effected, to rise and fall with the varying amplitude of the sonorous waves which are to be reproduced." Hence, as Mr. Pope, in the discourse to which we have alluded, goes on to remark :—"This having been accomplished, it was not difficult to foresee that two practical applications might be expected to follow, namely, multiple transmission and vocal transmission."

There yet remained however the difficulty of impressing upon an electric current the rapidly-changing *forms* of the sonorous waves which occur during the act of speaking. In the beginning of 1876 Gray conceived the idea of accomplishing this by attaching to a stretched membrane, such as was used by Reiss, an arrangement whereby the movements of the membrane should produce proportional alterations in the resistance of an otherwise constant electric circuit. Undulatory currents of fluctuating strength would thus be set up by the voice, and these, acting electro-magnetically upon a diaphragm at the far end—to which was attached a piece of soft iron—would cause it to be thrown into vibrations corresponding to those existing at the transmitting end. The problem of the transmission of speech was thu

theoretically solved. On February 14, 1876, Gray registered this invention at the American Patent Office, under the title of "a means of transmitting and receiving vocal sounds telegraphically," and in his caveat he gives an exact drawing of the method he adopts, and which we here reproduce.

Curiously enough, on the very same day, there appears the first documentary evidence on behalf of Prof. Graham Bell, and this, too, is for a patent granted to Bell—not, however, for the electric transmission of speech, but "for certain new and useful improvements in telegraphy." These improvements consist in the employment of *induced* undulatory electric currents, and form one of the numerous practical applications of Faraday's famous discovery of magneto-electric induction. By the approach and recession of the prongs of a magnetised tuning-fork, or by the oscillation of a magnetic diaphragm, alternating currents were generated in an adjacent coil of wire. This is the essence of Bell's patent, the advantages claimed by the use of such undulatory currents being increased speed of telegraphy and the possibility of multiplex telegraphy. Nothing is said about the transmission of speech till near the end of the specification, when it is stated that "one of the ways in which the armature may be set in motion [to generate these currents] is the wind. Another mode is the human voice, or by means of a musical instrument." So that, of the five claims made by this patent, the last, and apparently quite subsidiary one, was "the method of transmitting vocal or other sounds telegraphically." A diagram of the arrangement devised for this purpose accompanies the specification, which arrangement, however, upon subsequent trial, proved, as Prof. Bell stated in London, "unsatisfactory and discouraging." It is not, however, fair to conclude, as Mr. Prescott has done in the words we quoted earlier, that Bell had to resort to Gray's method before he was enabled to transmit speech electrically. The fact seems to be that some little time after he obtained his patent, Bell turned his attention to the development of the speaking telephone, and by a modification of the method he originally proposed, arrived at some important results which were published on May 10, 1876, in the *Proceedings* of the American Academy of Arts and Sciences. Sir W. Thomson heard articulate sounds transmitted by this telephone in August, 1876, but the instrument was then very imperfect, nor was it until the early part of 1877 that the speaking telephone may be said to have been a *fait accompli*; in May, 1877, it was successfully tried between Providence and Boston, places forty-three miles apart. There seems reason to believe that the important improvement of the substitution of permanent magnets for electro-magnets was made at the suggestion of Prof. Dolbear, and that Professors Peirce, Blake, Channing, and others contributed valuable modifications of the original design, until the Bell telephone assumed its present simple, elegant, and handy shape, growing in efficiency as it diminished in size and complexity.

Thus it will be seen both Gray and Bell can fairly claim the discovery of the principle of the articulating electric telephone. Gray solved the problem first theoretically, Bell first practically; the former proposed to vary the resistance of the circuit without changing the electromotive force; the latter varied the electromotive force without changing the resistance. And although Gray's method was only partially successful in operation, owing to his employing an electrolytic resistance, it is a method capable of yielding more striking results, owing to the use of more powerful currents. But where Gray failed, Edison has succeeded, and in another article we propose to trace the connection of this remarkable inventor with the subject of electric-telephony, up to his splendid discovery of the carbon telephone.

W. F. BARRETT

COLOUR BLINDNESS IN RELATION TO THE HOMERIC EXPRESSIONS FOR COLOUR[1]

II.

SO far as I can follow Mr. Gladstone's investigations, it appears to me that Homer has exactly fulfilled all the conditions mentioned in the previous article. As many references are made to natural objects which have the same colours now as they had in his time, I am able, with my colour-blind experience, to judge what sensations they would present to his eyes, supposing him colour-blind, and I can thus form a judgment of the appropriateness and consistency of his descriptions on that hypothesis. I can clearly trace the existence of two groups of epithets, which, so far as I can see, are kept fairly distinct, and the words in which are never mixed up with the ideas belonging to the contrary group. The epithets are—

For the group of the yellow sensation: ξανθός, ἐρυθρός, φοίνιξ, ῥοδόεις, χλωρός, κυάνεος, and perhaps οἶνοψ.

For the group of the blue sensation: πορφύρεος and ἰοειδής.

For neutral sensations, irrespective of the words λευκός and μέλας (which may be left out of consideration altogether, the use of them being normal, and the vision of the colour-blind in regard to them being normal also) there is the epithet πολιός, on which an important element of the argument hangs.

We will now take these various words *seriatim*, and compare what Mr. Gladstone says of their application with the use that might be expected to be made of them by a colour-blind writer.

Ξανθός.

Liddell and Scott's translation of this word is "yellow of various shades, often with a tinge of red, chestnut, auburn." Mr. Gladstone (N. 380) considers it, as used by Homer, to be a true word of colour, and that its applications are especially consistent.

It is used principally for human hair, and to the colour-blind *all* varieties of hair, except such as is positively jet-black, appear shades of yellow. Fair or golden hair is a light yellow, red and auburn hair are deeper tones, more intensely coloured, and all varieties of brown are darker still.

The word is also used for the colour of horses. All the varieties of chestnut and bay are to the colour-blind dark yellow, a yellow brown, the former of a lighter, the latter of a darker shade.

Ἐρυθρός.

This is, I suppose, the most usual Greek word for red. Mr. Gladstone (N. 375) takes it to be the best approach to a true genuine colour-epithet, but at the same time he remarks how strange it is that Homer's idea even of red does not seem to be wholly distinct.

The difficulty, however, vanishes if we suppose Homer to have been in the position of the colour-blind, to whom, as I have explained, the proper idea of red is unknown. The word, according to Mr. Gladstone (N. 375, H. 460), is applied to copper, nectar, wine, and blood, all which, though they may differ in appearance to the normal-eyed, present to the colour-blind only different modifications of the yellow sensation.

In regard to blood, the hue varies according to its condition, arterial blood differing materially from venous blood in its colour. I believe that normal-eyed people hesitate to recognise any yellow element in it in any condition, but it is quite certain that when bright and freshly-oxygenated, it presents a sensation of yellow to me; and this is consistent with the fact that its colour is said to be chiefly due to the oxygenation of the iron it contains, the peroxide of iron being to me very positively yellow.

I conceive it may be possible that in this, as in many

[1] Continued from p. 679.

other cases of red, the yellow element may really be there, but may be so overpowered, to the normal eye, by the more vivid red sensation as to be undistinguishable to them, whereas to me, who am free from such interference, it is distinctly visible.

When blood is in the venous state it alters its colour, losing the yellow and acquiring the blue; and I believe normal-eyed people admit the existence of the blue element in blood of this kind. This fact will be found of importance in a subsequent place.

Mr. Gladstone adds:—"The favourite use of the word is for wine; this is very remarkable, because wine is not of a redness proper, but only approximative, and with a decided infusion of the idea of darkness." He also notices its application to the dark hue of red sandstone rock, and sums up by saying that the word is, in the great majority of instances, associated with dark rather than with bright. This is quite in accordance with the darkening idea of red that pervades the colour-blind theory.

Φοίνιξ.

This word is translated by Liddell and Scott, "a purple red, purple, or crimson." It is used very frequently by Homer, and Mr. Gladstone (N. 372, H. 463) finds many difficulties from its being applied to colours materially different from each other. If, however, we test his examples by the dichromic perceptions, we shall find all the difficulties disappear.

The first application is to blood; and in this case the word would appear to be synonymous with *eruthros*, and is justifiable on either the normal or the colour-blind principle.

But it is also applied to the colour of a horse, who was *phoinix* all over except a white spot on his forehead. Mr. Gladstone says that the same epithet sits very ill upon blood and the colour of a horse, whether bay or chestnut; and no doubt this is true as far as normal-eyed people are concerned, inasmuch as the equine hues contain, I am told, a much larger amount of yellow, being, in accurate colour terms, different varieties of orange-brown. But to the colour-blind these present only their yellow element, and since it is by that same element that arterial blood is recognised, there is no incongruity in the person describing both by the same term. It is curious that, whereas in the case of blood *phoinix* is used as a synonym for *eruthros*, in the case of horses it is used as a synonym for *xanthos*—a strong presumption in favour of the *grouping* I have insisted on—the combination being justified through the common element of yellow.

Phoinix is also used for the back of a dragon or serpent, for jackals, and for the skin of a lion. The lion is, even to normal-eyed people, exactly my colour, yellow brown, and the jackal, though grey or variegated on the back, has much of the same hue about him. I never saw a dragon; but snakes vary much in colour, and at least half the varieties at the Zoological Gardens convey to me a positive impression of yellow.

A compound of the word is also applied indirectly, by a comparison with the serpent (H. 476) to the rainbow. For the explanation of this, see the word *porphureos* farther on. It is also applied to cloaks or mantles, which Mr. Gladstone concludes were not red, as Homer never applies to them the more positive epithet for that colour. As we do not know what hue they were, we cannot reason on this application: it is sufficient for my purpose to assume they may have been some of the many varieties of colour which would give the yellow sensation to the colour blind.

It is applied to the bows of ships, which are known to have been painted with some kind of red colour.

If the word *phoinix*, used in *Od.* vi. 163 to mean the palm tree, has any connection with the colour epithet, as Mr. Gladstone appears to suggest, it furnishes a startling addition to the proof of the colour-blind theory. The confusion of red and green is incomprehensible to the normal-eyed, but it is one of the best-marked symptoms of the dichromic malady. *Phoinix* to me would just as correctly represent the leaves of a palm as it would arterial blood, a chestnut horse, or the skin of a lion.

It is clear, therefore, that we have only to suppose *phoinix* to be one of a group of words, all representing the colour-blind sensation of yellow in some of its varied shades and tones, and the whole of these apparently strange and anomalous applications become natural and justifiable. Indeed, Mr. Gladstone (H. 455) notices the anology with *xanthos*, and remarks that *phoinix* appears merely to "render other words."

Ῥοδόεις.

Referring to the rose; rosy. In noticing the use of this word, Mr. Gladstone (N. 376) at once seizes on the remarkable fact that, although the redness of the rose is so obvious, yet "there is no direct point of contact between Homer's expressions taken from the rose, and *eruthros*, as they are never applied to the same objects."

But this is perfectly in accordance with the sensations of the colour-blind. It was pointed out long ago by Dalton, and I took some trouble to explain the fact scientifically in my paper, that "Crimson and pink (rose colour) appear to have no relation to the idea of red derived from vermilion or a soldier's coat;" and if the colour-blind person has been in the habit of using *eruthros* for the latter it would do violence to his sense of colour to use it also for the rose. This flower, beautiful and positive as its colour is to the world in general, gives to me a very vague impression. Its characteristic of redness being invisible to us, we see in most cases only a pale grey; if the colour inclines to scarlet this will be tinged with yellow; if very crimson it will be tinged with blue.

Mr. Gladstone (N. 376, H. 469) is naturally puzzled by the application of the epithet rosy to olive oil, but the above explanation disposes of the difficulty. I have certainly heard my friends describe as "rosy" objects which to my eye would fairly match the pale yellow of oil.

Κυάνεος.

This is a word the explanation of which appears to be involved in much difficulty. It is said to mean the colour of a substance called *kuanos*, but what this substance was, or even what its colour was, appears open to much doubt.

The usual translation of the adjective, according to Liddell and Scott, is "dark blue" (whence the chemical term *cyanogen*), and there is no doubt that, in later Greek, it acquired significations positively identified with this colour. Mr. Gladstone, in 1858 (H. 496), discussed the meaning of *kuanos* at much length, and thought it most probably referred to a native blue carbonate of copper, an interpretation in accordance with its subsequent use and description as a colour. If, therefore, this meaning were adopted, the word *kuaneos*, conveying a distinct idea of blue, could not be included in the same group with *xanthos*, *eruthros*, and *phoinix*, which all, as we have seen, belong to the opposite sensation.

But Mr. Gladstone, after reconsideration, appears, in his later article (N. 378, &c.), to have altered his view. He now considers it "almost certain that *kuanos* is bronze," and *kuaneos*, either "made of, or in hue like bronze." This implies the abandonment of the idea of blue as connected with the adjective; for, so far as I know, there is not a vestige of the blue element in the colour of any combination of copper and tin. In any case, however, there is no doubt that a very dark hue is referred to.

Now the impression conveyed by bronze to the colour-blind eye is very dark, almost black, but with a tinge of dark yellow-brown; and keeping this in mind, if we

review the various applications of the word given by Mr. Gladstone (N. 378, H. 462), we shall find nothing inconsistent with this explanation. It is applied—

To eyebrows, to hair, and to the coat of a horse, in any of which cases a very dark brown may be shown.

To a dark cloud, which may be of the same hue.

To the serried mass of the Greek and Trojan armies. "The colour of these," says Mr. Gladstone, "must have been derived from their arms, and these would probably be composed in the main of two elements, firstly copper, which is ruddy, and secondly, the hides of oxen upon the shields and elsewhere." He notes that to a normal eye the colours of these are not easy to combine in a common idea:—but to the colour-blind the combination is homogeneous enough; they both look dark yellow-brown, and the appearance is quite in accordance with the interpretation of the word *kuaneos* according to Mr. Gladstone's later view. When in 1858 he appeared generally to favour the blue interpretation he remarked justly that it could not hold in this instance.

To a very black mourning garment. But the blackest dyes have almost always some leaning either to brown or blue, and the use of another word instead of *melas* might possibly imply this leaning, without diminishing the intensity of the shade.

To the sea-sand, just left bare by the water, also yellow-brown. Here again the idea of blue seems inapplicable.

To Amphitrite, or the sea beating on rocks. The *deep* sea is dark blue or dark green; but its appearance close to the shore in shallow places may be so indefinite, that no positive inference can be drawn from this use of the term.

To the prow of a ship; this, we know by other passages, was painted with red earth or ochre, and if dark would appear the colour here implied.

We have here exhausted Mr. Gladstone's list of instances where this difficult word is used clearly as a colour-epithet. They are all perfectly consistent on the colour-blind hypothesis, clearly pointing to the classification of *kuaneos* in the yellow group; for, so far as I can judge, there is not a single instance where its application *necessarily* implies the idea of blue.

Mr. Gladstone remarks (H. 465):—

"The uses of this group of words (*i.e.*, the group formed from *kuanos*) thus appear to exhibit a degree of indefiniteness hardly reconcileable with the supposition that Homer possessed accurate ideas of colour; there is no one colour that can cover them all." This is true; but only suppose him dichromically colour-blind, and the dark yellow-brown hue he may call *kuaneos* will cover every example where he has used the term.

Χλωρός.

I suppose no doubt is entertained that this word, derived from *chloē* (young herbage), means, and always has meant, green, one of the most plentiful colours in nature, and one of the most positive and distinct to persons with ordinary eyes.

Now Homer's use of the word affords one of the strongest arguments as to the identity of his sensations with those of the colour-blind. Let us see the testimony which Mr. Gladstone offers to this fact. After quoting (H. 467) the application of the word to a pale face, to fresh-pulled twigs, to honey, to an olive-wood club, and to the nightingale, he remarks:—

"Upon the whole, then, *chlōros* indicates rather the absence than the presence of definite colour. If regarded as an epithet of colour it involves at once a hopeless contradiction between the colour of honey on the one side and greenness on the other. Again, the more we assume it to mean green the more startling it becomes that it could have taken paleness, as is manifestly the case, for its governing idea. . . . . The idea of green we scarcely find, unless once, connected with this word in the poems of Homer; and yet it is a remarkable fact that there is no other word in the poems that can even be supposed to represent a colour, which not the rainbow only, but every-day nature, presents so largely to the eye."

Again, in the later article Mr. Gladstone says (N. 380; the italics are mine):—

"It is plain, from the applications of it, that green was not on the list of Homer's colours. If I am to choose an English equivalent for the phrase it will be pale; and pale is not properly an epithet of colour so much as of light, although *there may perhaps be detected in it a very faint inkling*, so to speak, *of yellow*. If we strive to give the sense of colour we find there is none that will cover them in common, *yellow suiting in some cases*, green in others, neither of the two in all."

Speaking further of the application of *chloreïs* to the nightingale, he adds:—

"The balance of authority attaches the phrase to the hue or aspect of the bird, and, when so attached, it loses all definite idea of colour. . . . . Evidently enough, Homer's idea in this matter could not but be most vague and dim."

I have quoted these passages in order to show what a remarkably apposite commentary they offer on my own words, written twenty years ago.

"Green is a colour most perplexing to the patient, who cannot be said to manifest any definite sensation about it at all." It would scarcely be possible to give a more appropriate description than Mr. Gladstone has given of the impressions of the colour-blind in regard to green, although in all probability he knew little or nothing of these when he wrote the passages in question.

I have already explained how this arises, theoretically, from the invisibility of green proper to the colour-blind, and the appearance of green objects to them under false colours. As a matter of practice I have felt, throughout my life, that this colour has been my greatest stumbling-block, in regard to which my ideas and expressions have gone most astray.

In order to guess how a colour-blind person would be likely to use the term, we must bear in mind the fact, already stated, that the majority of greens in nature appear to him as varieties of yellow; chlorine gas, for example, which takes its name from the Greek word, is a decided yellow to me. And further, it is a fact within my own experience that, unless very powerfully coloured, such yellow greens have mostly a pale, washed-out appearance; indeed, if I find that a new object presents to my eye a sickly pale tint of yellow, I often make a successful chance shot in calling it green.

Keeping these explanations in mind, Homer's applications of the word appear quite natural.

The idea of paleness I have, I think, sufficiently explained. A pale face appears to me just such a sickly yellow as I have described.

I do not exactly know what the "fresh twigs" pulled by Eumæus to make a bed for Ulysses would be like, but they would probably be either green or brown, both which present to the colour-blind shades of yellow.

Honey, a pale yellow, is a perfect match to my eye with varieties of yellow green.

The club of the cyclops would be the colour of the bark of the olive tree, which is, I believe, a brownish grey, and would still be in the dark yellow category to the colour-blind.

The application of the term to the nightingale will naturally puzzle the normal-eyed, as the bird has nothing green about him. But he is described (N. 381) as a compound of tawny, olive, brown, and ash colour; and all these, except the last, which I do not quite understand, convey to the colour-blind the impression of modified yellow, by which chiefly they know green.

Οἶνοψ.

Wine-coloured. Homer (N. 377, H. 472), in speaking of wine, uses (omitting *aithops*, which, Mr. Gladstone thinks, may refer more to sparkling than to colour) two epithets: *eruthros*, red; and *melas*, black. This is con-

sistent enough with ordinary usage, as the red wine in the south of Europe often is very dark, and is called *vino nero*.

To the colour-blind, if red wine is moderately coloured it appears a dark yellow-brown, but when very dark the yellow element may disappear, being overpowered by the blue in the purple, when the impression is simply black, as to the normal-eyed. I frequently see strong red wines in which I can distinguish no colour at all.

Homer uses the word *oinops* for oxen, which, if a dark ruddy brown, would present to the colour-blind the same hue as red wine.

He also uses it for the sea, under special associations which seem to indicate darkness, as, for example, "under a rattling breeze at night." In such a case the sea would show no colour, and the term might be merely a poetical simile drawn from the *vino nero*.

We now come to the opposite group of colour-epithets, applied to objects which give to the colour-blind a sensation of blue; this group, in accordance with the comparative rarity of the impression it denotes, comprehends a less variety of words, being limited to two.

The most important is

Πορφύρεος,

which, I suppose, may be translated purple.

This word, with its compounds, has, Mr. Gladstone says, the largest and most varied application in Homer; he considers its use peculiarly embarrassing, and dwells (N. 373-4, H. 461) at considerable length on the anomalies it presents.

He states that Homer's uses of the word imply three very different forms of colour, namely, red, purple, and grey, and no doubt, to the normal-eyed, these are incongruous enough; but, when we consider the terms under the colour-blind aspect, the incongruities disappear.

The second colour appreciable to the dichromic vision is *blue*, and a great number of different hues in nature, which happen to contain blue in their composition, appear to the colour-blind as varieties or shades of this colour. For example, many crimson hues of red, verging towards violet, contain blue, and, being darkened by the red, show dark shades of this colour. Purple or violet is a still bluer compound. All blue-greens appear blue, and, in regard to dark greys, they often have blue in them, or at least give a blue impression.

Now assuming the poet to have dichromic vision, I suppose *porphureos* would be the most likely word in Mr. Gladstone's list to represent his idea of the various shades of blue; and it is easy to recognise its applications in this way by the examples given (N. 373, H. 461). Omitting the metaphorical uses of the word, we find it applied:—

To various articles of clothing and furniture, which might be of many colours, all conveying the sensation of blue.

To the rainbow. I have, in my paper, fully explained the appearance of the solar spectrum; it presents two colours only, the less refrangible part appearing yellow, the more refrangible part appearing blue. Hence a colour-blind person in speaking of the rainbow may correctly use either term. Homer appears to use both, for in another place he compares the rainbow to a dragon or a serpent, for which he uses the words *daphoinos* or *kuaneos*, both, as we have seen, belonging to the yellow category.

To blood. Under the word *eruthros* it has been pointed out that blood, when venous, loses what yellow element it possessed and by tending towards purple shows a blue impression to the colour-blind. This will account for the mention of blood in this class.

To a dark cloud. The prevailing hue of dark clouds is, both to the normal-eyed and the colour-blind, grey. But this grey may, by atmospheric causes, become tinged either with brown or with blue; the former case has been noticed under the word *kuaneos*, the latter comes in here.

To waves and to the darkening sea. The beautiful blue colour often seen in the Mediterranean is notorious, and to me, at least, it has been particularly marked in the darker aspects of the water.

To death. So far as this application of the colour epithet may be literal, it may be explained by the fact that the livid hue of a corpse has to the colour-blind a decidedly blue tinge.

Ἰοειδής.

Violet-coloured. This epithet clearly belongs to the blue group, for the colour violet is a compound of blue with red, and to the colour-blind eye the blue element alone is visible, the red addition having merely the effect of giving a dark shade. Hence the word may be used by them consistently enough for all impressions of darkened blue.

It is applied by Homer:—

To the sea, for which, on the colour-blind view, it is equally appropriate with *porphureos*.

To iron, which is both to the normal-eyed and to the colour-blind a bluish grey.

To wool, which Mr. Gladstone (N. 380) thinks may have been dyed to a deep purple.

To living sheep (H. 471). This application is not so intelligible, as, so far as I recollect the appearance of black sheep, their colour has inclined rather to brown than to blue. The word, however, used here is a compound one, *iodnephes*, meaning, according to Liddell and Scott, "violet-dark," and it may possibly refer to that variety of dark violet I have before mentioned, in which the blue tinge is indistinguishable. There would seem to be a certain analogy here with the use of *oinops* for the black sea.

Mr. Gladstone includes among his adjectives one which I call neutral, *i.e.*, used for objects which do not convey to the colour-blind either of their two colour sensations. This is

Πολιός,

usually translated grey or hoary. Mr. Gladstone says (N. 381, H. 466) it is applied to the human hair in old age, to iron, and to the hide of a wolf, in all which cases grey is a fair interpretation.

But it is also a stock adjective for the sea, being used for it in no less than twenty-four places. Now the standard colour of the sea (the blue being exceptional) is, I am told, green, and I know by my own observation that the particular hue of green is just that which is neutral to the colour-blind, thus appearing grey. It is possible that the word, in the sense of "hoary," may refer to the sea foam; but if it is really intended to mean grey, its repeated use for the green sea is an additional proof of the correspondence of the sensations of the writer with those of the colour-blind.

I pass over the words *aithos*, *aithops*, &c., as Mr. Gladstone, while finding great difficulty in their interpretation, hardly considers them epithets of colour.

It may be useful to add a summary, appreciable at a glance, of the various objects to which colour epithets have been applied by Homer, classifying them as above described.

GROUP I.—*Objects conveying to the Colour-blind the Sensation of Yellow or Yellow Darkened.*

Ξανθός—	Human hair.
	Coats of horses.
Ἐρυθρός—	Copper.
	Wine, Nectar.
	Blood (arterial).

Φοίνιξ—	Blood (arterial).
	Coat of a horse.
	The dragon or serpent.
	The rainbow.
	The jackal.
	The lion.
	Cloaks or mantles.
	Red prows of ships.
	The palm tree.
Ῥοδόεις—	The rose.
	Olive oil.
Κυάνεος—	Bronze.
	Dark eyebrows and hair.
	A dark cloud.
	The dark coat of a horse.
	Masses of armed men.
	Black mourning garments.
	Sea sand.
	The sea beating on rocks.
	Red prows of ships.
	The dragon or serpent.
Χλωρός—	A pale face.
	Fresh pulled twigs.
	Honey.
	Olive wood bark.
	The nightingale.
Οἶνοψ—	Red wine.
	Oxen.
	[The sea in circumstances of darkness.]

GROUP II.—*Objects conveying to the Colour-blind the Sensation of Blue, or Blue Darkened.*

Πορφύρεος—	Various articles of clothing and furniture.
	The rainbow.
	Blood (venous).
	A dark cloud.
	Waves and the darkening sea.
	Death.
Ἰοειδής—	The violet.
	The sea.
	Iron.
	Dark dyed wool.
	[Dark living sheep].

EXTRA GROUP.—*Objects conveying to the Colour-blind a Neutral Sensation.*

Πολιός—	Human hair in old age.
	Iron.
	The hide of a wolf.
	The sea.

I think the following propositions may be now taken as made out on the evidence supplied by Mr. Gladstone:—

1. That Homer's applications of colour epithets are in many cases inconsistent with the normal ideas in regard to them. This is the first and most general symptom of colour-blindness.

2. That this inconsistency is particularly noticeable in the use of the expressions for red and green. This is a further and more definite symptom, showing the peculiarly defective sensations in regard to these particular colours.

3. But that when the objects referred to are classified in two groups, according to the two colour sensations they respectively offer to the colour-blind eye, the use of the colour-epithets becomes consistent, no epithet belonging to one group being used (except in one doubtful case) for an object belonging to the other. This is a still more definite symptom, pointing, as it seems to me, to the *dichromic* nature of the malady.

It is not my province to carry the matter further; but if the explanation offered be correct, it may involve some very interesting considerations.

One may ask whether the defect in vision which gave rise to these singular uses of the colour epithets was likely to have been general among the people of the time? Do the expressions convey what would have been the general sense of the Greeks of the Homeric age? If so, we may fully concur in Mr. Gladstone's hypothesis, that the organ of colour was but partially developed among them, while at the same time we learn exactly what was the nature of their deficiency. It would be a most interesting fact in physiology and optics if we could show, in this way, that dichromatism was an early stage of human vision, out of which the present more comprehensive and perfect faculty has been gradually developed in the course of some thousands of years.

But on the other hand, it is quite possible that this defect was not general, that it existed only in the person or the writer whose language exhibits it. If this view is correct it may have a most important bearing on a dispute that has long agitated the scholarly world, namely, as to the authorship of the Homeric Poems.

If we can trace, running through the whole of these immortal works, the distinct and consistent evidence of a well-marked personal peculiarity in the writer—a positive characteristic by which his individual identity may be, in all parts, clearly inferred—we have the strongest possible proof, by internal evidence, of the existence of a single author, to whom the whole of the poems are due.

WILLIAM POLE

NOTES

AMONG the well-deserved decorations awarded in connection with the Paris Exhibition is that of Grand Officer of the Legion of Honour to the eminent chemist M. Pasteur.

AT the annual meeting of the Mathematical Society, November 14, Lord Rayleigh, F.R.S., instead of giving an address, will read a paper on the Instability of Jets.

IN connection with the operations of the United States Fish Commission during the past summer, *Harper's Weekly* furnishes some particulars of what may be considered as one of the most important discoveries of recent date in regard to the geology of North America. During the operations of the Commission a formation was met with which belongs probably to the miocene or later tertiary, as shown by the occurrence of numerous fragments of eroded, hard, compact, calcareous sandstone and sandy limestone. These are usually perforated by the burrows of *Saxicava rugosa*, and contain in more or less abundance fossil shells and fragments of lignite, radiates, &c. These fragments have generally been hauled up by trawl lines from depths of from 50 to 250 fathoms, and have already furnished a large number of species, some of them northern forms still living on the New England coast, others for the most part extinct. A conspicuous fossil of an undescribed species belongs to the genus *Isocardia*. Other genera are *Mya*, *Ensatella*, *Cyprina*, *Natica*, *Cardium*, *Cyclocardia*, *Fusus*, *Latirus*, *Turritella*, &c. The specimens so far obtained range from George's Bank to Banquereau, a region of at least several hundred miles in length, and extending along the outer banks from off Newfoundland nearly to Cape Cod. Indeed, it is suggested by Prof. Verrill that the formation constitutes in large part the plateaus known as fishing banks, frequented by such large numbers of cod, halibut, &c. The credit of bringing these specimens to light is due chiefly to Mr. Warren Upham, who originally visited Gloucester for the purpose of investigating certain glacial drift and fossiliferous deposits, and who obtained many of the specimens from fishermen who had brought them in and kept them as curiosities.

IN the summer of 1877 an expedition in the interest of the Princeton (U.S.) College Museum of Geology and Archæology was fitted out for the purpose of making explorations in

the tertiary beds around Fort Bridger and in Central Colorado, especially with a view of securing specimens of some of the interesting fossil vertebrates of which Prof. Cope and Prof. Marsh have described so many species. Six persons connected with Princeton College either as professors or students constituted the party, and the results of their labours were rich and varied beyond their expectation. The objects obtained since the return of the expedition have been subjected to a critical investigation by experts, and No. 1 of the report has just made its appearance in the form of a pamphlet of about 150 pages, with numerous illustrations. As might have been expected, the greater part of the collection consisted of species already collected and for the most part described. But in addition to these a considerable number of novelties rewarded the zeal of the explorers. These are described and many of them figured in the pamphlet referred to. Not the least valuable part of the report, *Harper's Weekly* states, consists of a systematic catalogue of the eocene vertebrates of Wyoming, as compiled from all accessible sources. Of the genera mentioned there are 70 belonging to the mammals, 3 to the birds, 27 to the reptiles and amphibians, and 17 to the fishes. Of species there are 114 of mammals, 7 of birds, 79 of reptiles and amphibians, and 51 of fishes, making a total of 251 species—certainly a very satisfactory showing for a portion of the extinct vertebrate fauna of the west.

As has been reported in the papers, M. Giffard's captive balloon has been sold to Mr. Gooch, of the Princess's Theatre, to be exhibited in London. The sale does not include the winding-up apparatus and machinery, which will remain in the Tuileries grounds, and be utilised for a second captive balloon, which will be built by M. Giffard, during the winter, for next season. The new captive balloon is to be enlarged and improved in details, so that its working capacities may be increased. The increased interest in ballooning has been manifested by the acceptance by the public authorities of the services of the École des Aéronautes Français for executing scientific ascents at Versailles on the day of the great *fête*, and at Paris on the occasion of the inauguration of the Mansion-house of the 19th arrondissement. This École des Aéronautes was established three years ago by a number of persons who escaped from Paris in a balloon during the siege, for the purpose of promoting practice in aeronautics.

NOTHING has occurred at the meeting of the Social Science Congress calling for special notice on our part. Lord Norton, the president, on the basis of doubtful statistics, seemed to think that the teaching of a "jumble of botany, physiology, &c.," in our elementary schools, is the cause of a supposed imperfection in the teaching of other branches in these schools. Certainly, if these subjects are jumbled they will do more harm than good, but as Lord Norton thinks our existing old universities are pretty near perfection, and are sufficient for the wants of the country, it may be doubted whether he has anything like an adequate knowledge of our educational needs. The Hon. G. C. Broderick seems to be pretty much of Lord Norton's opinion with regard to our universities, and virtually admitted that their highest purpose was to be "finishing schools for young gentlemen." On the question of increasing the number of our universities, there were various shades of opinion; all whose opinions are of any weight, however, agreed that increase is necessary, differing only as to the particular form which it should take.

THE following statement with regard to Mr. Edison's recent invention appears in the *Times*:—It appears, from the New York papers, that a company has been started in New York called "The Edison Electric Light Company," with a capital of 300,000 dollars. The object of the company is stated generally to be "the production of heat, light, and power by electricity." The present object, however, is to supply a fund which is to assist Mr. Edison in carrying forward his experiments to a point where he shall give a positive demonstration of the powers of his new inventions. Precisely what these inventions are in all their details of transmission of force and the multiplication of the light derived from electricity, Mr. Edison has not yet told to anybody, fearing that the devices may be patented abroad. The invention, as to the use of electric lights, it is said, will not include the use of carbon points, as ordinarily known in electric lights, but instead the incandescence of a metal simpler and cheaper in every way. Mr. Edison has determined upon the general features of his light, its manner of production, &c.; but in many minor points connected with the distribution of the light for ordinary domestic and business purposes much work has yet to be done. It was at first supposed that 100,000 dollars would be a sufficient experimental fund, but the larger amount was finally determined upon.

THE following is the title of the essay to which the Howard medal of the Statistical Society will be awarded in November, 1879; the essays to be sent in on or before June 30, 1879:—"On the Improvements that have taken place in the Education of Children and Young Persons during the Eighteenth and Nineteenth Centuries." The council have decided to grant the sum of 20*l.* to the writer who may gain the "Howard Medal" in November, 1879.

IN his just published report on the trade, &c., of Kiel, her Majesty's Vice-Consul states that the existence of a large bed of pure salt in the neighbourhood of Segeburg, about thirty-five miles distant, is certain. The bed lies about 144 metres from the surface. Two shafts were sunk, one of which reached 116 metres and the other 85 metres, when underground water filled both up to within 28 metres of the surface. Powerful pumps have been erected, which have emptied the water down to 40 metres, and, although constant pumping shows only a slight decrease, the chief engineer has no doubt of eventual success. In five days the largest pump brought up about 50,000 cubic metres of water, representing a weight of 50,000,000 kilos. The water contains from sixteen to twenty per cent. of salt. The report further states that, in the neighbourhood of Elmshorn, the German authorities are boring for coal. They have reached about 4,000 feet from the surface, but at present have found only red clay, intermixed with particles of salt.

ACCORDING to a Japan contemporary, an attempt, on a more extended scale than that of last year, has been made this season to introduce Japanese black teas into the European market. The samples sent in 1877 were favourably reported on, and it was hoped that an outlet had been found for the continually-increasing quantity of tea produced. These hopes, however, appear to be fading, and even if the existing prejudice against the article can be overcome, our contemporary finds it difficult to see how, with a yearly-increasing export from China and India, leaving Ceylon entirely out of the question, it can ever be made a paying speculation. Much is still hoped from the United States, but the verdict of the American trade upon it has yet to be received. From the same source we learn that the four Japanese gentlemen who were recently in Sydney have gone to Melbourne with the object of furthering an extension of trade with Australia, more especially in the direction of wool and sheep.

THE publication is announced (Ch. Stahl's Verlag in Neu-Ulm) of a "Grosses illustrirtes Kräuterbuch," containing a complete description of all plants and herbs in reference to their uses, their effects and application, their culture, collection, and preservation. It contains instructions for the preparation of all kinds of medicines, juices, syrups, conserves, essences,

powders, &c. The work contains coloured illustrations, and is published in fifteen parts.

ON Saturday evening a most interesting *soirée* took place at the Continental Hotel, Paris. The former pupils of the Central School of Arts and Manufactures received the foreign engineers who had taken part in the Universal Exhibition. At supper M. Dumas was in the chair, and had on his right hand M. Teisserenc de Bort, and on his left Mr. Cunliffe Owen. The hotel was illuminated as usual on such occasions, with the Jablochkoff candles, and a display of electric machines took place. While speaking of the exhibition we may state that the society for delivering lectures to the visitors, to the foundation and progress of which we have several times referred, has been a great success. It has been most heartily patronised by M. Bardoux, the Minister of Public Instruction. The visits to the exhibition by the working men travelling with the funds of the great lottery have been taken advantage of by the lecturing organisation, to give numerous special lectures in connection with the various industries. Not less than forty-four lectures were delivered on Monday week to as many different assemblies selected for the purpose.

A SILVER medal has been awarded at the Paris Exhibition to Mr. Edward Whymper for the engravings which he contributed. This is the highest award made to any British engraver, and is we believe the only silver medal that has been given to any engraver on wood of any country.

A CORRESPONDENT, Mr. Crowther, proposes that instead of using magneto-electric currents in the Bell telephone, induced electric currents be employed. This he proposes to accomplish by using adjacent flat spirals of copper wire, through one of which a current is sent and the other joined to the line wire and attached to a similar receiver at the distant end. We believe a somewhat similar suggestion has already been tried but with no practical benefit.

IT is stated that in the Island of St. Vincent the cocoa-nut palm (*Cocos nucifera*) is now found very sparingly, though at one time the palm was one of the most profitable of all the plants grown in the island. About a quarter of a century ago the palms were visited by a severe blight, from which they have never recovered. It is calculated that about a million cocoa-nut trees are about the present time bearing fruit in the archipelago of Seychelles, and during the next five years quite half as many more will probably be producing fruit.

HIGHLY interesting remains of Roman structures have recently been discovered on the Capersburg, near Friedberg, in the Grand Duchy of Hessen. The excavations are under the direction of Herr G. Dieffenbach, and are being made at the instigation of the Hessian Historical Society of Darmstadt.

THE Leeds Philosophical and Literary Society send us an attractive programme of lectures, mostly scientific, for session 1878-9. We notice that on January 7 Prof. Thorpe is to lecture on the Solar Eclipse of 1878.

SIGNOR A. PONTI, of Milan, has intimated to the Paris Academy that he intends to place at its disposal a sum of 60,000 lire for the foundation of an annual prize, to be distributed as the Academy thinks advisable.

IN Class 15 of the Paris Exhibition, a gold medal was awarded to Messrs. Légé and Co., not Léqé, as misspelt in the "first proof" of the list referred to last week.

ON September 23-26, 1879, the third meeting of the "International Congress of Americanists" will take place at Brussels, under the protectorate of the King of the Belgians and the presidency of the Count of Flanders.

WE have received Part I. of "The Herefordshire Pomona," containing coloured figures and descriptions of the most esteemed kinds of apples and pears, edited by Robert Hogg, LL.D., F.L.S., and published under the auspices of the Woolhope Club (London: Hardwicke and Bogue). The work promises to be one of the most magnificent of its kind, and the coloured illustrations are the finest specimens of chromolithography we have seen; they are by Severeyns, of Brussels. The text, besides descriptions of the various kinds of apples and pears figured, contains a learned and interesting paper by Dr. Bull "On the Early History of the Apple and Pear," and by the same author, a "Life of Thomas Andrew Knight," the eminent horticulturist, of whom there is a fine portrait. This work is in the highest degree creditable to the Woolhope Club. From a prefixed notice we learn that "The Herefordshire Pomona" was originally intended to form a work of local character, as its title indicates, but the great and widespread interest with which the announcement of its publication has been received induces the Woolhope Club to believe that it will be more useful if its scope be made more general. It is intended, therefore, subject to the favour and support it may meet with, to make this Pomona a thoroughly English work. Its local name will still be retained, but it will embrace all apples and pears of established merit cultivated in Great Britain, even though some of the new, or special varieties, may not as yet be grown in Herefordshire. The Second Part of "The Herefordshire Pomona" will be published during the summer of 1879, and will contain, in continuation of the introductory matter, a paper "On Modern Apple Lore;" "A Sketch of the Life of Lord Scudamore," by Dr. Bull, with a full-page portrait; and a paper "On the Cordon System of Growing Pears," by Sir Henry E. C. Scudamore Stanhope, Bart., with a full-page woodcut of the Cordon Wall at Holme Lacy. These will be followed by six coloured plates of such different varieties of fruits as may be procured in perfection during the ensuing season. The Pomona Committee of the Woolhope Club will feel indebted for any assistance that may be rendered to them by supplying information with reference to any new or rare apples and pears of acknowledged merit; their origin, date of production, and description of the fruit. If it be desired to submit them to the judgment of the Committee, with a view to their publication in the work, it will be necessary to send a few well-grown typical specimens of the fruit, that such as are selected may be carefully drawn and coloured from nature, and their descriptions and merits verified. Parcels of fruit should be sent to "The Pomona Committee, Free Library, Hereford."

A NEW mineral spring has recently been discovered at Suhl, in Thuringia, which is particularly rich in chloride of calcium, according to the analysis of Professors Reichardt (Jena) and Sonnenschein (Berlin). Otherwise it resembles the Elizabeth spring of Kreuznach in its composition. The authorities of Suhl intend transforming their charmingly situated little Thuringian town into a fashionable watering-place.

THE twenty-fifth volume of the excellent German scientific series, *Die Naturkräfte*, contains an able treatise on the conservation of energy as the basis of modern physics, by Dr. G. Krebs, of Frankfort-on-the-Main. After some introductory chapters on the changes occurring in nature, on forces, the conversion of finite motions and the meaning of the words work and energy, the author gives a condensed explanation of the sound-oscillations, the conversion of kinetic into caloric energy, and the mechanical equivalent of heat. He then treats of the inner constitution and the three aggregate states of matter, the propagation of heat and light, the identity of the last-named forces, and ends with a chapter on electricity and magnetism, and one on the dispersion of energy. The little book contains numerous woodcuts.

THE additions to the Zoological Society's Gardens during the past week include two Macaque Monkeys (*Macacus cynomolgus*) from India, presented respectively by Capt. E. Waterhouse and Mr. Samuel Thomson; a Common Roe (*Capreolus caprea*) from Greece, presented by Mr. Edward Jones; a Common Jackal (*Canis aureus*) from India, presented by Capt. Easson; a Common Seal (*Phoca vitulina*), European, presented by Messrs. Thompson and Gough; a Bornean Fireback (*Euplocamus nobilis*) from Borneo, presented by Mr. A. Dent; two Mandarin Ducks (*Aix galericulata*) from China, presented by Mr. Edward Trelawny; a Common Marmoset (*Hapala jacchus*), a Tuberculated Lizard (*Iguana tuberculata*), a Teguexin Lizard (*Teius teguexin*), a Merrem's Snake (*Liophis merremi*), a Black-headed Snake (*Homalocranion melanocephalum*), a Plumbeous Snake (*Oxyrhopus plumbeus*), a d'Orbigny's Snake (*Heterdon d'orbignyi*), an Anaconda (*Eunectes murinus*) from South America, purchased; a Collared Fruit Bat (*Cynonycteris collaris*), born in the Gardens.

A NEW GALVANOMETER FOR STRONG CURRENTS

ON the following principle an ordinary tangent galvanometer can be transformed into an instrument suitable for the measurement of strong currents such as produced by powerful magneto- or dynamo-electric machines.

Suppose the circular coil of a tangent galvanometer mounted so as to turn round its horizontal diameter lying in the meridian, and assume the needle to be freely movable *in all directions*, then the effect which the current produces upon the magnet at different inclinations of the coil to the horizontal plane is as follows:—

1st. If the ring is in the vertical position (in the meridian) we have the ordinary form of tangent galvanometer, for which

$$\tan \alpha = \frac{k\,I}{H} \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where α is the deflection of the needle in the horizontal plane, I the strength of the current, k a constant depending upon the dimensions of the coil, and H the horizontal component of the earth's magnetism.

2nd. If the ring is in the vertical position the magnet is only deflected in the plane of the meridian, and the deflection is determined by

$$\tan \beta = \frac{k\,I}{V} \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where β is the deflection and V the vertical component of the earth's magnetism. This would be a tangent galvanometer in which the directive force of the current is opposed by the *vertical* component of the terrestrial magnetism.

By the combination of these two formulæ we obtain

$$\frac{\tan \alpha}{\tan \beta} = \frac{V}{H}.$$

Hence, the tangents of the two deflections are in inverse proportion respectively to the two components of the earth's magnetism.

Since $\frac{V}{H} = \tan i$, where i is the "magnetic dip," this relation may be used to ascertain the "dip" by a method similar to that of Prof. Wilhelm Weber by the inductive action of the earth.

3rd. If the ring is neither in the vertical nor in the horizontal position, but is inclined at any angle ϕ to the horizontal plane, the magnet is simultaneously deflected from the plane of the meridian through an angle α and from the horizontal plane through an angle β. In this case we have to introduce, instead of k in the equations 1 and 2 respectively, $k \sin \phi$ and $k \cos \phi$, whereby they become

$$\tan \alpha = \frac{k\,I}{H} \cdot \sin \phi \quad . \quad . \quad . \quad . \quad . \quad (3)$$

$$\tan \beta = \frac{k\,I}{V} \cdot \cos \phi \quad . \quad . \quad . \quad . \quad . \quad (4)$$

Combining these two equations we obtain the formula

$$\frac{\tan \alpha}{\tan \beta} \cot \phi = \frac{V}{H} = \tan i,$$

ϕ being known and α and β read off, the "dip" may be found by such measurements without *altering* the inclination of the coil.

If the ring is gradually brought from the vertical to the horizontal position, whilst a current I passes through it, the deflection α decreases proportionally from the maximum $\tan \alpha = \frac{k\,I}{H}$ to zero. At the same time the deflection β increases from zero to the maximum $\tan \beta = \frac{k\,I}{V}$.

For practical measurements we need only consider the deflection α in the horizontal plane, and for this reason the needle should work on a vertical axle pivoted at both ends.

With this form of instrument I was enabled to measure very strong currents.

It will be readily understood that a current which would throw the needle to nearly 90° when the ring is vertical, will, when it is suitably inclined, only deflect the needle to that part of the scale (45°) where readings are most accurate.

If the instrument and place of observation remain the same, we can substitute in equation (3) a new constant K for $\frac{k}{H}$ whereby it is simplified to

$$\tan \alpha = K\,I \sin \phi.$$

Further we have for other currents I_1, I_2, &c., at other angles of inclination ϕ_1, ϕ_2, &c.

$$\tan \alpha_1 = K\,I_1 \sin \phi_1,$$
$$\tan \alpha_2 = K\,I_2 \sin \phi_2, \text{ \&c.,}$$

hence,

$$\tan \alpha : \tan \alpha_1 : \tan \alpha_2 \ldots = I \sin \phi : I_1 \sin \phi_1 : I_2 \sin \phi_2, \text{ \&c.,}$$

$$\text{or } I : I_1 : I_2 \ldots = \frac{\tan \alpha}{\sin \phi} : \frac{\tan \alpha_1}{\sin \phi_1} : \frac{\tan \alpha_2}{\sin \phi_2} \ldots$$

By this relation different currents measured at different inclinations of the ring can be compared.

The following separate cases may serve as further illustrations:—

Case 1. Currents of different strength I_1, I_1, $I_2 \ldots$, sent through the coil at the same inclination ϕ, give—

$$\tan \alpha : \tan \alpha_1 : \tan \alpha_2 \ldots = I : I_1 : I_2 \ldots$$

Therefore the law of tangents holds also for the inclined ring.

Case 2. The same current I sent through the ring at different angles of inclination ϕ, ϕ_1, $\phi_2 \ldots$ gives

$$\tan \alpha : \tan \alpha_1 : \tan \alpha_2 \ldots = \sin \phi : \sin \phi_1 : \sin \phi_2 \ldots$$

$$\text{or } \frac{\tan \alpha}{\sin \phi} : \frac{\tan \alpha_1}{\sin \phi_1} : \frac{\tan \alpha_2}{\sin \phi_2} = \ldots = C$$

where C a constant.

The tangents of the deflections are therefore in the same proportion as the sines of the inclinations; or in other words, the tangents of the deflections divided by the sines of the corresponding inclinations give for the same strength of current a constant value.

Case 3. For different currents I, I_1, $I_2 \ldots$ sent through the ring at inclinations ϕ, ϕ_1, $\phi_2 \ldots$ giving the *same* deflection α (say of 45°) we have

$$I : I_1 : I_2 : \ldots = \frac{1}{\sin \phi} : \frac{1}{\sin \phi_1} : \frac{1}{\sin \phi_2} \ldots$$
$$= \operatorname{cosec} \phi : \operatorname{cosec} \phi_1 : \operatorname{cosec} \phi_2 \ldots$$

and the instrument thus used acts as a *cosecant* galvanometer.

The instrument which I used to ascertain the degree of accuracy of the method described consisted of a wooden ring of 30 cm. diameter, wound for some experiments with a few convolutions of wire, and for other experiments with a copper band. This ring, in the centre of which a small magnetic needle was placed, could be turned round its horizontal diameter, and its inclination read off on a graduated quadrant. To adjust the instrument the ring is approximately placed in the horizontal position; a current is then sent through the coil, and if the needle is deflected from the meridian, the inclination of the ring must be carefully altered until no deflection occurs. In this position the quadrant is fixed so that its zero point coincides with the index attached to the coil, and the instrument is now ready for use.

The following tables contain records of some of the experiments made with this instrument:—

Table I. gives the results obtained with a coil of seven convolutions of wire of ·074 Siemens' units resistance, and with a needle turning on a point. One Bunsen's cell was used, and the strength of current varied by the introduction of resistances. For each current-strength readings were taken at inclinations of the ring, the sines of which are proportional to the even integers 2 to 10.

TABLE I.

Resistance in circuit.	1 S. U.			2 S. U.			3 S. U.			4 S. U.			5 S. U.			6 S. U.			1 S. U.		
sin φ	α	tan α	tan α / sin φ	α	tan α	tan α / sin φ	α	tan α	tan α / sin φ	α	tan α	tan α / sin φ	α	tan α	tan α / sin φ	α	tan α	tan α / sin φ	α	tan α	tan α / sin φ
1	59°·5	1·698	1·70	45°·9	1·032	1·03	35°·47	·7125	·712	28°·7	·5475	·547	23°·9	·4431	·443	20°·35	·3709	·371	61°·87	1·871	1·87
·8	56·2	1·494	1·87	39·5	·8243	1·03	29·7	·5704	·713	23·55	·4358	·545	19·42	·3526	·441	16·57	·2975	·372	56·32	1·500	1·87
·6	48·25	1·120	1·87	31·72	·6181	1·03	23·15	·4280	·713	18·0	·3249	·541	14·85	·2651	·442	12·4	·2199	·367	48·3	1·122	1·87
·4	36·67	·7446	1·86	22·17	·4061	1·01	15·85	·2839	·710	12·22	·2166	·541	10·0	·1763	·441	8·25	·1450	·362	36·87	·7450	1·86
·2	20·27	·3693	1·85	11·70	·2071	1·03	8·02	·1409	·704	6·05	·1060	·541	5·0	·0875	·437	4·17	·0729	·364	20·27	·3693	1·85

It will be seen from this table that the value of $\frac{\tan \alpha}{\sin \phi}$ is as nearly a constant as can be expected from this kind of measurement.

Table II. gives the results obtained with the same coil and needle as before, but with the current unaltered in strength (one Bunsen's cell suitably reduced by the insertion of a small resistance). The order of experiments is shown by the arrows.

TABLE II.

	I.			II.			III.		
sin φ	α	tan α	tan α / sin φ	α	tan α	tan α / sin φ	α	tan α	tan α / sin φ
·1	10°·4	·1835	1·83	10°·25	·1808	1·81	10°·25	·1808	1·81
·2	20·2	·3679	1·84	20·15	·3669	1·83	20·15	·3669	1·83
·3	29·1	·5566	1·85	29·05	·5554	1·85	29·00	·5543	1·85
·4	36·95	·7522	1·88	36·75	·7467	1·87	36·65	·7440	1·86
·5	42·95	·9309	1·86	42·85	·9276	1·85	42·95	·9309	1·86
·6	48·15	1·116	1·86	48·15	1·116	1·86	48·1	1·115	1·86
·7	52·45	1·301	1·86	52·5	1·303	1·86	52·45	1·301	1·86
·8	56·2	1·494	1·87	56·05	1·485	1·86	56·2	1·494	1·87
·9	59·1	1·671	1·86	59·1	1·671	1·86	59·1	1·671	1·86
1	61·9	1·873	1·87	61·8	1·865	1·86	61·7	1·857	1·86

The smaller value of the constant at the top of each column is doubtless due to slight mechanical inaccuracies in this *experimental* instrument.

Table III. gives the results obtained with the copper band provided with stout leading wires and with the needle fixed to an axle. The resistance of the copper band, including the leads, was only ·001 Siemens' units. Three Bunsen's cells connected parallel were used without additional resistance.

TABLE III.

sin φ	α	tan α	tan α / sin φ
1	66°·9	2·344	2·34
·8	61·6	1·849	2·31
·6	54·4	1·397	2·33
·5	49·4	1·167	2·33
·4	42·9	·9293	2·32
·2	25·1	·4684	2·34
·1	13·2	·2345	2·34
1	66·8	2·333	2·33

In this table the greatest difference from the mean of the constant (2·33) is not more than ·8 per cent.

The foregoing experiments having shown that the measurements with this instrument are, as to exactness, in no way inferior to those with the usual form of tangent galvanometer, I also employed it for the currents of dynamo-electric machines.

The first of these experiments was made with a Siemens' machine of smallest size. Besides the galvanometer an electric lamp was in circuit. The strength of the current and the intensity of the light was measured, and at the same time the number of revolutions of the armature counted. The results are given in the next table.

TABLE IV.

Revolutions of armature.	Deflection α (sin φ = ·2)	tan α	tan α / sin φ	Intensity of light in standard candles.
860	38°	·7813	3·906	3200
851	37	·7536	3·768	2750

The second experiment was made in the same way as the preceding, but with a Siemens' machine of medium size. The results are given in

TABLE V.

Revolutions of armature.	Deflection α (sin φ = ·1)	tan α	tan α / sin φ	Intensity of light in standard candles.
660	28°	·5317	5·317	4100
696	33·5	·6619	6·619	6400
692	33	·6494	6·494	7700[1]

The deflections in the experiments with the small machine were taken with the galvanometer ring at an inclination, the sine of which is equal to ·2; with the larger machine the sine of the angle of inclination was equal to ·1. The column headed $\frac{\tan \alpha}{\sin \phi}$ gives in both cases the tangent of the deflection which would be obtained with the vertical ring.

By these two tables I intend merely to show that even such powerful currents which give a light of thousands of candles produce readable deflections with a suitable inclination of the ring.

I may add that the method I have just described to reduce the action of a coil upon the magnetic needle by turning it round a horizontal axis will scarcely be limited to the tangent galvanometer, but that this method very likely can be used with other galvanometric apparatus for many purposes where shunts are not desirable.

EUGEN OBACH

GULF-WEED[2]

FROM the time of Columbus to the present day the gulf-weed growing in the "Sargasso Sea" has attracted the attention and excited the interest of all voyagers who have crossed the

[1] With other carbon points than used before.

[2] Om de under Korvetten *Josephines* expedition, sistliden sommar (1869), insamlade Algerne. Af J. G. Agardh, Öfversigt af *Kongl. Vetenskaps-Akademiens Förhandlingar*, 1870, No. 4, Stockholm.

Enumeration of Algæ collected by Mr. Moseley, Naturalist to H.M.S. *Challenger*, at St. Thomas, Bermudas, Coast of Brazil, Cape de Verde Islands, St. Paul's Rocks, Fernando de Noronha, Tristan d'Acunha, Inaccessible Island, Simon's Bay, Seal Island, Marion Island, Heard Island, and Kerguelen's Island. By Prof. G. Dickie, M.D., F.L.S., Linnean Society's *Journal*—Botany, vol. xiv.

Atlantic. Almost every one who meets with the plant on his first trip across the ocean brings home a small bottle filled with the weed, which is shown to admiring friends, and then put away and forgotten. But the plant is much too interesting to be thus thrown aside without examination. Let the bottle be carefully broken so that the gulf-weed may be removed without injury; then place it in salt water—in sea-water if it can be got—in a large vessel of clear glass, where it will have room to expand, and then will be seen its beauty and grace of form, no small addition to which are the pretty and minute species of Campanularia, Plumularia, and Sertularia which twine around its branches, while other parts of the plant are covered by Polyzoa with a delicate lace-work as hard as stone.

But it is not only on account of its beauty that gulf-weed is deserving of attention; there is a history attached to it which renders it one of the most interesting vegetable productions of the sea. I propose, therefore, to give a summary of all that is yet known respecting the habits and history of this plant.

Whether the Sargasso Sea was known to the ancients is doubtful. Two descriptions, by ancient writers, of a kind of "Mar de Sargasso," near the coast of Africa, have been transmitted to us.

Humboldt ("Aspects of Nature," pp. 46, 47, Bohn's edition) has shown that both these descriptions refer to localities too near the coast of Africa to be applicable to the Sargasso Sea. The first description is from a work which Humboldt says for a long time bore incorrectly the name of Aristotle; it is as follows:—"Phœnician mariners came in a four days' voyage from Gades to a place where the sea was found covered with rushes and sea-weed. The sea-weed is uncovered at ebb, and overflowed at flood tide." There are no rushes mixed with the sea-weed in the Sargasso Sea, neither is the weed covered or uncovered by the water according to the state of the tide. The second description, from the Periplus, which has been ascribed to Scylax, of Caryanda, is thus quoted by Humboldt: "The sea beyond Cerne [1] ceases to be navigable in consequence of its great shallowness, its muddiness, and its sea-grass. The sea-grass lies a span thick, and it is pointed at its upper extremity, so that it pricks." Now the Sargasso bank is in deep water, which is not muddy, and no sea-grass (which inhabits shallow water) ever grows on it. The Sargasso bank is much more than a span in thickness, and the upper extremity of the plant is not sharp enough to prick.

Columbus and his followers called the floating sea-weed "sargazo," a term which botanists have modified into SARGASSUM, as the generic name, adding, as the specific name, *bacciferum*, alluding to the great number of berry-like air-vesicles which assist to buoy up the plant when in the water. This alga is also sometimes called "*Fucus natans*," on account of its being found floating on the sea, and not attached to the shore or to rocks, while to sailors it is known by the name of "gulf-weed," and that part of the sea where it is met with in greatest abundance is called the Sargasso Sea.

The Sargasso Sea is situated in the North Atlantic, between 22° and 36° N. latitude, in the comparatively quiet space which is bounded on the south by the great Equatorial current, on the west and north by the Gulf Stream, and on the east by the Guinea current, which flows southwards. Humboldt [2] states that there are two principal banks, the larger of which lies a little to the west of Fayal, one of the Azores; the smaller near to the Bahamas. The situation, however, of the weed-banks varies in different seasons, according to the prevalent winds. Maury states [3] that "an area equal in extent to the Mississippi Valley, is so thickly matted over with gulf-weed, that the speed of vessels passing through it is often much retarded. When the companions of Columbus saw it, they thought it marked the limits of navigation, and became alarmed. To the eye, at a little distance, it seems substantial enough to walk upon. Patches of the weed are generally to be seen floating along the outer edge of the Gulf Stream. The sea-weed always 'tails' to a steady or constant wind, so that it serves the mariner as a sort of marine anemometer, telling him whether the wind, as he finds it, has been blowing for some time, or whether it has just shifted, and which way. Columbus first found this weedy sea on his voyage of discovery; there it has remained to this day, moving up and down, and changing its position, like the calms of Cancer, according to the seasons, the storms, and the winds. Exact observation as to its limits and their range, extending back for fifty years, assure us that its mean position has not been altered since that time." Dr. Harvey says [1] that he had made the voyage across the Atlantic four times, and only once found gulf-weed in any quantity. It then occurred in ridges of great length from ten to twenty yards in breadth. These ridges are separated by water, which flows between them like rivers or lakes.[2]

One fact respecting the gulf-weed, hitherto unnoticed by botanists, has not escaped the keen eye of the sailor, namely, that the plants rise a few inches *above* the water, the upper branches not being immersed; hence it is readily observed from a distance. It is this power of supporting the upper branches out of the water in an erect position—a very unusual power in sea-weeds—that enables the gulf-weed to "tail" to the wind, as before mentioned.

Another peculiarity attending the floating weed is that no other marine plant has ever been found growing on it or with it; small zoophytes and polyzoa are, however, often attached to it. Although its vegetation is limited to one species, the Sargasso Sea is a great resort of animal life, and it lies within the northern limits of the wanderings of the Sperm whale. The Right whale sometimes crosses its northern boundary, where the water is cooler.

A third peculiarity affecting the *floating* gulf-weed is that it has neither root nor fruit; never in the Atlantic, or in other localities where it is met with, has it ever been found in fruit. [3] On this point I shall have more to say hereafter.

The genus Sargassum is the most highly organised of the Melanospermeæ, or olive-coloured sea-weeds. It possesses root, stem, branches, leaves, air-vesicles, and distinct organs of fructification. The species are very numerous. Agardh, in his "Species, Genera et Ordines Algarum," part I, published in 1848, describes 126 species, which number has since been greatly increased. These species are classified into three sections and twelve tribes. Gulf-weed belongs to the highest section, namely, *Eu-sargassum*, or Sargassum proper, and to the twelfth tribe Cymosæ.

The genus Sargassum inhabits the tropical and sub-tropical seas of both hemispheres, extending on each side of the equator to about the 45° parallel of latitude, gradually increasing in number of species towards the line. With the exception of *S. bacciferum* (gulf-weed) and *S. vulgare*, which is also sometimes called "*Fucus natans*," [4] the species are very local. Thus some grow on the coasts of Australia only, and the species of North Australia differ from those of the south. A remarkable group of Sargasso inhabits the coasts of Japan, where the plants grow in the warm waters of "the Black Current," the Pacific analogue of the Gulf-Stream of the Atlantic; other species are found in the China Seas, many in the Indian Ocean, and these are generally distinct from those of the Red Sea. The section *Cymosæ*, to which Gulf-weed belongs, inhabit chiefly the Atlantic and Indian Oceans and the Australian coasts.

With these extremely local habits, and permanently distinct species, it seems difficult to reconcile the errant habits and the fixed forms of *S. vulgare*, and the plant which has given its name to the Sargasso Sea; both species are found in most of the warmer seas in both hemispheres.[5] Slight deviations sometimes occur in these plants, but they are clearly traceable to local causes. Thus, in the Sargasso Sea, the plants have often shorter leaves, the branches are contracted, and the bristles of the air-vessels broken off; whereas, specimens from Sydney, New South Wales, have longer leaves, the air-vessels have very long bristles, which frequently form narrow leaves, and the habit of the plants is more lax and straggling.

S. vulgare produces fruit in all the localities where it is found; but, with regard to *S. bacciferum*, it has already been

[1] "The Phœnician station for merchant vessels (Gaulea); or, according to Gosselin, the small estuary of Fedallah, on the north-west coast of Mauritiania."—Humboldt. *Ib.*

[2] See "Views of Nature," Bohn's translation, p. 48.

[3] "Physical Geography of the Sea," p. 28, tenth edition.

[1] "Manual of British Algæ." Introduction, p. xxi., xxii., second edition.

[2] Besides the Sargasso-bank, in the Atlantic, Maury mentions several other accumulations of sea-weed known to mariners as "Sargassos." The immense banks of weeds in the South Pacific, through which ships pass in going to the Australian colonies, consist principally of a pelagic form of Macrocystis; of the composition of the other weed-banks little is known.

[3] "Om de under Korvetten *Josephines* expedition, sistlidensommar (1869) insamlade Algerne." Af J. G. Agardh, Öfversigt af *Kongl. Vetenskaps-Akademiens Förhandlingar*, 1870, No. 4, Stockholm.

[4] It is much to be regretted that this term "*Fucus natans*" should not have been limited by authors to *S. bacciforum*.

[5] *S. bacciferum* is found in the Atlantic, between 22° and 58°, being sometimes carried on the Gulf Stream as far as the Orkney Islands. It is also found on the coasts of Spain and Portugal, and in the Mediterranean Sea, its presence in these localities also being due to the Gulf Stream. It is likewise met with in the Indian Ocean, the Pacific, on the coasts of Australia, and New Zealand.

observed that the specimens found *floating* in all the seas are always barren. The question then arises, how is the floating weed propagated? To this question Dr. Harvey[1] replies:—"It seems to me that the old frond, which is exceedingly brittle, is broken by accident, and the branches, continuing to live, push out young shoots from all sides. Many minute pieces that I have examined were as vigorous as those of a large size; but they were certainly not seedlings, and appeared to me to be broken branches, all having a piece of the old frond from which the young shoots spring. As the plant increases in size it takes something of a globular form, from the branches issuing in all directions, as from a centre."[2] The specimens brought to this country are merely the uppermost branches, the whole plant often attaining a diameter of three or four feet. The upper branches are of a light olive colour, the lower more or less brown; the lowest parts of all wither and decay, and finally drop away.

With regard to the origin of the plant there has been much controversy among algologists. On this point Harvey observes:—[3] "Nothing has yet been discovered; for, though species of Sargassum abound along the shores of tropical countries, none exactly corresponds with *S. bacciferum*. That the ancestors of the present banks have migrated from some fixed station is probable, but further than probability we can say nothing." Prof. Agardh, however, thought he had found the parent stock in a plant which grows on the banks of Newfoundland; and in his "Species, Genera, et Ordines Algarum," vol. i., published in 1848—a year before the second edition of Harvey's "Manual" appeared—thus writes of it:—"Natans semper sterilis, nec in pratis Atlanticis fructigera, Fructiferam et affixam e mari Americanum alluente habeo." In his before-mentioned notes on the algæ collected by the *Josephine* expedition, after referring to the passage just quoted, he says:—"I have received from the banks in the neighbourhood of Newfoundland specimens with root and fruit, which, although somewhat different in form, I nevertheless did not scruple to consider as belonging to the same species" (*S. bacciferum*); and he adds that "from no facts yet come to my knowledge have I had reason to change the opinion I had expressed in 1840 at the meeting of naturalists at Copenhagen, that the floating form originates from the Banks of Newfoundland, and perhaps grows on similar localities on the east coast of America, when it is provided with root and fruit." Since the publication of these observations, additional information relative to the fruit of *S. bacciferum* has been obtained. Specimens "covered with fructification" have been found by Mr. Moseley,[4] the naturalist of the *Challenger* expedition, in Harrington Sound, Bermudas, which islands lie in the very heart of the Sargasso Sea.

The fact, therefore, that *S. bacciferum* bears fruit *when attached to the land* in the Sargasso Sea and the Gulf Stream, may be considered as established.

With regard to the comparative antiquity of the Bermudas and Newfoundland plants, it must be observed that they both occur within the influence of the Gulf Stream. It is not likely that the Sargassum could make its way from the northern locality *against* the current of the stream; hence it seems most probable that it vegetated first at Bermudas, and thence emigrated, with the stream, northwards.

Other facts in connection with the two islands favour this view. Sir Wyville Thomson says[5]:—"Bermudas is practically an 'atoll,' or annular coral reef. . . . . What the basis on which the Bermudas reef rests may be, we have no means of telling; in fact, its having the form of an atoll precludes the possibility of our doing so. There seems to be little doubt, from Darwin's beautiful generalisation, which has been fully indorsed by Dana and other competent observers, that the atoll form is due to the entire disappearance by subsidence of the island round which the reef was originally formed. The abruptness and isolation of this peak, which runs up to a solitary cone about equal to that of Mont Blanc, is certainly unusual; probably the most reasonable hypothesis may be that the kernel is a volcanic mountain comparable in character with Pico in the Azores or the Peak of Teneriffe." It has been also stated[1] that "in the process of excavations carried out in order to accommodate the great floating dock, a bed of peat, with stumps of cedar trees in a vertical position, was found at a depth of forty-five feet below low-water mark, covered by beds of rock. Most conclusive proof is thus afforded that the Bermudas have changed their level, and sunk since they have been covered with a similar vegetation to their present one." These statements afford considerable evidence of the antiquity of the Bermudas. Now as to the comparative age of the Newfoundland Banks. Lieut. Payer[2] has observed respecting them:—"It is an established fact that ice-bergs and ice-fields laden with the *débris* and rubbish of Arctic lands, deposit these burdens round the outer edge of the Frozen Ocean, and to this process, partially at least, the origin of the Newfoundland banks is to be ascribed." It would seem from these observations that these banks have been formed since the glacial period, hence that they are more recent than the Bermudas. On the above grounds, therefore, it is but reasonable to conclude that the Bermudas plant has superior claims to that of the Newfoundland banks to be considered as the parent-stock of the Atlantic plant.

That the parent-stock originated in the Atlantic, and then found its way into the Indian and Pacific Oceans is, at least, doubtful, especially as a form of the plant called var. Capillifolium,[3] bearing fruit abundantly, has been found at Mauritius by Col. Pike. Thus, then, in two oceans out of the three, *S. bacciferum* has been obtained in fruit, but with sufficient differences in the forms to constitute varieties; whether the Bermudas plant differs from the others I have not been able to ascertain.

Prof. Mertens (see his work on the algæ collected by the Prussian expedition to Eastern Asia) supposed that *Sarg. dentifolium*, of the Red Sea, was the parent-stock of the floating Sargassum, on the ground that the same species of polyzoa were found on both plants. Not a very strong argument, especially as a remarkable difference exists in the structure of the two plants. *S. dentifolium* has a serrated midrib, while that of *S. bacciferum* is plain. This difference was observed by Prof. Agardh, who, in addition, expressed his belief[4] that *S. dentifolium*, an inhabitant of a tropical sea, could not make, or survive, the voyage round the Cape of Good Hope. It must then have had other means of making the voyage, as the plant has actually been found at Key West, Florida, by Dr. E. Palmer. Dr. Farlow,[5] who mentions this fact, observes: "It is not stated whether it was floating or attached. Dr. Palmer's specimens are more luxurious than those of the Red Sea, but the serrated midrib seems sufficiently characteristic to warrant us in supposing that the species is the same." Here then we have another instance of a tropical plant being found in both hemispheres. How did it get there?

According to the present conformation of the land in tropical and sub-tropical seas, there is no water-passage open in those latitudes between the Atlantic and Pacific, or between the Atlantic and Indian Oceans. In the north and also in the south, round "the Horn," the extreme cold would be fatal to all the species of Sargassum; and although certain species of this genus grow at the Cape of Good Hope, *S. bacciferum* has never yet been brought home from thence.[6] The presence of the plant in the warmer parts of the three great oceans where means of communication are now impossible, except between the Indian and Pacific Oceans, owing to the conformation of the land and climatal obstructions, remains still to be accounted for. This can only be done by referring to a time when Mexico was submerged, and the isthmus not yet in existence, and there was thus communication between the tropical Atlantic and Pacific; when open passages also existed between the Indian Ocean and the warmer parts of the Atlantic, and between the Indian and Pacific Oceans. Tropical and sub-tropical algæ would thus be free to pass from sea to sea. That these plants did so is the opinion of botanists and other scientific persons. In support of this opinion may be mentioned the occurrence of a considerable

1 "Manual of British Marine Algæ," second edition.

2 The floating gulf-weed is not the only plant which can maintain life in vigour, and propagate itself without producing fruit. *Gigartina Teedii*, and *Griffithsia secundiflora*, have vegetated many years on rocks in the vicinity of Plymouth, without producing fruit. They are both natives of the Mediterranean, where they yield fruit freely.

3 "Manual," *l. c.*

4 See extract from Mr. Moseley's letters communicated by Dr. Hooker, Linnean Society's *Journal*, vol. xiv. It is much to be regretted that Mr. Moseley has sent home no specimens of the Bermudas plant.

5 "Letters from the *Challenger*" in *Good Words* for February, 1876, pp. 98–100.

1 *Athenæum* for December 1, 1877. Review of voyage of the *Challenger*.

2 "New Lands Within the Arctic Circle," vol. i. pp. 41, 42.

3 I am indebted to Prof. Agardh for a fragment of a specimen of *S. bacciferum* from the banks of Newfoundland, and to Dr. Oliver, of Kew, for a fruitful specimen of Col. Pike's plant.

4 Agardh, *l. c.*

5 "List of Marine Algæ of the United States, with Notes of New and imperfectly-known Species." By G. W. Farlow. From *Proceedings* of the American Academy of Arts and Sciences, presented March 9, 1875.

6 Agardh, *l. c.*

number of sea-fishes inhabiting both sides of the Isthmus, which Dr. Günther has shown to be absolutely identical. To these may be added several species of mollusca. With reference to the mollusca Mr. Wallace[1] observes, "The long-continued separation of North and South America by one or more arms of the sea . . . is further rendered necessary by the molluscan fauna of the Pacific shores of tropical America, which is much more closely allied to that of the Caribbean sea, and even of West Africa, than to that of the Pacific Islands. The families of many of the genera are the same, and a certain proportion of very closely allied or identical species, shows that the union of the two oceans continued late into tertiary times. If fishes and mollusca could thus pass from ocean to ocean, there is no doubt that algæ could also pass. Besides *Sarg. bacciferum*, *S. vulgare*, and *S. dentifolium*, the following species, among others, are common to both hemispheres, namely, *Chnoospora fastigiata*, *Hydroclathrus cancellatus*, *Digenia simplex*, *Acanthophora Thierri*, and others too numerous to mention. It is further thought that such plants are among the oldest forms of algæ, and that algæ are among the oldest productions of the vegetable world. Geologists are of opinion that the tropical passage between the Atlantic and Pacific was open during the tertiary and cretaceous epochs. How long a time has elapsed since this period is another question that remains to be answered. This can only be done approximately. "From the Devonian period, or earlier," says Prof. Huxley,[2] "to the present day, the four great oceans, Atlantic, Pacific, Indian, and Antarctic, may have occupied their present positions, and only the coasts and channels of communication have undergone an incessant alteration." Mr. Croll, who, in his most interesting work "Climate and Time," brings astronomical science to bear upon the elucidation of geological problems, states that "the great ocean basins are probably of immense antiquity; that the great depressions of the Atlantic, Pacific, and Indian Oceans may be as old as the Laurentian period for any thing which geology shows to the contrary." He also remarks—"all our main continents and islands not only existed during the glacial period as they do now; the very contour of the surface was much the same as at the present day."[3] The migration of *S. bacciferum* from one ocean to the other must then have taken place previously to the commencement of the glacial period—the most recent glacial period, I mean—for Mr. Croll thinks that there were other glacial periods with intermediate warm periods during the tertiary epoch. He gives astronomical reasons[4] for saying that the commencement of the most recent glacial epoch cannot date back more than 240,000 years. As, then, there has been no material alteration of the surface of the land since that period, and our plant now inhabits and can live in the warmer parts only of the three great oceans, the barriers to their intercommunication being now closed as before-mentioned, it follows that this alga must be at least of greater antiquity than the glacial period, 240,000 years ago. How many thousands or hundreds of thousands additional years may be added to its age, it is impossible to say. Perhaps on some slab of rock from the depths of the earth the astonished and admiring botanist may yet recognise a fossil plant of the wandering Sargassum.[5]

If the presence of a great number of species in a limited area is suggestive that the parent-stock may have originated in that locality, then it is probable that the primary habitat of the genus Sargassum may have been in the Indian and adjacent oceans, since it is on the southern coasts of Asia, the islands in the Indian Ocean, and round the coasts of Australia and New Zealand (Mr. Wallace's "Oriental and Australian Regions," also his "Ethiopian Regions," Nos. 1 and 4), that the greater number of species of Sargassum are found. No less than forty species are known to inhabit the seas around Australia and New Zealand.

There are fair grounds for the opinion that many of the tropical algæ of the three great oceans are probably among the oldest forms of this class of plants—*S. bacciferum* and its congener *S. vulgare*, also *S. dentifolium*, and other algæ before mentioned may, therefore, be "survivals," still existing in health and vigour, of the marine vegetation of a very remote period, as ancient, at least, as the miocene[6] epoch, when the appearance and configuration of the country was, in all probability, different from what it is at the present day.

One cannot but look with wonder and admiration mixed with somewhat of veneration, on the wandering Sargussum, still in vigorous existence, which has survived so many changes of climate affecting different parts of the earth's surface; so much variation in the boundaries of the sea-shores; before which the rise and fall of empires, and the very existence of man, form almost inappreciable items in its life-history.

In order to show the great numerical increase in the species of Sargassum in the warmer seas, I shall conclude this article with a tabular view of their geographical distribution. In the division of regions I have followed Mr. Wallace. It is to be observed that as some species range through more than one region, they are consequently entered in each region, and thus the aggregate of species appears to be greater than it really is.

	1. Palæarctic.				2. Ethiopian.				3. Oriental.				4. Australian.				5. Neotropical.				6. Nearctic.			
	North Europe.	Mediterranean.	Siberia.	Manchuria or Japan.	East Africa.	West Africa.	South Africa.	Madagascar.	Hindostan.	Ceylon.	Indo-China.	Malayan.	Austro-Malayan.	Australia.	Polynesia.	New Zealand.	Chili.	Brazil.	Mexico.	Antilles or West Indies.	California.	Rocky Mountains.	East United States.	Canada.
Number of regions.	1	2	3	4	1	2	3	4	1	2	3	4	1	2	3	4	1	2	3	4	1	2	3	4
Number of species.	–	6	–	13	11	2	7	9	22	4	16	20	1	36	13	8	2	6	1	10	–	–	1	–

MARY P. MERRIFIELD

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

THE Master of Trinity College, Cambridge, has forwarded to the Vice-Chancellor certain statutes made by the College affecting the University, and in doing so intimates that the Colleges consider that the provisions of the Universities of Oxford and Cambridge Act, 1877, do not bind them to postpone their final adoption until one month after they have been communicated to the Council of the Senate. The statutes have reference to, among others, the Trinity Professorship of Physiology. They provide that any person hereafter elected to the Professorship shall be entitled to a Fellowship at Trinity unless he is Master or Fellow of some other College. The Trinity Professor of Physiology is to receive an annual stipend of 500*l*., in addition to the emoluments of a Fellowship. The new statutes also provide that there shall be paid by the College to the University an annual sum calculated upon the amount of the distributable income of the College, which is particularly defined. Such annual sums to commence from the time the statutes come into operation, and shall be in the first instance equal to 5 per cent. of the distributable income, to be increased to 7½ per cent. when the statutes have been ten years in operation, and to 10 per cent. when they have been fifteen years in operation. The provisions of these statutes with respect to the Trinity Professorship of Physiology shall take effect from and after the appointment of the first Trinity Professor of Physiology, under the provisions of a statute or statutes to be made with the consent of the College for the establishment of the said professorship.

THE French Government proposes to do an act of justice in raising the stipends of professors in science and medicine to the same amount as in the case of law and letters, 15,000 francs. Dr. Simplice, who writes on the subject in the *Union Médicale*, points out how unequally professors of pure science, as botany and chemistry, are rewarded as compared with, say, clinical professors, who can add enormously to their income by private

[1] "Geographical Distribution of Animals," vol. ii., p. 58.

[2] Address to the Geological Society, reported in the *Journal* of the Geological Society, May, 1870.

[3] "Climate and Time," p. 9.

[4] *l.c.*, p. 355.

[5] Among the fossil algæ known to botanists are some specimens of Sargassum.

[6] Mr. J. S. Gardner, F.G.S., has recently expressed his opinion that the American continents became united during the eocene period (see NATURE, vol. xviii. p. 192). If this be the fact, a great addition must be made to the antiquity of these plants.

practice. Dr. Simplice proposes that a maximum salary should be given to the former, and a minimum to the latter. This is a subject that calls for consideration here as well as in France.

SOCIETIES AND ACADEMIES

Boston, U.S.A.

American Academy of Arts and Sciences, October 9.—Charles Francis Adams, president, in the chair.—Prof. W. A. Rogers read a paper on the limits of accuracy in measurements with the microscope, in which he stated that Prof. E. N. Morley and himself independently measured 195 spaces having a magnitude of about $\frac{1}{500}$ of an inch, each space, however, varying slightly from this value. The measures were made with a glass eye-piece micrometer, a Beck's spider line micrometer, and with a screw attached to the sub-stage of the microscope. After the results were prepared for the press they were for the first time compared. It was found that the average difference between the results for a single space was 32 millionths of an inch, and the greatest difference was 12 millionths. There were only four cases in which the difference amounted to one hundred thousandth of an inch.—In a second paper Prof. Rogers gave a determination of the errors of the sub-divisions of a copy of the British yard known as Bronze No. 11 and of the metre of the U.S. Bureau of Weights and Measures and the production therefrom of an inch, which is one thirty-sixth of this particular yard, and of a centimetre, which is one-hundredth part of this particular metre, the temperature in both cases being 67° F.—Prof. John Trowbridge described a new electro-dynamometer for measuring strong electric currents without shunting them. The principle consists in cooling the two points in the revolving axis of the instrument where the current enters and leaves by means of a current of water and in using mercury pivots. The instrument can measure from a fraction of a Weber up to six hundred Webers. It is especially adapted for the measurement of currents produced by dynamo-electric machines.

Göttingen

Royal Academy of Sciences, July 6.—The following papers were read:—On the solution of equations of the fifth degree, by L. Kiepert.—On *Duboisia myoporoides*, by W. Marmé.—Herr Hänselmann of Brunswick presented to the Academy the certified copies of eighty-two letters written by or addressed to Gauss.

August 3.—On the feldspar in the basalt of the Hohe Hagne, near Göttingen, and its relation to the feldspar from the Monte Gibele on the Island of Pantellaria.

Paris

Academy of Sciences, October 22.—M. Daubree in the chair.—The President announced the deaths of M. Bienaymé, free Academician, and M. Leymerie, correspondent in mineralogy.—M. Des Cloizeaux read a note on the works of the late M. Delafosse.—The following papers were read:—On the thermal formation of the combinations of oxide of carbon with other elements, by M. Berthelot. The heats liberated by chlorinised and sulphurised combinations of carbonic oxide are less than those of hydrogen: which corresponds to their less stability.—Various thermal determinations, by M. Berthelot. This relates to boric acid, chromate of soda, biacetate of soda, iodide of silicium, and earthy phosphates.—On the vision of colours, and especially on the influence exercised on vision of coloured objects in circular motion when observed comparatively with similar bodies in repose; extract from a small work by M. Chevreul. He supports the view of dyers and artists that there are three simple colours, viz., red, yellow, and blue. He finds, also, that by a motion having a maximum of 160 to 120 turns, and a minimum of sixty per minute, one may generate the complementary of every colour submitted to this movement.—On ytterbine, a new earth contained in gadolinite, by M. Marignac. The name is given to recall its presence in the mineral of Ytterby, also its similarity to yttria, on one hand, by absence of colour, and to erbine, on the other, by the elevation of its equivalent (say 131); to both, by the *ensemble* of its properties. The atomic weight deduced for ytterbium would be 115 or 172·5, according as its oxide receives the formula YbO or Yb_2O_3.—On the dentition of Smilodons, by M. Gervais.—The disease of chestnuts in the Cevennes, by M. Planchon. The gradual death of the stem and branches is caused by an alteration of the roots, which become softened with a sort of moist gangrene, giving out an exudation of tannic nature. These phenomena are caused by the mycelium of a fungus. M. Planchon thinks untimely irrigations are the chief *occasional* cause of the evil.—Processes for determining the butter in milk; reply to note by M. Adam, by M. Marchand.—Complementary observations on formulæ relating to perforation of iron armour plates, by M. Martin de Brettes.—M. Ponti, of Milan, announced his intention to place 60,000 Italian pounds at the disposal of the Academy, for founding an annual prize.—Observations on a communication from M. Amigues on flattening of the planet Mars, by M. Hennedy. He confirms M. Amigues' calculations from independent researches.—Remarks on M. Levy's note regarding a universal law relative to dilatation of bodies, by M. Boltzmann. He finds in fluid water a contradiction of M. Levy's theorem (about the pressure of an inclosed heated body increasing rigorously with the temperature).—Note relative to the theorem on the composition of accelerations of any order, by M. Lignine.—On the rectification of the ovals of Descartes, by M. Darboux.—Second note on the resolution in whole numbers of the equation (1) $ax^4 + by^4 = cz^2$, by M. Desboves.—On the Mosandrum of Prof. L. Smith, by M. Delafontaine. He rejects Prof. Smith's claim of priority, and affirms the identity of mosandric acid and terbine.—Researches on sulphates, by M. Etard. This relates to rose ferroso-ferric sulphates, mixed proto-sulphates, and simple or double sulphates, more or less hydrated.—On the nerve terminations in striated muscle, by M. Tschiriew. He has found, in several species, new forms of nerve-termination, intermediate between the motor termination (as met with in the frog), and the terminal plates. The most simple is in the tortoise; nerve-fibres deprived of myeline, ramify without anastomosing, and terminate, on the muscular bundles, by rods, sometimes smooth, but oftener moniliform, or surrounded by grains. There are generally several such terminations on one muscular fibre.—On the albuminoid matters of organs and of the spleen in particular, by M. Picard. Globuline exists in the spleen independently of the presence of blood.—On the hydrophorous reservoirs of Dypsacus, by M. Barthelemy. He rejects M. Boyer's view that the water present is produced by secretion (principally) and by dew, and attributes the liquid entirely to rain.—Apparatus for experimenting on the action of electricity on living plants, by M. Celi. This consists of a bell-jar, into which electricity is admitted by a metallic collector, connected with an insulated metallic vessel at 2 m. height, from which streams a thin vein of water.—Influence of salicylic and thymic acid, and some essences on germination, by M. Haeckel. While phenic acid suspends germination, salicylic acid (even in very small quantity) stops it altogether.

CONTENTS